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OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

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THE following communications were read:—

I. Extract of a Letter from Mr. Maclear to Capt. Beaufort, accompanied by the original Circle and Transit Observations of Halley's Comet since January.

The number of meridian observations thus obtained is upwards of thirty. The reductions will be forwarded in a short time; the delay arising from Mr. Maclear being employed in observing the stars of the Brisbane list, in aid of Sir John Herschel, who, he states, is now occupied in reducing his Catalogue of Southern Nebulæ.

II. A Catalogue of the Right Ascensions of 1318 Stars, Observed at Blackheath. By Mr. Wrottesley.

These papers consist of a catalogue in Right Ascension of 1318 stars (those of the 6th and 7th magnitude inclusive, contained in the Astronomical Society's catalogue), with an explanatory introduction and notes; and also of the original observations, and the reduced mean places, from which the catalogue is formed. As this is the first contribution by a private observer to a more accurate knowledge of the places of the fixed stars, which has been made *in consequence of the Society's Catalogue*, and *according to its directions*; and as the deductions are of great value and importance; a distinct report has been made by a Committee, and adopted by the Council, in order that due credit may be given to the labours of Mr. Wrottesley, and of his assistant, Mr. Hartnup.

The observations were made with a transit telescope, by Mr. Thomas Jones, of 3¾ inches clear aperture, 62 inches focal length, and 27 inches horizontal axis. The power used was 142. The position of the instrument was ascertained, when practicable, by consecutive transits of *Polaris* above and below pole; and in other circumstances, by single transits of *Polaris* or δ *Ursæ Minoris*. This was checked, in some degree, by a close mark seen through a fixed lens; which, however, discharged a better purpose in determining the error of collimation whenever the instrument was reversed. This was done every month. The level was always applied (of course in reversed positions) once every night, and often

twice, viz. at the beginning and end of a series of observations. At first, corrections for the instrumental errors were computed and applied, but the amount was found to be so small, that Mr. Wrottesley subsequently preferred taking the clock error for the stars of his catalogue from the standard or standards, which, having nearly the same declination, were affected by the same instrumental errors. Thus the instrumental errors, which were always noted and kept low, were, as to sense, eliminated.

The bases of this catalogue are, the fundamental catalogue of Bessel for the *mean* places of the standard stars (omitting some not suited to Mr. Wrottesley's purposes, and substituting his own place of Fomalhaut for the erroneous place of Bessel); and for the corrections, the constants and precessions of the Astronomical Society's catalogue, and the values of A, B, C, D, contained in the *Nautical Almanac*. The number of observations of the stars in the catalogue is 12007, or rather more than 9 observations for each star, on an average.

The partial mean places appear to be as close to each other, or nearly so, as those of the Greenwich observations. It follows, from Mr. Wrottesley's method of deducing the clock error for each star from one, two, or more standards which are near it, that the *accidental* errors are greater than he would have had, if he had used all the standard stars for his clock error; but, as it is, a greatest difference from the mean of ten or twelve observations exceeding $0^s,3$ is not common, which leaves only a small uncertainty upon the final result; especially since this difference, being the sum of two independent errors of observation, viz. that of the standard determining star and that of the catalogue star, may be expected to have little effect in the mean of several observations. Indeed, there can be no doubt, that this catalogue of Mr. Wrottesley's may be used for all purposes, with nearly, if not altogether, the same confidence as the fundamental catalogue from which it is derived.

Of the means taken to insure accuracy in the reductions, Mr. Wrottesley has given a very satisfactory account, and that these means have been effectual, he has stated the following proof. In Mr. Wrottesley's catalogue are 138 stars observed and reduced by Professor Airy in the Cambridge observations. "Of these, 46 agree within $0^s,05$; 89 within $0^s,10$; 115 within $0^s,15$; 131 within $0^s,20$; and only one differs so much as $0^s,30$." We have a further and independent proof of the correctness of this catalogue in the Remarks (appended by Mr. Baily) on the differences of $1^s,0$ and upwards, between the Catalogue of Mr. Wrottesley and that of the Astronomical Society. It is known to the Society, that our catalogue has been pretty nearly reobserved by Mr. Taylor at Madras, and very well observed, though, unfortunately, very few copies have found their way into the hands of English astronomers. There are 29 stars having such differences, of which 24 have been observed by Mr. Taylor, and in every instance his result confirms that of Mr. Wrottesley. Whether these anomalies

are to be attributed to errors in the catalogues of Bradley or Piazzi (for Mr. Baily has examined the computations by which our catalogue is *deduced* from theirs), or whether there is some irregular motion in the stars themselves, time will shew. It is from such undertakings as this of Mr. Wrottesley, and so executed, that we must expect to fix the state of the heavens at certain epochs, and so prepare the data for future speculation and future discovery. For such inquiries, Mr. Wrottesley's present of the original transit books, to which the partial mean places serves as a complete index, will be of great and permanent value.

There are several remarks which deserve, and doubtless will receive, the attention of practical astronomers, but which would be here out of place. We must not, however, omit mentioning that the whole work has been performed, to use Mr. Wrottesley's words, "without any foreign aid," by himself, and, under his superintendence, by his assistant, Mr. John Hartnup; upon whom, indeed, in consequence of Mr. Wrottesley's frequent and continued absence from home, the task of observing and computing chiefly fell, and who has executed this task with extraordinary zeal, skill, and fidelity.

III. On the Projection of Maps and Charts. By Professor Littrow.

Three kinds of projections are chiefly had recourse to in the construction of maps,—the *orthographic*, the *stereographic*, and the *central*. Each has its peculiar advantages and disadvantages in particular cases; but as the stereographic possesses two very remarkable properties,—1st, that all circles of the sphere are projected either into circles or straight lines; and, 2nd, that the angles made by two projected circles, are equal to the angles made by those circles on the sphere,—it has been most frequently employed in the construction of planispheres.

All projections of the sphere on a plane have this disadvantage, that they distort the map in a greater or less degree. With a view to lessen the defect, various modifications of the stereographic projection have been proposed, such as that of La Hire, who supposed the eye to be situated out of the sphere, and at a distance from its convex surface equal to the sine of 45°. But, though by this means the distances are less altered on the map, the great advantages of the stereographic projection are lost. The circles of the sphere become in general ellipses or hyperbolas, and the projections of the meridians and parallels no longer intersect at right angles.

Though it is convenient to employ the principles of perspective in representing spherical surfaces on a plane, there is nothing in the nature of the thing itself which renders such a method of proceeding necessary. Let the meridians and parallels be represented on the map by any lines whatever, the representation will still be accurate if the different points are laid down so as to have the same relations to those lines as the points represented have to

the corresponding circles of latitude and longitude. The construction of a map in this way is evidently an indeterminate problem; but it is rendered determinate by subjecting the representation to certain conditions which may have no reference to perspective. Mercator's chart is an example. The condition to be fulfilled in this chart is, that the rhumbs shall be straight lines, and make with each other the same angles as on the sphere. This condition determines the well-known property of the chart, namely, that the degrees of longitude are all equal, while the degrees of latitude are inversely as the sines of the polar distances.

It is stated by Lagrange, that Lambert was the first who considered the theory of the construction of maps under this general view. The problem which Lambert undertook, was to determine the lines representing the meridians and parallels, so that all the angles on the map should be equal to the corresponding angles on the sphere. This problem is extremely important, because it follows, from the equality of the angles, that any infinitely small portion of the surface of the sphere, and its representation on the map, are similar figures, and, consequently, that small parts represented differ from the original only in respect of magnitude. Lambert's solution is given in Vol. III. of his *Beytrage*, &c. The same problem was undertaken by Euler in the *Petersburg Memoirs* for 1777; but neither of these illustrious geometers prosecuted the subject further than to shew, that the known properties of the stereographic projection, and Mercator's chart, were comprehended in their general solution.

The *Berlin Transactions* for 1779 contain two memoirs, by Lagrange, on the same subject. In the first, he gave a new solution of the problem of Lambert, of which he shewed the stereographic projection to be only a particular case; and, in the second, undertook the more difficult question of determining the form of the arbitrary functions which enter into the general solution, so that the lines representing the meridians and parallels shall be of a given nature.

Gauss also, in answer to a prize question proposed by the Royal Society of Sciences of Stockholm, has given a memoir on this subject, which was published in No. 3 of *Schumacher's Abhandlungen*, and a translation of it in the *Philosophical Magazine* for August and September, 1828. Instead of confining himself, however, to the particular case of the representation of a spherical surface on a plane, he undertook the solution of the general problem, namely, to represent the parts of any given surface on any other given surface, so that the differential elements of the first surface shall be similar to their representations on the second. Having determined, by a very beautiful analysis, the differential expressions from which the co-ordinates of any point of the map are found, in terms of the co-ordinates of the corresponding point of the primitive surface, he applies his general solution to the particular cases in which it is required to represent a plane surface on a plane, the surface of a cone on a plane, of a sphere and

ellipsoid on a plane, and, lastly, of an ellipsoid of revolution on the surface of a sphere.

The object of Professor Littrow is to deduce the general properties of the three principal projections, which, though they differ from each other in no other respect than in the situation of the eye and perspective plane, with regard to the principal circles of the sphere, have hitherto been always treated as distinct and independent problems. The manner in which he accomplishes this may be easily described.

He supposes the eye to be situated at any distance above the sphere, and the plane of projection to be inclined at any angle to the straight line drawn from the eye to the centre. Having assumed x and y as the rectangular co-ordinates of any point on the map, he chooses for the axis of x the intersection of the plane passing through the eye, the centre of the sphere, and the pole with the plane of projection, and finds expressions for x and y in terms of the latitude and longitude of the corresponding point on the sphere. Two equations are thus obtained. Eliminating from these two equations the latitude, the resulting equation is that of the projection of the meridian passing through the projected point; and, by eliminating the longitude, he gets the equation of the parallel circles. He then applies this general solution to each of the three projections successively, finds in each case the equations of the projections of the meridians and parallels, and shews the cases in which they are circles, or straight lines, or any of the three conic sections. He then considers the projection of the spheroid of revolution, and gives the formulæ which express the latitude of any point on the sphere in terms of the latitude of the corresponding point on the spheroid.

The concluding part of the memoir contains some general remarks on the solutions of Gauss and Lagrange; and a demonstration that Gauss's formulæ are comprehended in those of Lagrange, the latter being only particular values of the former. The analysis is extremely neat, symmetrical, and perspicuous; and the subject, as before remarked, one of much interest.

IV. On the construction of the Hour-lines of Sun Dials. By Professor Littrow.

The remarks which Professor Littrow makes on this subject, and his method of treating it, are precisely analogous to those employed in the preceding paper. Indeed, the two subjects are closely related, as the whole theory of Dialling may be deduced from the properties of the central projection of the sphere. He begins by remarking that, though the subject has been very frequently treated of by others, the problem has not hitherto been resolved with all that generality of which it is susceptible, and which it deserves in so eminent a degree; that what regards the length of the shadows has never been satisfactorily handled; and that with regard to dials on curve surfaces, nothing complete, in point of theory, has

been done to the present time. His object, in the present paper, is to supply the deficiency.

Suppose twelve planes all to intersect in the same straight line, and to make equal angles with each other, and that one of these planes coincides with the meridian of any plane, and the line in which they intersect is parallel to the earth's axis of rotation. Then, if we also suppose the sun's diurnal motion to be uniform during a day, each of the twelve planes will successively coincide with the plane of a horary circle, and the shadow of the line of intersection will fall on the line formed by the intersection of that plane with the surface of the dial. The first question, therefore, resolves itself into this:—Trace the lines in which a given surface is intersected by planes given in position.

The second question is, that of finding the length of the shadow. It is assumed that the sun's declination does not vary sensibly during a day. The shadow of a fixed point in space, therefore, generates the surface of a cone, of which the fixed point is the vertex, and the sun's diurnal circle the base; and the path of the shadow is the intersection of the opposite cone with the surface on which the dial is traced. If, then, the dial is on a plane, the path is one of the conic sections. In any case, the question is to determine the intersection of a cone with a given surface.

M. Littrow gives the general equations of the hour-lines, and of the path of the shadow, first, when the dial is on a plane and having any inclination with respect to the equation and horizon; and then, by assigning particular values to the arbitrary quantities, he deduces the ordinary expressions for horizontal, vertical, equatorial, &c. dials. He then considers the case when the dial is in a curve surface, and points out a method of combining the equation of the hour planes and that of the given surface, so as to eliminate one of the variables, and obtain the equations of the intersections; and applies his method to the particular cases in which the dial is described on a cylinder, and on a cone with a circular base.

V. On the formulæ for the computation of precession. By M. Mattheus Valente do Conto, Director of the Observatory at Lisbon.

The object of this memoir is to correct an error into which Delambre had fallen respecting the value to be assigned to the variation of the obliquity of the ecliptic in the formulæ for the annual precession of a star in right ascension and declination.

In Vol. III. of his Astronomy (1814), Chapter 32, No. 19, Delambre, after having given the rigorous formulæ for the precession of a star in right ascension and declination (in which $d\omega$ does not appear), obtains the formulæ given by La Place (*Méc. Céleste* Vol. II. p. 350) without demonstration, viz.

$$d\text{Æ} = d\,\text{L}\cos\omega - 0''{\cdot}20168 + d\,\text{L}\sin\omega\sin\text{Æ}\tan\text{D} - d\omega\cos\text{Æ}\tan\text{D}$$
$$d\,\text{D} = d\,\text{L}\sin\omega\cos\text{Æ} + d\omega\sin\text{Æ}$$

in which $d\,L$ is the precession, D the declination of a star, ω the obliquity.

"Astronomers," Delambre adds, "have made use of these expressions, with the exception of the terms $d\,Æ = -d\,\omega \cos Æ \tan D$, and $d\,D = -d\,\omega \sin Æ$, which have always been neglected. The latter cannot exceed 0″·5 a year, but it would amount to 50″ in a century; the former would amount to 16″·5 a-year in the case of the pole star. These terms, therefore, would seem to deserve all the attention of astronomers, but they have believed that the motion of the ecliptic did not at all affect the declinations, nor even the right ascensions, which would be diminished only by the annual motion 0″·202 of the equinoctial points."

In deducing the above expressions, Delambre supposes the latitude constant, that is, that the plane of the ecliptic is fixed, and that of the equator alone movable; therefore the variation $d\,\omega$ of the obliquity must be produced by a small motion of rotation of the equator round the equinoctial points. Thus, it appears that the variation ($d\,\omega$) here employed is not that of the true ecliptic upon the fixed ecliptic, and, consequently, cannot be, as Delambre supposes, 0″·5.

M. do Conto deduces the same differential expressions from the spherical triangle, formed by the star and the poles of the ecliptic and equator. In this he supposes the latitude constant, and the equator movable, so that the point of intersection with the ecliptic, and the inclination, vary simultaneously.

It appears, from the theory of the luni-solar precession, that the *inclination* of the movable equator upon the fixed ecliptic would be constant; but that its displacement by the planetary action would produce, besides the annual diminution of the obliquity, a small increase in the above-mentioned constant inclination. It is this small increase which ought to be taken as the value of $d\,\omega$, and is $.00000984\,t^2$ nearly. In the thirty years which follow 1750, this would amount to 0″·00886, which, multiplied by the tangent of the declination 89° 42′·7, or 200, would give 1″·77; but the declinations of all the stars of which any use is made being much less than this, the error of neglecting this quantity will, in general, be very small.

VI. Notice of a forthcoming Work on the Measures of Double Stars. By Professor Struve.

Professor Struve hoped, in June last, to complete his extended catalogue of double stars, containing all the observations made since those already published in his well-known *Catalogus Stellarum Duplicium*, Dorpat, 1827 (or from 1824 to 1836). This last-mentioned catalogue contained 3112 stars; from which the professor has, for various reasons assigned, excluded 490, and has added 64 remarkable new ones of greater distance than 32″, and 21 of less distance. The number of stars, therefore, is 2707. The measures were made with a wire-micrometer applied to the large refractor, with a power varying from 320 to 1000, and mostly

in an illuminated field. Calling each night's observations of one star a measure, the number of measures is about 11,000, or, on an average, four to each star.

Professor Struve has divided these stars into eight classes (Sir W. Herschel used four), as follows :—

W. Herschel.		Struve.	
I.	0″ to 4″ of distance.	I.	0″ to 1″ of distance.
		II.	1 — 2 ——
		III.	2 — 4 ——
II.	4 — 8 ——	IV.	4 — 8 ——
		V.	8 — 12 ——
III.	8 — 16 ——	VI.	12 — 16 ——
		VII.	16 — 24 ——
IV.	16 — 32 ——	VIII.	24 — 32 ——

Each class is further divided into two divisions, *lucidæ* and *reliquæ;* the former containing those in which the companion is not of less than the eighth magnitude. The principle of this division is, Professor Struve states, that the catalogue is very nearly complete with respect to the *lucidæ*, on which, therefore, certain theoretical conjectures may be formed, relative to the numbers of double stars in different orders of magnitude. The introduction, besides all the matters explanatory of the present and emendatory of the former, catalogue, which might certainly have been looked for from Professor Struve, will contain conclusions concerning the nature of double stars, from their distribution among the orders of magnitude, their brightness, proper and relative motions, &c. Professor Struve has added a specimen of the catalogue, and several interesting conclusions, of which our limits will only enable us here to notice the very rapid motion of 42 *Comæ Berenices* (130° in six years) the reduction of the period of λ *Ophiuchi* to less than 40 years, and the close approach to their nearest distance of γ *Coronæ* and ω *Leonis*. The latter system, between Sir W. Herschel and Professor Struve, has now been watched from the greatest to the least apparent distance.

VII. Stars observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the months of June—October, 1836.

Day.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
June 20	Moon 1 L. ...	...	...	10 57 5,78
23	α Virginis	...	...	13 16 34,81
	Moon 1 L. ...	...	...	13 22 40,63
24	Moon 1 L. ...	...	14 14 56,44	...
	ι^1 Libræ	...	15 2 55,04	...
25	α^2 Libræ	...	14 41 50,75	14 41 50,41
	ι^1 Libræ	...	15 2 55,08	15 2 54,87
	Moon 1 L. ...	...	15 12 8,38	15 12 40,97

Day.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
	π Libræ	...	15 34 53,48	15 34 53,18
	θ Libræ	...	15 44 31,79	15 44 31,53
28	μ¹ Sagittarii ..	...	18 4 0,07	18 3 59,97
	δ Sagittarii ...	...	18 10 32,51	...
	Moon 1 L. ...	...	18 34 7,42*	18 34 46,51
	Moon 2 L. ...	...	18 36 45,75	18 37 24,55
	τ Sagittarii ...	...	18 56 44,47	18 56 44,16
	h² Sagittarii ..	...	19 26 45,74	19 26 45,97
29	τ Sagittarii ...	18 56 44,43	18 56 44,59	...
	h² Sagittarii ..	19 26 45,65	19 26 45,70	...
	Moon 2 L. ...	19 47 46,84	19 47 45,59	...
	σ Capricorni ..	20 9 57,68	20 9 57,81	...
July 1	Moon 2 L. ...	21 56 37,38	...	...
	σ Aquarii	22 21 59,53	...	...
	τ² Aquarii ...	22 40 55,78	...	...
18	Moon 1 L. ...	...	...	11 29 24,40
27	59 Sagittarii ..	19 46 55,28	19 46 55,45	...
	e Sagittarii ...	19 52 36,69	19 52 36,74	...
	Moon 1 L. ...	20 18 8,08	20 18 7,01	...
	Moon 2 L. ...	20 20 41,18	20 20 40,25*	...
	ψ Capricorni	20 36 25,22	20 36 25,35	...
	π Capricorni ..	...	20 55 6,38	...
Aug. 2	ι Piscium	1 32 55,63	...	...
	Moon 2 L. ...	1 48 23,42	...	...
4	Moon 2 L. ...	...	...	3 25 49,78
20	Moon 1 L. ...	...	...	16 25 41,01
21	Moon 1 L. ...	17 29 51,27	17 29 50,10	...
	μ¹ Sagittarii ..	18 3 59,69	18 3 59,77	...
	λ Sagittarii ...	18 17 53,48	...	...
22	μ¹ Sagittarii ..	...	...	18 3 59,60
	λ Sagittarii ...	...	...	18 17 53,04
	Moon 1 L. ...	...	...	18 38 22,14
	h² Sagittarii ..	...	...	19 26 45,93
24	σ Capricorni	...	...	20 9 57,96
	ψ Capricorni	20 36 25,43	20 36 25,51	20 36 25,04
	Moon 1 L. ...	20 52 10,17	20 52 9,04	20 52 44,04
	ζ Capricorni ..	21 17 20,67	21 17 20,71	21 17 20,47
	γ Capricorni ..	21 31 2,64	21 31 2,89	21 31 2,44
25	ζ Capricorni ..	21 17 20,52	21 17 20,67	...
	γ Capricorni ..	21 31 2,52	21 31 2,76	...
	Moon 1 L. ...	21 53 58,39	21 53 57,35	...
27	φ Aquarii	...	23 5 52,40	23 5 52,08
	ψ³ Aquarii ...	...	23 10 28,47	23 10 27,88
	Moon 2 L. ...	...	23 46 47,64	23 47 15,68
28	ι Piscium	23 56 59,08	...	...
	θ Piscium	0 17 2,39	...	...
	Moon 2 L. ...	0 37 23,75	...	...
	ι Piscium	0 54 28,71	...	...
	ε Piscium	0 59 58,08	...	...

* Correction applied for defect of illumination on June 28, $0^s,01$, on July 27, $0^s,06$.

Day.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
29	ε Piscium	0 59 57,92	...	...
	Moon 2 L. ...	1 26 31,37	...	...
	ξ Piscium	1 45 6,31	...	...
30	ξ Piscium	1 45 6,58	...	...
	ξ¹ Ceti	2 4 21,22	...	...
	Moon 2 L. ...	2 15 21,75	...	...
	π Arietis	2 40 11,16	...	...
31	π Arietis	2 40 11,08	...	...
	Moon 2 L. ...	3 4 51,70	...	...
	η Tauri	3 37 46,78	...	...
Sept. 2	τ Tauri	...	...	4 32 26,12
	Moon 2 L. ...	...	...	4 48 44,12
	β Tauri	...	...	5 15 57,16
	ζ Tauri	...	...	5 27 52,08
16	Moon 1 L. ...	...	...	16 6 52,75
17	Moon 1 L. ..	...	...	17 9 34,97
19	σ Sagittarii ...	...	...	18 45 7,33
	ζ Sagittarii....	...	...	18 52 12,01
	Moon 1 L. ...	...	...	19 22 11,04
	59 Sagittarii..	...	...	19 46 54,84
20	59 Sagittarii..	...	...	19 46 54,75
	Moon 1 L. ...	...	...	20 27 21,68
21	κ Capricorni ..	...	...	20 55 5,94
	ζ Capricorni ..	...	...	21 17 20,02
	Moon 1 L. ...	...	...	21 29 5,35
	θ Aquarii......	...	...	22 8 12,85
23	λ Aquarii	...	22 45 59,65	...
	φ Aquarii	...	23 5 52,52	...
	Moon 1 L. ...	...	23 20 13,63	...
	p Piscium	...	23 50 19,58	...
	ω Piscium	...	23 56 59,38	...
24	p Piscium	23 50 19,38	...	23 50 19,03
	ω Piscium	23 56 59,19	...	23 56 58,94
	Moon 1 L. ...	0 11 30,14	0 11 29,36*	0 11 56,87
	Moon 2 L. ...	0 13 41,43	0 13 40,96	...
	δ Piscium	0 40 13,44	0 40 13,72	...
	m Ceti	...	...	0 44 40,14
Oct. 2	ω¹ Geminorum	...	...	6 52 26,72
	Moon 2 L. ...	...	...	7 10 38,95
	α¹ Geminorum	...	...	7 24 8,85
	α² Geminorum	...	...	7 24 9,15
	β Geminorum	...	...	7 35 17,95
3	β Geminorum	7 35 18,03	7 35 18,07	...
	Moon 2 L. ...	8 4 35,02	8 4 34,30	...
	γ Cancri	...	8 33 48,59	...
4	γ Cancri	...	8 33 48,59	8 33 48,54
	Moon 2 L. ...	...	8 57 29,74	8 57 57,86
5	Moon 2 L. ...	...	...	9 49 6,77
	γ Leonis	...	...	10 10 56,26

* Correction applied for defect of illumination on September 24, $0^s,02$.

Day.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
16	Moon 1 L. ...	...	...	19 2 22,44
	h^2 Sagittarii ..	...	...	19 26 45,35
	c Sagittarii ...	...	...	19 52 36,06
18	Moon 1 L. ...	...	21 9 1,56	...
	γ Capricorni ..	...	21 31 2,31	...
	δ Capricorni ..	...	21 38 1,52	...
19	γ Capricorni ..	21 31 2,28	...	21 31 2,00
	δ Capricorni ..	21 38 1,43	...	21 38 1,10
	Moon 1 L. ...	22 6 42,95	...	22 7 12,27
	τ^2 Aquarii ...	22 40 56,90	...	22 40 56,62
	δ Aquarii	22 45 59,14	...	22 45 58,73
21	ψ^3 Aquarii ...	23 10 28,42	23 10 28,52	...
	κ Piscium ...	23 39 33,43	23 39 33,70	...
	Moon 1 L. ...	23 51 25,94	23 51 25,38	23 51 52,49
	ι Piscium	...	0 17 2,90	0 17 2,68
22	ι Piscium	...	0 17 2,92	...
	Moon 1 L. ...	0 40 36,50	0 40 35,78	...
	ε Piscium	0 59 58,64	0 59 58,57	...
23	ε Piscium	0 59 58,61	0 59 58,56	0 59 58,54
	Moon 1 L. ...	1 29 13,03	1 29 12,03	1 29 38,82
	ξ^1 Ceti	...	...	2 4 22,03
24	ξ^1 Ceti	...	...	2 4 22,00
	Moon 2 L. ...	...	...	2 20 54,38
	π Arietis	...	...	2 40 12,22
	ι Arietis	...	...	2 49 54,00
25	π Arietis	...	...	2 40 12,29
	ι Arietis	...	...	2 49 53,91
	Moon 2 L. ...	...	...	3 11 17,96
	η Tauri	...	...	3 37 47,98
	A^1 Tauri	...	...	3 55 3,75

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *December* 9, 1836. No. 2.

Mr. Baily announced to the Meeting, that His Royal Highness the Duke of Sussex, President of the Royal Society, had, on the 30th ultimo, delivered up to this Society, for their sole use, benefit, and advantage, the room on the mezzanine floor, immediately over their present meeting-room. On which it was

Resolved unanimously,

That the thanks of the Society be given to His Royal Highness, for his kind attention to the wants and wishes of this Society, in making over to them the room above mentioned.

The following communications were then read, viz.:—

I. On a remarkable phenomenon that occurs in total and annular eclipses of the sun. By Mr. Baily, Vice-President of the Society.

The author states, that, having read of certain singular appearances that are recorded as having taken place in annular eclipses of the sun, at the moment that the whole disc of the moon enters on the disc of the sun, he was desirous of witnessing those phenomena at the solar eclipse of May 15th last; and, finding that the central path of the moon's shadow would pass nearly in a straight line from Ayr, on the western coast of Scotland, to Alnwick on the eastern coast of Northumberland, he proceeded to Scotland for that express purpose. Having computed, from the elements given in the *Nautical Almanac*, that the central line of the moon's umbra would pass directly over, or very near to, Jedburgh in Roxburghshire; and having ascertained that this place was within eight or ten miles of Makerston, the seat of Lieut.-General Sir Thomas Macdougal Brisbane, Bart., who has a well-furnished observatory there, and from whom he was sure of obtaining the correct time for his chronometers, he resolved to make that town his head-quarters. Mr. Baily took with him a 3½ feet refracting telescope by Dolland, 2¾ inches aperture, and magnifying about 40 times; a 20 inch Rochon's prismatic telescope, for measuring the distances between the borders of the sun and moon; two thermometers; a burning-glass; and four pocket chronometers.

Mr. Baily took up his station at the house of Mr. Veitch, a very ingenious gentleman, residing at Inch Bonney, about half-a-mile to the southward of the town of Jedburgh; who afforded him every facility for making the observations. The morning of the

15th of May is described as being remarkably fine and clear; not a cloud to be seen in any part of the heavens during the whole time of the eclipse. The times of the beginning and ending of the eclipse, and of the formation and dissolution of the annulus, have already been given in the preceding volume of these monthly abstracts, page 200. But Mr. Baily does not lay much stress on this part of his observations—more especially those connected with the annulus—since his attention was taken up with other more interesting phenomena. He says he was in expectation of meeting with something extraordinary at the formation of the annulus; but imagined that it would be only momentary, and consequently that it would not interrupt the noting of the time of its occurrence. In this, however, he was deceived, as the following facts will shew. For, when the cusps of the sun were about 40° asunder, a row of lucid points, like a string of beads, irregular in size, and distance from each other, *suddenly* formed round that part of the circumference of the moon that was about to enter on the sun's disc. This he intended to note as the correct time of the formation of the annulus, expecting every moment to see the thread of light completed round the moon; and attributing this serrated appearance of the moon's limb (as others had done before him) to the lunar mountains; although the remaining portion of the moon's circumference was perfectly smooth and circular, as seen through his telescope. He was somewhat surprised, however, to find that these luminous points, as well as the dark intervening spaces, increased in magnitude; some of the contiguous ones appearing to run into each other like drops of water. Finally, as the moon pursued her course, these dark intervening spaces were stretched out into long, black, thick, parallel lines, joining the limbs of the sun and moon: when, all at once, they *suddenly* gave way, and left the circumferences of the sun and moon in those points, as in all the rest, apparently smooth and circular, and the moon perceptibly advanced on the face of the sun. This moment of time Mr. Baily considers to be that which most persons would assume and record as the formation of the annulus; but he adduces strong reasons afterwards to shew that the true formation of the annulus was some seconds prior to that event.

After the formation of the annulus, as thus described, the moon preserved her circular outline during its progress across the sun's disc, till her opposite limb again approached the border of the sun, and the annulus was about to be dissolved. When, all at once (the limb of the moon being at some distance from the edge of the sun), a number of long, black, thick, parallel lines, exactly similar in appearance to the former ones above mentioned, *suddenly darted forward*, and joined the two limbs as before: and the same phenomena were repeated, but in an inverse order. For, as those dark lines got shorter, the intervening bright parts assumed a more circular shape, and at length terminated in a fine, curved line of bright beads (as at the commencement), till they ultimately vanished, and the annulus consequently became wholly dissolved. This re-

markable and singular phenomenon was also observed by Mr. Veitch, and also by Sir Thomas Brisbane, as well as by Mr. Henderson at Edinburgh; with some slight differences, however, in the detail. The appearance of the dark lines, or threads, was likewise noticed by Mr. Bell, at Alnwick, who sent an account of the same to the Philosophical and Literary Society at Newcastle. Mr. Baily describes them to have been as plain, as distinct, and as well-defined, as the open fingers of the human hand held up to the light; and that there could not have been any doubt as to their form and existence, since they were seen by different observers, at different places, and with different telescopes. Several drawings accompanied the paper, shewing the appearances at various stages of the annulus.

The number of these dark lines, or threads, Mr. Baily considers to have been about eight; in which opinion he was confirmed by Mr. Veitch. Sir Thomas Brisbane, however, thinks there were not more than six; whilst Mr. Bell, who noticed four at the dissolution of the annulus, says that there were only two at its formation. On these and other points Mr. Baily thinks there is ample room for a diversity of opinion, since the observer is taken, as it were, by surprise, and the phenomenon itself, during the short period of its existence, is constantly varying in some minute particulars.

Mr. Baily remarks, that the diminution of light was not so great during the existence of the annulus as was generally expected, being little more than might be caused by a temporary cloud passing over the sun: the light, however, was of a peculiar kind, somewhat resembling that produced by the sun shining through a morning mist. The thermometer in the shade fell only about three or four degrees. The birds in the hedges were in full song during the whole time of the eclipse. About twenty minutes before the formation of the annulus, Venus was seen with the naked eye; and a few minutes afterwards it was impossible to fire gunpowder, with the concentrated rays of the sun, through a lens of three inches in diameter. The same lens, likewise, had no effect on the ball of a thermometer during the existence of the annulus.

For the cause of the remarkable optical deception above described, Mr. Baily does not attempt to account; but he confesses his surprise that the phenomenon has not (with one single exception, which will be presently alluded to) been noticed, or recorded, on former occasions, since it must have been seen by every person who watched for the formation and dissolution of the annulus; and although detached portions of the phenomenon have been recorded by different observers, as seen at different places (various extracts from whose accounts are quoted by Mr. Baily), yet it is impossible from those descriptions to form an accurate idea of the whole, or to trace the origin, progress, and termination of this phenomenon, which is certainly one of the most remarkable in astronomy. M. Van Swinden is the only person who has placed on record the observation of the dark lines, or threads, which connect the borders of the sun and moon, at the formation and dissolution of the

annulus. His account is inserted in the first volume of the *Memoirs* of this Society (page 146), accompanied with drawings, which coincide almost exactly with those given by Mr. Baily. In nearly all the accounts by other observers, the description of the phenomenon is restricted to the very commencement of the annulus, or to the formation of the string of luminous points which on a sudden are seen to surround that portion of the moon's limb about to enter on the sun's disc; and no notice whatever is taken of the continuation of the phenomenon, or of the stretching out of the dark spaces into parallel lines, as above mentioned: nor of their *sudden* rupture and *disappearance*, which is by far the most remarkable part of the phenomenon.

How far any of these appearances may favour the hypothesis of a lunar atmosphere, or whether, indeed, they could be accounted for on such an assumption, the author does not stop to discuss; but, with a view to assist those who are disposed to enter on such an inquiry, he has adduced various accounts of a similar phenomenon to that of the dark lines, observed at the transits of Venus over the sun in 1761 and 1769. For, on each of those occasions, many astronomers remarked, that, at the interior contact of Venus with the sun (both on its ingress and egress), there was formed a sort of dark ligament between the border of Venus and the border of the sun, which appeared like a protuberance from the planet, and which continued several seconds. This dark ligament is represented, in the drawings which accompany the several memoirs on this subject, to be much thicker, and to continue longer, than the dark lines in a solar eclipse; so that the planet, during the progress of the ingress and egress, assumes a shape which has been variously described as resembling a pear, a Florence flask, and a skittle. But all the accounts agree in stating the *sudden* rupture of the ligament; and that immediately thereon the planet assumes its usual circular shape. Nothing of this kind, however, has been noticed at the transits of Mercury over the sun: on the contrary, we have the direct evidence of Sir William Herschel (who examined Mercury, with that special object in view, at the transit of November 9, 1802), that he could not discern any thing out of the usual course. He expressly states, that the whole disc of Mercury was as sharply defined as possible; and that there was no kind of distortion of the limb, either at its ingress or egress: the appearance of the planet remained well defined from first to last.

Mr. Baily considers, and adduces certain facts to shew, that the circular edge of the moon is always distorted at those points which are in contact (or nearly so) with the sun's circumference; and which have occasionally given rise to the supposition of lunar mountains in high relief. He thence infers, that all measures of the moon's diameter, when passing over the sun's disc, must be taken with great caution, and with due attention to the proximity of the part measured to the edge of the sun's disc (where alone the distortion seems to take place), otherwise errors and discordances

will occur. Those prodigious lunar elevations and depressions, so frequently described in solar eclipses, are seldom or never seen, except at the commencement or termination of the eclipse, or in places near the solar cusps: that is, in those points only which are near the edge of the sun; every other portion of the moon's circumference being comparatively smooth and circular. If this notion be correct, it would seem that the measurement of the solar cusps during an eclipse may be liable also to discordances from this very cause.

Mr. Baily concludes by expressing a hope, that, at the total eclipse of the sun in 1842, and the annular one in 1847 (both of which will be central in Europe), the attention of astronomers will be directed more particularly to this subject, both as to its existence and its cause; and that such a regular system of observations in various places will be adopted, as may best tend to elucidate and explain this very remarkable phenomenon.

*** There was laid on the table, for the inspection of the members present, a small floating collimator, made by M. Amici. This instrument was only 1½ inch in length, and, together with the mercury on which it floats, was packed in a small round box, 2 inches diameter in the inside, and 2 inches high, which might be carried in the pocket. It is intended for voyagers, and other persons, to whom a larger instrument would be a great inconvenience. It was the first that had ever been made of such small dimensions.

*** There was also laid on the table a drawing, or representation, of several *shooting stars*, that were observed at Plymouth from the 11th to the 14th of November last, together with the direction which they severally took, as compared with the fixed stars then visible.

II. Stars observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of November, 1836.

Day.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
Nov. 1	*q* Cancri	...	...	9 9 51,00
	Moon 2 L. ...	...	...	9 27 16,48
3	Moon 2 L. ...	...	...	11 4 56,97
4	Moon 2 L. ...	...	...	11 52 45,36
13	Moon 1 L. ...	...	...	19 47 4,27
14	ψ Capricorni .	20 36 24,42	20 36 24,38	...
	Moon 1 L. ...	20 50 38,14	20 50 38,04	20 51 10,57
	ζ Capricorni ..	21 17 19,63	21 17 19,79	21 17 19,32
	ι Capricorni ..	21 27 55,39	21 27 55,59	21 27 55,13
18	τ Piscium	23 53 35,80	...	23 53 35,27
	t Piscium......	0 17 2,65	...	...
	Moon 1 L. ...	0 24 22,26	..	0 24 47,73
	m Ceti	0 44 40,63	...	0 44 40,58
	ι Piscium	0 54 29,19	...	0 54 29,44
19	Moon 1 L. ...	...	1 12 7,63	1 12 33,77
	s Piscium	...	1 36 47,80	...
20	*s* Piscium	1 36 47,45	1 36 47,61	1 36 47,44
	Moon 1 L. ...	2 0 6,55	2 0 6,05	2 0 32,37
	38 Arietis.....	2 36 5,36	2 36 5,44	2 36 5,22
	ϖ Arietis	2 40 12,51	2 40 12,62	2 40 12,29
21	38 Arietis.....	...	...	2 36 5,31
	ϖ Arietis	...	...	2 40 12,56
	Moon 1 L. ...	...	...	2 49 41,98
	δ Arietis	...	...	3 2 19,31
	g Arietis	...	...	3 14 41,82
23	A[1] Tauri	...	3 55 4,38	...
	A[2] Tauri	...	4 7 43,35	...
	Moon 2 L. ...	...	4 35 32,39	...
	n Tauri	...	5 9 29,47	...
	β Tauri	...	5 15 59,85	...
24	*n* Tauri	5 9 29,48	5 9 29,42	...
	β Tauri	5 15 59,71	5 15 59,85	...
	Moon 2 L. ...	5 30 20,86	5 30 19,62	...
30	Moon 2 L. ...	...	10 43 48,31	10 44 13,72
	n Leonis	...	11 7 18,85	...
	ι Leonis	...	11 15 24,29	...

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *January* 13, 1837. No. 3.

The following communications were read: —

I. Translation of a Paper by Professor Bessel (*Astron. Nachr.* No. 320) on the Solar Eclipse of May 15, 1836. Translated by Mr. Galloway.

The observatory of Konigsberg is situated so near the northern limit of that zone of the earth's surface within which the eclipse appeared annular, that Mr. Bessel could not determine with certainty, from the previous calculations, whether the limit extended to the observatory, or went beyond it. Encke's *Ephemeris* gave the eclipse annular for the observatory, but it gave the annulus at its narrowest part a breadth of less than 4″, which is within the limits of the probable errors of the tables in respect of the moon's latitude. It was doubtful, therefore, whether the eclipse would appear annular; but, in any case, the near proximity of the observatory to the annular limit gave the observation an unusual interest, inasmuch as an opportunity of observing an eclipse under similar circumstances must be of very rare occurrence.

Mr. Bessel observed the eclipse with a power of 179 applied to the heliometer. During its continuance, the part of the moon's border on the sun exhibited, as usual, protuberances and cavities. The exterior edges of the cusps, which, at the time of the nearest approach of the centres, were extremely fine, had the regular curvature of the sun's border, but their interior edges shewed the irregularities of the border of the moon. They appeared also, when their extremities fell on a part of the moon's limb not particularly mountainous, to terminate in sharp, prominent points; whereas, when the extremities fell on the slope of a lunar mountain, they assumed a corresponding figure. There was no rounding of the points, which could not manifestly be ascribed to this circumstance; nor was any thing seen in the telescope made use of, which could be considered as indicating an irradiation of the sun's disk.

At the time the cusps approached nearest to each other, they terminated at a very rugged part of the moon's limb; and Mr. Bessel expected, but in vain, that some parts of the sun's disk would make their appearance between the points. About twenty-five seconds, however, before the nearest approach, there appeared, near the termination of the upper cusp, a point, which, though far indeed from exhibiting the clear light of the sun, was sufficiently distinguishable in the powerful telescope of the heliometer. As the

cusps had then approached very near, he expected every instant to see the annulus formed; this, however, did not take place, but the luminous point became more bright, and other similar points appeared besides it, which soon united, and in this manner rendered visible the whole of the moon's border, between the extremities of the cusps. Still he looked for the formation of the annulus itself; and he first remarked it would not take place, when the disappearance of some points on the moon's border shewed that the eclipse had begun to decrease.

During the time that the moon's limb continued visible, he could clearly distinguish the hills and valleys. The brightness of the moon's limb might have about the same proportion to the brightness of the sun, as the brightness of that part of the moon's disk which is illuminated by the earth a short time before or after the change, has to the brightness of the portion illuminated by the sun. According to this estimate, the illumination of the visible part of the moon's border was so strong, that it might be supposed it could scarcely escape notice; yet it was not perceived by three other observers, Mr. Busch, Mr. Zernow, and William Bessel. When the cusps approached nearest each other, he estimated their distance to be four minutes at most. It would have been easy to have measured it, but he was apprehensive that, by so doing, he should hazard the observation of the duration of the annulus, which he expected every instant to appear.

The time during which the whole portion of the moon's limb between the cusps was seen, was perhaps fifteen seconds; some points of it were visible longer. The depressed parts of the border appeared in a stronger light than the protuberant parts. This light was much feebler than that of the sun's disk, but much stronger than the usual light of the moon, being, in fact, visible through the coloured glasses of the telescope, which (as he afterwards ascertained by direct experiment) the ordinary moonlight could not penetrate, even when the illumination of the atmosphere was much feebler than it was during the eclipse.

Mr. Bessel considers the phenomenon observed by Mr. Van Swinden at Amsterdam, on the occasion of the solar eclipse of the 7th September, 1820, of which a description is given in the 1st vol. of the *Memoirs of the Astronomical Society*, to have been the same as that which he observed.

This eclipse was annular at Amsterdam; but the duration of the annulus was only about three-quarters of a minute, so that the approach of the two borders before the formation of the annulus, and their separation after it was broken, took place very slowly, and Mr. Van Swinden was consequently enabled to see what preceded and followed these instants (while the borders were almost in contact), during a much longer time than can usually be done in the case of annular eclipses, in which the phenomenon, on account of its almost instantaneous disappearance, cannot, perhaps, be observed with sufficient certainty. Mr. Van Swinden also remarked a bright arch connecting the two cusps, but he makes no mention of the in-

equalities of the moon's border, which were seen on it by Mr. Bessel. As the duration of the phenomenon at Amsterdam, notwithstanding the favourable circumstances, was extremely short, Mr. Bessel thinks it probable that it was on this account that Mr. Van Swinden failed to notice all the appearances which presented themselves to him in Konigsberg, under circumstances still more favourable, by reason of the greater length of time the phenomenon continued. He thinks, therefore, that the circumstance of Mr. Van Swinden's not mentioning the inequalities of the moon's limb, affords no ground for calling into question the identity of the appearances. There is also another discrepancy between the two descriptions. The luminous arch connecting the cusps is described by Mr. Van Swinden as having a smaller radius of curvature than the moon. M. Bessel, however, thinks this was only an optical deception, occasioned by its having a different curvature from the sun.

From the estimate given above of the brightness of the line connecting the cusps, it follows that it was much too bright to be ascribable to the usual reflected light of the sun, far less to any lunar twilight, or to any inflexion of the solar rays. He therefore thinks it a probable hypothesis, that the sun itself is surrounded by a corona of luminous matter, which is not covered by the moon's disk, while the latter covers the disk of the sun. Such a corona must have a very small extension, inasmuch as it only becomes visible when the moon *nearly* covers the solar disk. That it cannot be remarked about the border of the sun, when uncovered, may be explained partly by its extremely small breadth, and partly by the feebleness of its light, in comparison of the light of the sun.

The remainder of this interesting paper is occupied with an investigation of the probable distance between the borders of the sun and moon, when the latter became visible; and from different data he concludes that, when the luminous line appeared, the distance of the moon's border from that of the sun could not be much greater than half a second, or, at most, exceed a whole second.

II. Observations of the Solar Eclipse, May 15, 1836, made at Ormskirk. By the Rev. W. R. Dawes.

"Having computed the arc included between the southern point of the sun's disk, and the point of first apparent contact, I separated the threads of a parallel thread micrometer to a distance equal to the versed sine of that arc, the sun's semi-diameter being radius. The threads having been previously placed in a direction parallel to the equator, the upper one was made a tangent to the sun's southern limb; the lower one cutting the edge of his disk on the western side at the computed point of first contact. This was brought into the middle of the field, and kept there by the equatorial motion; the power employed being 80 on my five-feet achromatic telescope. With a sidereal chronometer in my hand, I thus waited the commencement of the eclipse, which was observed to take place within about 5^s of the computed time. The first impression was made by a lunar mountain, which projected

on the disk in ***apparent contact with the micrometer thread***; and suggested the idea of the point of a lead pencil having been pushed on to the sun's disk. I immediately began to count the beats of the chronometer; and believe that the observation was accurately made to the smallest appreciable portion of time.

"The termination of the eclipse was perfectly well observed with the same power on the telescope; and the sun's edge was sharply defined both at the beginning and ending.

"On the moon's edge passing over the largest of the solar spots visible on the day of the eclipse, a phenomenon was distinctly noticed, which was not observed during the occultation of any of the smaller ones. The black nucleus appeared considerably illuminated close to the moon's limb.

"Mr. Lassel and Mr. Alfred King were observing at Liverpool; and, having noticed a remarkable appearance during the occultation of this spot, they made diagrams of the phenomenon, without previous communication, which agreed very nearly with each other and with that which I made myself. No distortion of the edges of the spot was noticed by any of the observers.

"At the greatest obscuration, *Mercury* was distinctly seen in the finder of my equatorial, having an object-glass of only 0·55 inch diameter, and power 7. In the 5-feet achromatic, power 140, this planet was better seen than under any other circumstances, the sky appearing of a fine purple colour from the diminished solar light. *Venus* was obvious enough to the naked eye; but *Jupiter* was not, nor were any of his satellites visible with the 5-feet achromatic."

III. Observations of the Meteoric Bodies which were seen at Plymouth, Nov. 11–15, 1836. Communicated by a Committee of the Plymouth Institution.

This communication consists of three tables:

1. The original observations by Lieutenant Derriman, of the Medway; Messrs. Dryden and Keys, of the Malta; Lieutenant Knapman, of the Armada; Lieut. Hall, of the Invincible; Lieut. Parker, of the Windsor Castle; Lieut. Darke, of the San Josef; and Mr. Southwood of Devonport. They give the time of each phenomenon, its altitude and direction, the character of the light, and short incidental remarks.

2. Results of observation tabulated.

3. A diagram, exhibiting the situations of the meteors.

On the 11th, one only was noted; on the 12th, the weather was hazy; on the 13th, 14th, and 15th, were noted 25, 18, and 16;—in all, 60.

Of these the directions were as follows: west, 31; south-west, 3; north-west, 1; north, 6; north-east, 1; south, 4; south-east, 2; south-east by east, 1; east, 2; unknown, 9.

Of the whole, 41 were observed between midnight and sunrise, and 19 between sunset and midnight.

The constellations in which the phenomena appeared were as follows: *Cygnus*, 1; *Musca Borealis*, 1; *Auriga*, 12; *Lynx*, 2; *Perseus*, 5; *Camelopardalus*, 1; *Triangula*, 2; *Ursa Minor*, 1; *Ursa Major*, 1; *Leo Minor*, 1; *Andromeda*, 2; *Taurus*, 14; *Cassiopeia*, 2; *Cepheus*, 1; *Pegasus*, 2; *Orion*, 2; *Monoceros*, 1; *Gemini*, 3; *Draco*, 1; *Cancer*, 1; *Cetus*, 1; *Canis Major*, 1; *Virgo*, 2.—Total, 60.

IV. Observations of Halley's Comet, after its Perihelion Passage. By Sir John Herschel.

This paper contains the observations of Halley's comet, compared with certain stars near it, from January 25th to May 5th, 1836, both inclusive, made in the southern hemisphere, together with the right ascensions and declinations of the comet, deduced from those comparisons, on the respective days of observation; and are a valuable appendage to those made in the northern hemisphere. In the letter to Mr. Baily, containing the above observations, were enclosed a number of drawings of the appearance of the comet, and which exhibited in a striking manner the remarkable increase in its size, after the perihelion passage; and also the formulæ for computing the parallax and refraction, which Sir John had not time to complete before the ship sailed. Sir John Herschel speaks in high terms of the ephemeris of Halley's comet, which had been computed by the superintendent of the *Nautical Almanac*, copies of which had been forwarded to the Cape of Good Hope: and he also states that the detail of his observations with the twenty-feet reflector will shortly follow the present communication.

V. Determination of the Longitude of the Observatory at Port Louis, Mauritius. By Captain Lloyd. Communicated by Sir John Herschel.

Of 54 observations of moon-culminating stars, made in the year 1834, the greatest and least determinations are $3^h\ 51^m\ 10^s,80$ and $3^h\ 49^m\ 20^s,11$.

The mean of the whole is	3^h	49^m	$57^s,64$	E. of Greenwich.
Mean of 4 eclipses, 20 occultations, and 62 lunar distances	3	49	58,20	

VI. Continuation of Researches into the Value of *Jupiter's* Mass. By Mr. Airy. (Continued from Vol. III. p. 113.)

The method of making these observations has been already described; the principal difference in the present series being that, with one exception, the observations were made at Greenwich with the Shuckburgh equatorial. From twelve sets of observations (November 21, 1835, to February 20, 1836) it appears, that the mass of the Jovial system is $\frac{1}{1045\cdot29}$, and no trace was observed of that difference between results, according as the satellite preceded or followed the planet, which was so apparent in the first series.

The following is the list of results :

Year.		Log. Mass of the Jovial System.		No. of Obs.
1832	....	6·9793486	....	10
1833	....	6·9797717	....	6
1834	....	6·9792902	....	5
1835 and 1836	....	6·9807648	....	12

The increase, Mr. Airy observes, appears to indicate that the tabular inclination of the satellite's orbit to the equator is too small, in which case the observations of 1835 and 1836 (when the line of elongation was nearly parallel to the equator) would be the best. On the whole, Mr. Airy is inclined to adopt the mean determined by giving weights of 2, 2, 3, and 8, to the preceding results : which gives $\frac{1}{1046 \cdot 77}$ as the mass of the system in question.

VII. Stars Observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of December, 1836.

1836.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
Dec. 2	Moon 2 L. ...	...	12 17 22,31	12 17 48,01
	γ^1 Virginis ...	...	12 33 22,81	12 33 22,59
	δ Virginis ...	...	12 47 22,14	12 47 22,01
3	γ^1 Virginis ...	...	...	12 33 22,47
	δ Virginis ...	...	...	12 47 22,09
	Moon 2 L. ...	...	...	13 6 0,09
13	μ Capricorni .	...	21 44 22.86	...
	ι Aquarii	...	21 57 36,41	...
	Moon 1 L. ...	...	22 26 32,00	22 27 2,09
	δ Aquarii......	...	22 45 58,86	22 45 58,55
	ψ^3 Aquarii ...	...	23 10 28,02	...
14	ψ^3 Aquarii ...	23 10 27,60	...	...
	Moon 1 L. ...	23 19 40,28	...	...
	p Piscium . ..	23 50 18,97	...	...
	q Piscium	23 53 27,58	...	...
15	*p* Piscium ...	23 50 19,08	...	...
	q Piscium	23 53 27,61	...	23 53 27,53
	Moon 1 L. ...	0 9 29,72	...	0 9 54,95
16	*m* Ceti	0 44 40,40	0 44 40,70	0 44 40,48
	Moon 1 L. ...	0 57 29,30	0 57 28.56	0 57 54,46
	μ Piscium	1 21 38,74	1 21 38,95	1 21 38,89
	ν Piscium......	1 32 57,05	1 32 57,17	1 32 57,10
19	ϵ Arietis	..	...	2 49 54,10
	δ Arietis	...	...	3 2 19,24
	Moon 1 L. ...	...	...	3 23 28.73
	A^1 Tauri	...	...	3 55 4,47
	ω^2 Tauri	...	...	4 7 43,45
20	Moon 1 L. ...	...	4 14 52,50	4 15 21,17
	τ Tauri	...	4 32 28,80	...

1836.	Object.	Apparent R.A. from Observation, Greenwich.	Apparent R.A. from Observation, Cambridge.	True Sidereal Time, Edinburgh.
		h m s	h m s	h m s
Dec. 21	τ Tauri	...	...	4 32 28,64
	ι Tauri.........	...	...	4 53 22,12
	Moon 1 L. ...	...	...	5 9 14,48
	C Tauri	...	...	5 43 5,53
23	ι Geminorum	...	...	6 34 54,79
	Moon 2 L. ...	...	7 1 44,80	7 2 14,62
	α Geminorum	...	7 24 12,30	...
	α^1 Geminorum	...	...	7 24 11,64
	α^2 Geminorum	...	...	7 24 11.94
	β Geminorum	...	7 35 20,70	7 35 20,34
24	α Geminorum	7 24 12,15	7 24 12,33	...
	β Geminorum	7 35 20,85	7 35 20,67	...
	Moon 2 L. ...	7 56 10,16	7 56 9,55	...
	η Cancri	8 23 17,01	8 23 17,05	...
	γ Cancri	...	8 33 51,18	...
31	θ Virginis	...	...	13 1 29,94
	α Virginis	...	...	13 16 35,99
	Moon 1 L.* ...	...	...	13 30 34,04
	Moon 2 L.† ...	...	...	13 32 44,84
	κ Virginis ...	...	..	14 4 11,41
	λ Virginis ..	...	...	14 10 16,73

* Dark. † Enlightened.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *February* 10, 1837. No. 4.

Report of the Council of the Society to the Seventeenth Annual General Meeting, held this day.

The Council, agreeably to the annual custom, proceed to report, at this the Seventeenth Anniversary of the Society, upon the several subjects which have engaged their attention during the past year, or which may be interesting to the Fellows at large. And it is with much pleasure that they can commence their statement by announcing the acquisition of an additional room to their apartments, which was given up to the Society, by His Royal Highness the President of the Royal Society, on the 30th of November last. This room is situate immediately over the meeting-room of the Society, and affords an opportunity of placing the property of the Society in a more convenient and accessible position. The Council, however, at the same time regret that the rooms below the ground floor, which they were in hopes would have been placed in the possession of the Society before this time, have not yet been obtained.

Since the last Anniversary, the Committee which was formed for examining and arranging the books in the library, have terminated their labours. Many of the volumes have been bound, and, by a methodical arrangement of the whole, the books are now more accessible to the Fellows. The Assistant Secretary is engaged in making a catalogue; which, when completed, will render the library more available than it has hitherto been. When it is considered that very few of the books in the library have been purchased by the Society, but that they are almost wholly presents from the respective authors, or from public bodies, and that many of them are rare in this country, it is evident that this portion of the property of the Society cannot be guarded with too great care, nor the rules for its circulation and protection too strictly enforced. At the same time the Council are desirous of impressing upon the members at large the expediency of extending this portion of the means of the Society, so as to make it a focus of rare and valuable works on the science, and to prevent the necessity of an appeal to other sources for information on all points connected with physical or practical astronomy.

The Auditors appointed by the Society have examined and

approved the accounts of the Treasurer for the past year, and the following is the abstract of the Receipts and Expenditure during that period: to which is annexed the present state of the funds and property of the Society, as nearly as the same can be estimated.

Receipt.

	£.	s.	d.
Balance of last year's Account	93	13	7
1 year's dividend on 500*l.* Consols	15	0	0
½ yr's div. on 1409*l.* 15*s.* 9*d.* New 3½ per Cents	24	13	5
Ditto on 1431*l.* 13*s.* 4*d.* ditto	25	1	1
For 2 silver medals and cases allowed to S. Hole-house, Esq. by the Council	3	14	0
For 100 copies of Captain W. H. Smyth's paper, "Telescopic Appearance of Halley's Comet"	0	13	9
On account of arrears	43	1	0
82 annual contributions—1836–1837	172	4	0
2 compositions	42	0	0
10 admission fees	21	0	0
9 first year's contributions	16	16	0
Sale of Memoirs	171	9	0
	£629	5	10

Expenditure.

	£.	s.	d.
William Wyon for 4 silver medals and cases	6	8	0
J. Moyes for printing Monthly Notices, Nos. 16 to 23, both inclusive, and for paper	27	13	6
Remnant and Edmonds for binding books for the Library	17	2	3
J. Jackson for ditto	6	10	5
J. B. Baillière for books	2	15	0
Purchase of 21*l.* 17*s.* 7*d.* New 3½ per Cents (the Rev. S. W. Waud's composition)	21	0	0
J. Weale for paper and printing Vol. IX. Memoirs of the Society, &c.	236	5	2
J. Basire for drawing Telescopic Appearance of Halley's Comet 1835, and paper and printing	8	10	0
J. Basire for engraving view of the Armagh Mural Circle	16	5	0
R. Lloyd for paper and printing two views New Armagh Mural Circle	3	0	0
The Guardians of the Strand Union, two poor rates	4	3	4
1 yr's salary to the Assist.-Sec., to Dec. 10, 1836	100	0	0
Harrison and Ranson for stationery supplied for the Society	10	8	0
For coals, wood, oil, and candles, for the year	11	4	5
Tea, sugar, cakes, and biscuits, milk and cream, ditto	13	13	0
Porterage and porters' work at the rooms, cleaning windows, chimneys, &c.	10	18	0
Engraving medals, and printing, &c.	2	8	10
Collector's and bookseller's commission on sale of Memoirs, and collection of contributions, &c. 334*l.* 14*s.* 6*d.*	14	4	6
Balance in the Treasurer's hands, 30th Jan. 1837	116	16	5
	£629	5	10

The assets and present property of the Society may be estimated as follow : viz.

		£.	s.	d.
Balance in the hands of the Treasurer		116	16	5
Arrears, January 30, 1837.				
1 contribution of five years' standing	£10 10 0			
1 ——— of four ditto	8 8 0			
7 ——— of three ditto	44 2 0	130	12	0
7 ——— of two ditto	29 8 0			
22 ——— of one ditto	46 4 0			
2 admission fees and first year's contributions, at four guineas each, new Fellows	8 8 0	27	6	0
6 ditto and ditto, at three guineas each ditto	18 18 0			
£1431, 13*s.* 4*d.* 3½ per Cent stock; £500, 3 per Cent ditto — valued at about		1850	0	0
Unsold Memoirs.				
Various astronomical instruments, books, prints, &c.				
2 gold medals unappropriated.				
2 silver ditto ditto.				

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year : viz.

	Compounded.	Annual Contributors.	Non-resident.	Patron, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1836	66	117	99	5	287	37	324
Since elected	1	17	...	1	19	...	19
Deceased	−1	−2	−3	...	−6	−1	−7
Resigned	...	−2	...	...	−2	...	−2
Removals	+1	−1					
	...	+1	−1				
Reinstated	...	+1	...	...	+1	...	+1
February 1837	67	131	95	6	299	36	335

It is always a source of regret to the Council, in these annual statements, to have to recur to the loss which the Society has sustained by the decease of any of its distinguished members or benefactors. Amongst the deaths in the past year, the Society has to deplore, on the home list, the loss of Mr. Pond, Lieut. Murphy, Capt. Horsburgh, Mr. Ramage, and Professor Farish, together with that of M. Gambart, one of the associates.

Mr. Pond was born in London about the year 1767. His father realised in trade a fortune sufficient to enable him to retire in the prime of life, and to settle at Dulwich, where he passed the mainder of his days. When about seven years old, Mr. Pon nine sent to school, under the Rev. Mr. Garrow, at Hadleigh some Barnet, and, two or three years afterwards, to Mr. Cherry using

master of the free grammar-school at Maidstone. Mr. Cherry was afterwards head master of Merchant Tailors' School.

After the age of fourteen, he resided at home with his family, and attended as a private pupil Mr. Wales, then mathematical teacher at Christ's Hospital, better known as the nautical astronomer who accompanied Capt. Cook in his voyages of discovery. To Mr. Wales, Mr. Pond remarked an appearance of discrepancy in the Greenwich Observations, implying some imperfection in the instruments, but his suggestion was, naturally enough, neglected by the veteran mathematician, himself a friend and admirer of Dr. Maskelyne. At a subsequent period, Mr. Pond verified his early suspicion.

At sixteen he was entered at Trinity College, Cambridge, with a particular introduction to Dr. Waring, the Lucasian professor of mathematics. Mr. Jones was his public, and the late Professor Lax his private tutor. Unfortunately, his attention was not directed to the studies of the university as steadily as it should have been. His mathematical taste and talents were of a high order, and so esteemed by his fellow-students; but he had all along been extremely fond of chemistry, and this distracted his application to geometry. He had erected a furnace and laboratory at his father's house; and at Cambridge he attended with a delight, which he never forgot, the lectures of Dean Milner, who then filled the chair of chemistry with great ability. It is more to our purpose to notice that he was one of three students who united to induce the Plumian professor, Mr. Vince, to give a course of lectures on practical astronomy.

His health, however, gave way, and a severe pulmonary attack compelled him to seek a warmer climate. He spent two or three years in the south of France and in Spain, and then returned to college, where, during a residence of two or three years, he formed many valuable friendships, which were only dissolved by death. To this part of his life he always reverted with great pleasure; and among her numerous sons, Trinity College had none more zealous and affectionate than Mr. Pond. A second attack of illness obliged him to go abroad, and he resided for some time in Portugal, Constantinople, and Egypt. On his return he settled at Westbury, in Somersetshire.

He had become possessed of a very beautiful altitude and azimuth circle by Troughton, and with this instrument he undertook a series of observations, which, with the conclusions deduced from them, are published in the *Philosophical Transactions* for 1806. In this important memoir he proved, beyond the possibility of doubt, *that the Greenwich quadrant had changed its form since it was erected by Bird and used by Bradley.** This had been suspected before, partly on account of the difference found between the distances of the two solstices from the equator. The subject however, so much complicated with refraction, and the then

* Mr. Troughton shewed this opinion to be true, by actually measuring radii and chords with an optical beam compass. The middle of the arc ..nd to be flattened and brought nearer to the centre.

fashionable repeating circle so oddly confirmed this apparent difference (although no physical cause could be assigned for its existence), that we may fairly consider Mr. Pond's memoir to have been the first *conclusive* evidence against the quadrant, and that which led to a change in the form of the declination instrument at Greenwich and elsewhere. He was elected a fellow of the Royal Society in February 1807, and married the same year.

After the publication of this investigation, Mr. Pond resided for some time in London, where he continued to occupy himself with practical astronomy; and, on the death of Dr. Maskelyne in 1811, was appointed astronomer-royal. The instruments which Mr. Pond found at Greenwich were those erected for Bradley sixty years before: the quadrants, now deformed by the yielding of their frames; the transit, which had, from some cause, ceased to describe a true circle; and the zenith sector. The only accurate "foundations" of astronomy, were Maskelyne's right ascensions of 36 principal stars, and the zenith distances of γ *Draconis*. The mural circle, which had been previously ordered, came into Mr. Pond's possession in 1812, but the mode of using it, and the defects, or rather the accidents, to which the new instrument was liable, were to be discovered, and it was indeed fortunate that this *experiment* fell into his hands. Mr. Pond saw, almost intuitively, the vast superiority of this over every preceding form of the declination instrument, and for some years he, and the artist who constructed it, were, perhaps, the only persons who did clearly see, and broadly assert, that the operation of a circle does not depend upon having a bearing on each side, or a complete axis.* The gigantic transit was erected in 1816, and it is from this epoch we may date correct catalogues in Right Ascension and in North Polar Declination of English staple.

A mural circle by Mr. Thomas Jones, which was intended for the Cape of Good Hope, was sent in 1825 to Greenwich, that it might previously be tried and verified. In the course of this inquiry, Mr. Pond hit upon his method of combining two circles, so as to form one instrument, which he always deemed to be a most important improvement. At his earnest entreaty, he was allowed to keep the second circle, which, with its prototype, continues to adorn the Royal Observatory. The 25 feet zenith tube was only completed in 1833, and a very ingenious method of turning it to the best account, and of eliminating the errors of the measuring screw, was adopted by Mr. Pond; but some defects in the mechanism, and difficulties in equalising the temperature of the room (which are now understood to be overcome) have hitherto prevented the performance of this noble instrument being altogether satisfactory.

The annual parallax of some of the brightest of the fixed stars

* Those who wish thoroughly to understand the mural circle, should examine Mr. Pond's memoirs, and the volumes of the *Greenwich Observations*. Some account may be found both of the instrument, and of Mr. Pond's mode of using it, in the *Penny Cyclopædia*, article "Circle."

had been investigated by Dr. Brinkley; and, from a considerable series of careful observation, he determined the parallax of α *Lyræ*, α *Aquilæ*, *Arcturus*, and α *Cygni*. This result was not confirmed by Mr. Pond, who declared the quantity of parallax discovered by Dr. Brinkley to be inadmissible, and contradictory to the results of the mural circle. Mr. Pond supported this opinion by observations on α *Cygni* and β *Aurigæ*, with a fixed telescope, used to measure differences. Further observations convinced Dr. Brinkley that the amount of parallax was sensible, and he attempted to shew that it was not inconsistent with the data of the Greenwich instrument. Both astronomers remained, as might have been expected, in their original opinions; for the whole inequality, if true, is too small to be perceived, except in masses of favourable and careful observations. It is to their credit, that neither, in this debate, forgot the respect due to his opponent or to himself, and that they differed decidedly and positively, without losing the language or temper of gentlemen and philosophers.* The opinion of the present day seems to be, that though there must be some parallax, its existence was not *proved* by Dr. Brinkley, and that Mr. Pond was justified in denying *that any amount of parallax which is sensible in our present instruments had been detected.* The discovery of the distance of any fixed star seems to be yet a desideratum in astronomy. The Royal Society gave the Copley medal in 1827, to Mr. Pond, for his various papers and observations on subjects of astronomy. He had received the Lalande medal in 1817 from the Academie des Sciences, of which he was a corresponding member.

For several years Mr. Pond was subject to very painful and harassing complaints. He resigned his office towards the close of 1835, when a retiring pension of 600*l.* was granted him for life. He died at Blackheath, on the 7th September, 1836, and was buried at Lee, in Kent, in the same tomb with his predecessor Halley.

An examination of Mr. Pond's observations and memoirs, during the twenty-four years he held his arduous and important office, will shew that neither his feeble health, nor repeated discouragements, cooled his zeal for the public service, or his love for practical astronomy. His friend Troughton declared, that "a new instrument was at all times a better cordial for the astronomer-royal than any which the doctor could supply;" and certainly, neither pain, nor bodily weakness, could keep Mr. Pond in a prudent state of rest, when he had to bring a new instrument into work, or to make an interesting observation. As a handler of astronomical instruments, the same competent authority declared, "that Mr. Pond had, within his knowledge, no equal or rival except Captain Kater;" and his excellence as an observer is shewn by his use of the Westbury circle, and by the early volumes of the *Greenwich Observations*, many of which he personally made.

* This controversy on parallax is well worth the attention of the practical astronomer. See *Philosophical Transactions*, 1810, and subsequent volumes, and the 12th volume of the *Transactions of the Irish Academy*.

The mode of observation established by Mr. Pond, *is now substantially practised in every well-conducted British observatory, and may be said to be substantially different from any which preceded it.* Besides the peculiar methods which are of his invention, he first laid down and practised the broad principle of multiplying observations, and relying upon masses,* only rejecting, and that very sparingly, such as are manifestly erroneous. It is not too much to say, that meridian sidereal observation (which excludes the Herschelian branch of astronomy) owes more to him than to all his countrymen put together, since the time of Bradley. In 1833, Mr. Pond supplied a want which had been long felt, by the publication of a truly standard catalogue of 1113 stars.

The application of revolving or shifting microscopes to circles moving in azimuth, was suggested by Mr. Pond to Troughton, and executed first in the Westbury circle. He also applied the *repeating table* to the theodolite, and in both these instances procured nearly all the advantages of the circle of Borda, without any of the imperfections of that instrument. The instruments which were constructed by different artists under Mr. Pond's direction, are remarkable for their symmetry, strength, and elegance. Of the mural transit, recommended by him for Paramatta and for Oxford, it is yet too early to speak from actual experience; but this form, which is a return to the *rota meridionalis* of Röemer, combined with the solid fixing of the microscopes, seems the most promising which has yet been given to the transit circle.

It has already been mentioned, that Mr. Pond's attention to mathematics when at Cambridge was not equal to his opportunities, nor such as might have been expected from his talents and early education. This was a subject of regret to him in after life; but it must be remembered, that, at that time, mathematico-physical research was almost unknown in this country, and that the then course of reading at Cambridge (though it might and did, in Brinkley and Woodhouse, lead to better things), was not such as to give any immediate grasp upon the physical constitution of the universe. Mr. Pond had, from early youth, all the mathematics strictly necessary for a practical astronomer, and was perfectly well acquainted with *plane* astronomy, in distinction to *physical*. This was quite sufficient for the branch of astronomy to which he particularly devoted himself, viz. the determination of the places, corrections, motions, &c. of the fixed stars,—the basis on which all ulterior progress must be founded. He was perfectly free from that narrowness of comprehension which too often marks the dexterous manipulator. He always felt and expressed the

* When the present board of visitors was instituted, a sub-committee was appointed to suggest any alterations in the Royal Observatory which might seem advisable. The computation and reduction books were laid before them, and they were agreeably surprised to find such order and extent of calculation. Every observation was, and always had been, carefully reduced. The partial results, now published in the *Greenwich Observations*, had been omitted solely on account of the expense.

highest respect for those who were to take up the subject where he laid it down, and were destined to pierce into the future by the knowledge which he could afford them of the past. It is some corroboration of this point of Mr. Pond's character, that, besides his memoirs and observations, the only work he ever published is a translation of the *Système du Monde* of La Place. Rare and fortunate individuals may be sometimes found who unite the requisites which form a perfect astronomer,—delicacy of organ, mechanical skill, mathematical knowledge and invention, and unwearied industry; but if the choice of an astronomer-royal lay between a skilful practical observer (not a noter of phenomena) and a deep geometer, no one could doubt where that choice should fall. In the circle of Mr. Pond's contemporaries, can one be pointed out who would have filled the office as ably as he? Let any one look at the state of practical astronomy at the Royal Observatory and elsewhere, as he found it, and as he left it, and then answer the question.

Mr. Pond, though independent in temper, was remarkably mild and gentle in his manners; and his health and disposition, together with his attention to his office, and his dislike to every thing like contention, which almost amounted to a failing, kept him very much at home, and out of the reach of general acquaintance. His memoirs were short and compressed; and as they *supposed* the reader to be an astronomer, and consisted chiefly of facts tabularly arranged, they were dry, and absolutely unintelligible on a careless or cursory perusal. This invited at different times a good deal of criticism; but it may be said, that of those who thus decided upon the manner in which the astronomer-royal performed his duty, there was hardly one whose previous publications could be cited to prove his acquaintance either with the principle of Mr. Pond's methods, or even with the routine of his practice. Upon one occasion, when a formal charge was made against the accuracy of his Observations, and the fidelity with which they were printed, a very spirited vindication of the *Greenwich Observations* was offered by our illustrious associate Bessel (*Astronomische Nachrichten*, No. 84), and, coming from such authority, this stopped all further discussion.

The scattered remarks which are to be found in the volumes of the *Greenwich Observations* and in Mr. Pond's memoirs inserted in the *Transactions* of the Royal and of the Royal Astronomical Societies, are full of valuable information, and supply many hints for consideration to the patient and intelligent practical astronomer, who will be compensated for the inconvenience of the form, and the want of arrangement, by the greater clearness and distinctness which original documents always convey. His views, as detailed in conversation, were acute, sound, and philosophical; and those who have enjoyed his society when in tolerable health, will not readily forget it.

Mr. Pond's zeal led him to urge repeated increases of the establishment at Greenwich upon the government, and in the end,

with great success. He commenced with one assistant, and he left it with six; and even yet, such is the severity of observation and of reduction demanded by the Greenwich instruments, and the present state of astronomy, that the Royal Observatory may be considered, in fine weather, to be not sufficiently provided with hands. In these and similar attempts, Sir Joseph Banks, then president of the Royal Society, gave him effectual aid; but after his death, it was only by repeated applications, that Mr. Pond could obtain the amount of force absolutely necessary to carry his system of observation into full vigour. He also effectually resisted a well meant, but injudicious attempt, to give him for assistants persons who, from the mode of their appointment and pretensions, were little likely to submit to the rigid subordination required in a well-ordered establishment. His firmness on this occasion probably saved the Greenwich Observatory from being a board of co-ordinate rivals, instead of the best disciplined, most effective, and most economical institution in the country.

Mr. John Ramage, well known as a practical optician, was a native of Aberdeen. He was originally engaged in trade, but began at an early period of life to give his attention to astronomical subjects, and particularly to the means of improving the reflecting telescope. His first essays were made on telescopes of the Gregorian form; but experience soon induced him to abandon that construction, and to adopt the principle of the front view, as had been done by Sir William Herschel. In 1817 he constructed a telescope of 20 feet focal length, with a mirror of 13½ inches in diameter; and subsequently three others of 25 feet focal length, with mirrors of 15 inches. But his greatest achievement was one of 54 feet focal length and a 21-inch mirror, which was executed in 1823, and of which a description is given in the 2d Volume of the *Memoirs* of this Society. Of the immense labour which he bestowed in attempting to improve the art of making metallic specula, an idea may be formed from his statement, that it was not "till after the experience obtained in casting and polishing upwards of a hundred 15-inch mirrors, besides numerous smaller ones, that he was enabled to improve the processes recommended by writers on the subject."

Mr. Ramage's experiments were not confined to the casting and polishing of specula, but extended also to the means of simplifying the mechanism of the stands, and of facilitating the use of reflecting telescopes, and of improving the methods of mounting equatorial instruments in general. The mechanical contrivances which he employed for these purposes evince very considerable ingenuity.

Though Mr. Ramage was rather an observer of phenomena than a cultivator of the science of astronomy, he possessed extensive information on most subjects relating to its practice and theory, as well as several other branches of physics. He died at Aberdeen on the 26th of December, 1835, in the fifty-second year of his age; and this account ought more properly to have been contained in the Report of the last anniversary; but the inform-

ation of his decease did not reach the Council in sufficient time for such communication.

Captain James Horsburgh was born at Ely, in Fifeshire, on the 23d of September, 1762. His parents, though respectable, were poor: yet they contrived to give him all the benefit which a small school in the neighbourhood could instil; and he was afterwards apprenticed to a vessel in the coal trade. In this hard noviciate he had nearly served his stipulated time, when his ship was captured, and himself carried a prisoner to Dunkirk. After his liberation, young Horsburgh made a voyage to the West Indies; and, on his return to London, he proceeded to Calcutta, but in a humble capacity. Here he joined the country trade as a free mariner; and, by his diligence and good conduct, became chief mate of the Atlas, in 1786. This vessel had the misfortune to be wrecked, in the same year, upon the isle of Diego Garcia, owing to the erroneousness of the charts then in use; and the disaster awakened the mind of Horsburgh to the neglected state of Oriental hydrography. Thus stimulated, during the ten following years in which he served as first mate of several large ships which traded between Bombay, Bengal, and China, he neglected no opportunity of examining the various bays, coasts, and islands he touched at, to the extent of his means and ability. To qualify himself for this object, he had, by the assistance of books, previously exercised himself in the computation of lunar observations, and in the use of chronometers. He also taught himself drawing and spherics, from one of the most useful of nautical works, *Robertson's Elements of Navigation.*

The observations and corrections thus made enabled him to construct three charts of the China Sea and its channels, which being forwarded from the factory at Canton, by Mr. Drummond, now Lord Strathallan, they met with such approbation from Mr. Dalrymple, that the East India Company sent him their thanks, with a pecuniary present for the purchase of nautical instruments.

In 1796, Mr. Horsburgh arrived in England, where he was so kindly noticed by Sir Joseph Banks, Dr. Maskelyne, Mr. Cavendish, and other gentlemen of eminence, that it encouraged him to renew his exertions with vigour. Accordingly, in 1798, when he obtained the command of the Anna, a Bombay ship of which he had formerly been mate, he continued his observations upon a larger scale, during several voyages between Bengal, China, and England, till the year 1805, when he finally returned to this country, and, in 1810, was appointed hydrographer to the East India Company.

From the commencement of 1802 to the middle of 1804, Captain Horsburgh kept a careful register of the indications of two marine barometers, taken every four hours, day and night, at sea and in port. His curious deductions from thence of the intertropical motions, were communicated in a paper published in the 95th Volume of the *Philosophical Transactions;* and he afterwards, to extend the utility of barometrical observations, published his

Atmospheric Register, which affords a more simple view of the range of the mercury, than that of numerical tables.

Captain Horsburgh returned from China in the Cirencester; and he had the good fortune to have the late Captain Peter Heywood for his fellow passenger. Following the advice of that experienced officer, and with his cordial assistance, Horsburgh resolved to apply himself closely to his great work, the *East India Sailing Directory*, which, after five years' incessant labour, he produced. It is needless to add a remark on the value of this undertaking, as its utility is proved by the demand for four large editions, and by the praises of all its purchasers. Besides this noble contribution to the interests of navigation, Captain Horsburgh published several other works upon nautical subjects, of which we may name his communication on the "Causes of the luminous appearances of the sea," and "Remarks on icebergs seen in low latitudes," both inserted in the *Philosophical Transactions*; and a new edition of Murdoch Mackenzie's *Maritime Surveying*.

Captain Horsburgh was a man of mild unassuming manners and even temper; he enjoyed almost uninterrupted good health, which enabled him to apply to his useful pursuits till within a short time of his death, which happened on the 14th of last May, at his house at Herne Hill, in Surrey, in the 74th year of his age.

The Rev. William Farish, Jacksonian Professor of Natural and Experimental Philosophy in the University of Cambridge, was the son of the Rev. James Farish, a minor canon of Carlisle, and was born at that place, March 22d, 1758. He entered as a sizar at Magdalen College, Cambridge, in 1774, and took the degree of Bachelor of Arts in 1778, being the senior wrangler of his year. A current story in the University (not founded, however, in fact) asserts him to have availed himself, on this occasion, of the privilege of *challenging* the students who were placed above him by the examiners, or demanding an additional examination, which terminated in his favour. The public examinations were not then conducted with their present degree of exact comparison; and Mr. Farish himself (when afterwards moderator, which he was in 1783, 1786, and 1793) was the first who introduced the plan of making a numerical estimation of the value of each student's answer to each question.

Immediately upon obtaining his degree, not being then twenty years old, Mr. Farish was appointed tutor of his college. In 1794, he succeeded Mr. Pennington as Professor of Chemistry, and in 1813, he was removed to the Jacksonian Professorship, which he held till his death. This happened on the 12th of January last, at his living of Little Stonham, in Suffolk.

Professor Farish will be long remembered in the University of Cambridge, as the introducer of lectures on the state of the useful arts, accompanied by the actual performance of a great many of their processes. Having taken a personal survey of all the districts in Great Britain, in which mining, manufactures, or agriculture, were especially cultivated, he provided himself with elements for

the construction of models on a new and most ingenious plan. By means so simple that they excited no wonder, he presented working models of almost every machine which he described, and certainly of every fundamental form of mechanical contrivance. A few bars, axles, clamps, wheels, &c. appeared in different forms throughout the lectures, in all the diversified constructions which it was necessary to explain. Nothing could be conceived better adapted for the purposes of University instruction: the sameness of the elements employed excluded all the peculiarities of structure which depend upon particular details, and which are frequently the most striking external parts of a machine. In the lectures of Professor Farish, identity of principle was remembered by identity of structure, whether the contrivance were part of a watch or a watermill.

Professor Farish was the first president of the Cambridge Philosophical Society, and his only publication is a paper in the First Volume of its Transactions, on Isometrical Perspective, a method of drawing by which he gave clearness to the representation of machinery, and in general all pictures in which there are many parallels and perpendiculars. This he had invented to enable his assistants to set up his working models without his personal attendance. The method has since been written on in an elementary manner; and will, in all probability, be much used by future engineers.

Professor Farish possessed simplicity and independence of character in a peculiar degree, combined with high moral worth and social qualities. In addition to the duties of his chair, he performed those of a parish priest, to the end of his life; and it was not till his seventy-ninth year that he permitted himself to think of retirement, having been then actively engaged in university duties for fifty-nine years.

Lieut. Hastings Fitz-Edward Murphy was the son of the Rev. John Murphy, rector of Kiltallagh, county of Kerry, in Ireland: and, although he had but recently been associated with this Society, yet his ardent love for science, and his devotion in its cause, had long been previously known, and duly appreciated by many of the members. He first entered the Royal Military Academy at Woolwich, where his rising talents secured to him the notice and the friendship of the lieutenant-governor, General Mudge: and, in the year 1815, joined the corps of engineers. After a few years' service in the usual duties, he was selected to assist in the trigonometrical survey of Ireland; and was one of the officers employed in the measurement of the base line on the shores of Loch Foyle: a service for which, from his education and habits, he was peculiarly adapted. Soon after he had joined this Society, he was placed on the Council, and was a constant attendant at its meetings when in London, and assisted them most readily on every occasion with his advice and zealous co-operation. When the Council decided, in 1834, on making a new standard scale, Lieut. Murphy was the person unanimously pointed out as the most proper to carry it into execution; and, day after day, for several successive weeks, he was

unremitting in his exertions to accomplish this object. This sacrifice of time and attention, even in ordinary cases, can be properly appreciated by those only who have embarked in similar undertakings; but, when we consider Lieut. Murphy's skill in manipulation, and his rigid and almost too scrupulous accuracy in recording his observations, it is impossible to estimate too highly the results which he may have obtained. Before he had completed this laborious task, he was called away to another and a very different sphere of action. An expedition was at that time fitting out, with an intention of proceeding down the Euphrates with two iron steam-boats, under the command of Colonel Chesney, for the purpose of opening a communication with India by means of that route. Lieut. Murphy was desirous of accompanying him, as astronomer to the expedition; and, although at the prospect of a great pecuniary sacrifice, such was his active disposition and ardent love of science, that he resolved to brave the dangers and difficulties of the undertaking. He took out with him several astronomical and philosophical instruments; and, amongst others, the two pendulums belonging to this Society, which he intended to swing at various places on the line of route. It is needless here to recur to the sad catastrophe which befel the expedition during their descent down the river, as it is too well known to most of the members. But, although Lieut. Murphy himself escaped all personal danger on that occasion, yet there is too much reason to fear that his exertions immediately subsequent thereto, and the shock which a constitution naturally weak, and a mind sensibly alive to so dreadful a disaster, must have received, as well as the ungenial climate of the country, shortened the period of his existence. Soon after the arrival of the expedition at Bussora, and as soon as the hurry and fatigue of Lieut. Murphy's various duties were over, he began to complain of the oppressive heat of the place; and, on the 2d of of August, was attacked with a fever which eventually carried him off in seven days. He died universally regretted and esteemed by every one that knew him, on the 9th of August, 1836; and his remains were buried, with every mark of respect and attention, in the Armenian church at Bussora. Lieut. Murphy was a man of very mild and amiable manners, which endeared him to all his companions both at home and abroad. The astronomical and pendulum observations which he made during his voyage have not yet reached this country; but there is no doubt, when they are received and examined, they will confirm his high character for correct judgment in making his observations, and scrupulous accuracy in recording them. And this Society cannot sufficiently deplore the loss of one who had shewn himself to be so valuable and distinguished a member.

The foreign associate whose loss we have to record, was the active astronomer, Mons. Jean Gambart, who, unfortunately for the interests of science, has been cut off in the very prime of life. The observatory of Marseilles had not been distinguished so much for its observations, as for the occasional papers which were issued

by its directors, St. Jacques de Sylvabelle and M. Thulis. The principal cause of this was undoubtedly owing to the bad situation of the observatory in a crowded and confined part of the old city, and the meanness of its equipment; for even when the diligent Mons. Pons commenced his cometic discoveries there, he described the telescope which he used as being rather paralytic than parallactic. "*On ne fait plus de bonnes observations à Marseille, depuis le départ de M. Pons,*" said Baron de Zach, angry at what he termed the *astro-comique* announcement of the comet of January 1821, in *Pegasus*, by M. Blanplain, the director who succeeded Thulis; and it was under this bad reputation that the direction was assumed by M. Gambart. Youthful and diligent, he very soon elevated the estimation of the neglected observatory in the eyes of Europe, by his skill and exactness; though the means at his disposal were so inadequate to the object, as to be discreditable to a public establishment, especially in the town which had given birth to Pytheas.

Cramped thus in his efforts, M. Gambart resolved to make comets his principal object, as not only the path best adapted to his instruments, but also as one which requires the exclusive labour of some of the astronomical corps. One of the first proofs of the inequality between his means and his wants, was the discovery of the comet of May 1822, in *Auriga;* but, on this occasion, his intelligence supplied resources to his execrable instruments, and he produced such creditable observations, that his computations of the orbital elements were excellent in result. The observations and reductions of various other comets, well-known to this Society, attested at once his activity and knowledge; but the periodic comet of six years and three-quarters deserves especial mention. That object was perceived by Captain Biela, at Johanisberg, on the 27th of February, 1826; and ten days afterwards by M. Gambart, at Marseilles. The latter, without delay, calculated its parabolic elements from his own observations; and he recognised that it was not its first appearance, but that it had already been observed in 1805 and in 1772. It was necessary, from thence, to leave the parabolic for the elliptic elements; it was requisite to discover the duration of the comet's revolution, which the parabolic elements leave quite undetermined. This computation was separately undertaken by Messrs. Clausen and Gambart, who found, each of them nearly at the same time, that the new comet made its entire revolution round the sun in a period of about seven years. This period was reduced to six years and three-quarters, by our associate M. Damoiseau, on a close study of all the sensible derangements and perturbations to which it was liable; and the interest inspired by its return in October 1832, must be fresh in the recollection of this meeting.

By the unexpected demise of M. Gambart, this important and interesting branch of Astronomy has lost one of its most zealous and talented votaries; and society a pleasing, cheerful, and truly amiable man.

During the past year another volume of the *Memoirs* of the Society has been published, being the ninth of the series; and which, the Council trust, will be considered equally valuable and important with any of the preceding ones. This rapidity of publication has encroached so much on the funds of the Society, as to render it the more necessary to attend to that system of economy, and that care in the selection of papers and mode of publication, which the Council have ever wished to pursue. The very small funds of the Society will not enable them to carry into effect all the objects which they may be desirous of prosecuting; they must consequently limit their measures to those subjects which, being within the means of their resources, may best tend to promote the ends of the Institution.

The third volume of the *Monthly Notices* of the Society is now completed, and the fourth volume commences with the present session. As only a limited number of these Notices is now printed, those members who are desirous of preserving them should understand that their loss cannot readily be supplied.

In the report read at the last Anniversary, it was stated that a Committee had been formed for repeating the experiment of Cavendish on the mutual attraction of terrestrial substances. That Committee has not been inattentive to the subject; but, at present, no further progress has been made in carrying it into effect, than to ascertain that a convenient and proper site can be procured for performing the experiment.

During the past year a new Society has been formed for promoting the study of Numismatics;—which is here noticed, because a request has been made by its Members to the Council for leave to hold their meetings, during their first session, in the meeting-room of this Society. The Council, bearing in mind the advantage and benefit derived by the Astronomical Society from a similar indulgence granted to them, in their infancy, by the Geological Society, and being also desirous of extending any assistance in their power to a rising institution, have not hesitated to comply with that request; and they trust that the present meeting will approve that decision.

The instruments belonging to the Society remain in possession of the several parties to whom they have been intrusted, as at the last Anniversary. The clock made by the celebrated John Harrison, and presented to the Society by Mr. Barton, of the Royal Mint, as announced at the last Anniversary, has not yet been sent to the apartments of the Society. It is still in the hands of the clock-maker, for the purpose of being cleaned and adjusted; and Mr. Barton has directed it to be forwarded to the Society as soon as that process is finished.

The Council are happy to announce, that the United States of America are about to commence a voyage of scientific discovery, on a very extensive scale. Lieutenant Wilkes, an active and intelligent officer in their service, has recently visited Europe for the express purpose of procuring the best and most improved

instruments of various kinds. The workshops of Paris, Munich, and London, have each furnished whatever was requisite, and no pains have been spared to place the expedition in the first state of excellence in this respect. Amongst the various instruments provided for the occasion, are two invariable pendulums, one of iron and the other of brass, of a construction somewhat similar to those belonging to this Society, but so far different as to be rather better adapted for travelling instruments. Mr. Baily, who superintended the formation and construction of these pendulums, has, in conjunction with Lieutenant Wilkes (whose zeal and energy are an earnest of his future success), made a number of observations with them, prior to their being forwarded to America, for the purpose of similar experiments about to be made in other parts of the globe. A description of these pendulums, and the result of the observations made in London, will probably be submitted to the Society during the present session. It is the intention of Lieutenant Wilkes to propose that these pendulums be swung on some of the frozen lakes of America and the land contiguous thereto, on the Alleghany mountains, and in other places in the interior of that vast continent, far distant from the sea-shore; and, under the superintendence and direction of this enterprising and discreet officer, the public may look forward to correct and important results.

Amongst the presents made to the Society in the past year, the Council wish to notice most particularly the highly finished marble bust of Newton, executed by Mr. Smith, and taken from that which exists in the library of Trinity College, Cambridge. This bust has been presented to the Society by the Rev. Charles Turnor, whose family now possess the estate at Woolsthorpe, the birthplace of our distinguished countryman. And in a society whose pursuits are so inseparably connected with the vast and comprehensive discoveries of this illustrious philosopher, not only will the intrinsic value of the gift be duly appreciated and approved, but also the correct judgment and feeling of the donor, in testifying, in so appropriate a manner, his esteem and regard for the Society.

It is with much pleasure, also, that the Council announce the completion of a very useful and valuable work, undertaken by one of the Fellows of this Society, at a sacrifice of time, labour, and expense, which cannot be too highly appreciated: more especially when it is considered that the sole motive for such an undertaking was a sincere and ardent desire for the promotion of Astronomy. It is well known that the positions of many of the small stars in the catalogue of this Society have been determined solely from one observer, either Piazzi or La Caille—there being no corresponding observations of such stars in the catalogue of Bradley; and, consequently, that many discordances might now be discovered, without the means of ascertaining whether such discordances arose from instrumental errors or from proper motion. Mr. Wrottesley, therefore, has rendered an essential advantage to Astronomy by presenting to the Society a MS. catalogue of the

right ascension of 1318 stars, chiefly of the smaller magnitudes, selected from the Society's catalogue, which have been observed with that skill and attention which he is so well known to possess, and reduced, with great care, by competent computers to the year 1830. And the Council trust, that the example which has thus been so honourably furnished by Mr. Wrottesley will be followed by other members of the Society, in those departments of the science that still require illustration and investigation.

Amongst other advantages attending private observatories, when conducted with that ability which has just been alluded to, may be mentioned the valuable assistance which they afford to Astronomy, by making observations, out of the meridian, which do not exactly fall within the regular work of the public observations; and even in co-operating with them in observing such phenomena as casually present themselves, and where the result of numerous observations, in various situations, is required—such as comets, eclipses, occultations, &c. An interesting example of this kind was afforded last year, by the valuable communication of the foreign secretary, Captain Smyth, of his observations of Halley's comet; and which shews how materially and successfully the well-directed exertions of a private individual may improve and advance the object we have in view—the promotion of Astronomy.

The Members are aware that, for the last three years, this Society has been at the expense of printing and publishing gratuitously a planetary ephemeris, a work which has been found to be of great utility and convenience in every observatory. This expense, however, thus recurring annually, was a great encroachment on the very limited funds of the Society; and as the work was for the public benefit, it was but reasonable that it should be executed at the public cost. A representation and request was therefore made by the Council to His Majesty's Government that it might form an appendage to the *Nautical Almanac*, being well assured of the cordial co-operation of the Superintendant of that national work, who has been ever ready to extend its utility and means of information. The wishes of the Council were most readily complied with; and the planetary ephemeris for the present and subsequent years will therefore be regularly computed and published at the public expense.

In the report of the Council at the last Anniversary, it was stated that the Superintendant of the *Nautical Almanac* had computed a third ephemeris of Halley's comet from the 1st of August, 1835, to the 31st of March, 1836; beyond which limits it was not expected that the comet would be visible to any inhabitant of this globe. Recent accounts, however, which have been received from Sir John Herschel and Mr. Maclear, shew that it was seen as late as the 5th of May last; and, in fact, each of those astronomers has transmitted continued observations of it, after its perihelion passage, up to that date, with drawings of its appearance at different times. Of the accuracy of the ephemeris for the computed period, Sir John Herschel speaks in the highest terms. "Tell Mr. Stratford," says

he, "that his beautiful ephemeris seems to be quite as good as observation itself; it is quite delightful to run down the columns of it with the places deduced by observation." Mr. Stratford's voluntary and arduous exertions, however, did not terminate there; for he has just completed the *final* ephemeris alluded to in the report of last year, which includes the planetary disturbances during the time that the comet was expected to have been visible in either hemisphere, but which the Superintendent proposes to extend, so as to embrace the latest observations in the southern hemisphere.

The last subject which the Council has to notice is the appropriation of the Medal, which has been awarded to Professor Rosenberger for his investigations relative to Halley's comet. Of the labours of M. Rosenberger in various branches of physical and practical Astronomy, it is needless here to speak much at length, since they must be well known to all those who have attended to such subjects. Of his special claim to the medal of the Society for his investigations above mentioned, the President will state the grounds, and justify the appropriation, in his address to the meeting, which will immediately follow the reading of this report; and the Council feel assured that their decision will meet the approbation of the Members at large.

Titles of Papers read before the Society, between February 1836 *and February* 1837.

1836.

Mar. 11. An Account of Experiments made for ascertaining how far Reflecting Telescopes are suited to Micrometrical purposes: with a description of a mode of illuminating all kinds of Telescopes. By Mr. Thomas Grubb, of Dublin.

The Fourth and concluding portion of the Report on the new Standard Scale of the Society, drawn up at the request of the Council. By F. Baily, Esq. V.P. of the Society.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, in January and February 1836.

April 8. Account of an Observatory constructed at the Mauritius, by Captain J. A. Lloyd. Communicated by Sir John Herschel.

Extract of a Letter from Mr. Babbage to Mr. Baily, relative to the application of a Camera-Lucida to a Telescope mounted equatorially, and governed by a Clock.

Extract of a Letter from Sir John Herschel to Mr. Baily, dated Feb. 3, 1836, relative to the appearances of Halley's Comet.

1836.

On the Instruments used by Dr. Halley at the Royal Observatory, Greenwich. By Professor Rigaud.

*** Part of this paper read.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, in the month of March 1836.

May 13. Conclusion of Professor Rigaud's paper on the Instruments at Greenwich in the time of Dr. Halley.

A Letter from Dr. Kaiser, Director of the Observatory at Leyden, to the President, dated April 20, 1836, expressing an intention of forwarding some Observations of Halley's Comet and Lunar Occultations to the Society.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, in the month of April 1836.

June 10. Extract of a Letter from Sir John Herschel to Mr. Baily, dated Feb. 27, 1836, on the present appearance of *γ Virginis*.

Observations of Halley's Comet, made at Bedford. By Capt. Smyth, R.N.

Observations of the Solar Eclipse, May 15, 1836, by various observers; including Observations of the Annulus, made at four different places.

*** At the close of this paper, Mr. Baily gave a verbal account of the remarkable phenomena observed at the formation and dissolution of the annulus. Sir Thomas Brisbane and Mr. Henderson, who were present at the meeting, confirmed the statement made by Mr. Baily.

Observed Opposition of *Jupiter* and of *Juno*, in January 1836, and of *Vesta*, in March 1836, at the Royal Observatory of Edinburgh. By Professor Henderson.

Observed Occultations and Eclipses of *Jupiter's* Satellites at Edinburgh, September 1835 to April 1836. By Professor Henderson.

Observed Occultations at Ashurst, from January to July 1836. By Mr. Snow.

Meridian Observations of Halley's Comet, on the 10th and 11th of October, 1835, at Blackheath. By Mr. Wrottesley.

A List of Occultations observed at Breslau, February 1832 to March 1836. By Captain Boguslawski, Director of the Observatory.

Occultations observed at Mr. Wrottesley's Observatory, Blackheath, in May 1836. Communicated by Mr. Wrottesley.

Observations of Stars with the Moon, at Hartwell, in the year 1835. Communicated by Dr. Lee.

Example of a Graphical Method of finding the Longitude

1836.

from an Observed Occultation. By Capt. Lloyd, R.N. Communicated by Sir John Herschel.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, the Observatory at Cambridge, and Mr. Wrottesley's Observatory at Blackheath, in May 1836.

Nov. 11. Extract of a Letter from Mr. Maclear to Capt. Beaufort, accompanied by the original Circle and Transit Observations of Halley's Comet, made at the Cape Observatory, since January.

A Catalogue of the Right Ascensions of 1318 Stars, observed at Mr. Wrottesley's Observatory, Blackheath. By Mr. Wrottesley

***** The Report on this Catalogue, made by a Committee, and adopted by the Council, was read at the meeting.**

On the Projections of Maps and Charts, and on the Construction of the Hour-lines of Sun-dials. By Professor Littrow.

On the Formulæ for the Computation of Precession. By M. Mattheus Valente do Couto, Director of the Observatory at Lisbon.

Notice of a forthcoming work on the Measures of Double Stars. By Professor Struve.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, and at the Observatory of Cambridge, from June to October 1836.

Dec. 9. On a remarkable Phenomenon that occurs in Total and Annular Eclipses of the Sun. By Mr. Baily, V.P. of the Society.

***** At this meeting there was laid on the table, for the inspection of the members present, a small portable Floating Collimator, made by M. Amici.—There was also laid on the table a drawing or representation of several Shooting Stars that were observed at Plymouth, from the 11th to the 14th of November last.**

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, and at the Observatory at Cambridge, in November 1836.

1837.

Jan. 13. On the Longitude of Capt. Lloyd's Observatory at the Mauritius. By Capt. Lloyd.

Observations of Halley's Comet at the Cape of Good Hope. By Sir John Herschel.

Observations of the Solar Eclipse, May 15, 1836, at Ormskirk. By Mr. Dawes.

Observations of the Solar Eclipse, May 15, 1836. By Professor Bessel. Translated from the *Astronomische Nachrichten*, No. 320, by Mr. Galloway.

Continuation of Researches into the Value of the Mass of *Jupiter*. By G. B. Airy, Esq.

On Meteoric Stars observed at Plymouth by various observers.

1837.

Observations of Stars with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory at Cambridge, in December 1836.

List of Public Institutions, and of Persons, who have contributed to the Society's Library, &c. since the last Anniversary.

Lords Commissioners of the Admiralty.
American Philosophical Society.
Society of Arts.
Royal Asiatic Society of London.
Asiatic Society of Bengal.
Editor of the Athenæum Journal.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences of Brussels.
Society for the Diffusion of Useful Knowledge.
Academy of Sciences of Dijon.
Honourable East India Company.
Royal Society of Edinburgh.
Institution of Civil Engineers.
L'Académie Royale des Sciences de l'Institut de France.
Le Bureau des Longitudes de France.
Le Depôt Général de la Marine de France.
Royal Geographical Society.
Geological Society of London.
Society of Geneva.
Linnean Society of London.
Royal Academy of Sciences of Lisbon.
Royal Society of London.
Imperial Academy of Sciences of St. Petersburg.
Royal College of Physicians.
Zoological Society of London.

G. B. Airy, Esq. Ast. Roy.
M. Arago.
Professor Argelander.
F. Baily, Esq.
Professor Bessel.
Sir W. Betham.
Professor Bianchi.
M. Biot.
W. R. Birt, Esq.
M. Cacciatore.
M. Cerquero.
S. H. Christie, Esq.
M. Daussy.
A. De Morgan, Esq.
Professor Encke.
Mr. Epps.
Dr. Forster.
Professor Gruithuisen.
M. Gruyer.
Capt. B. Hall.
Professor Hansen.
H. Harvey, Esq.
J. Herapath, Esq.
S. Holehouse, Esq.
M. le Baron de Humboldt.
G. Innes, Esq.
Dr. Kaiser.
J. Lamont, Esq.
Dr. Lee.
Capt. Lloyd.

J. W. Lubbock, Esq.
S. Mackintosh, Esq.
J. Millard, Esq.
M. Albert Montemont.
Professor Mossotti.
Rev. M. Nicholson.
Professor Quetelet.
E. Riddle, Esq.
Professor Rigaud.
M. de la Rive.
J. Rowbotham, Esq.
Professor Santini.
Professor Schumacher.
Professor Slavinski.
Lieutenant Stratford.
R. Taylor, Esq.
Rev. C. Turnor.
J. Williams, Esq.
Rev. W. Whewell.

(The President then addressed the Meeting on the subject of the award of the Medal, as follows:—)

I have to announce to you, Gentlemen, that your Council have awarded the Society's Medal to our foreign associate, Professor Rosenberger, of Halle, for his elaborate calculations relating to the return of Halley's Comet. Permit me to point out to you the general train of considerations which have induced your Council to fix upon this matter as one of the greatest importance to Astronomy, and to select this astronomer as fully entitled, by his labours upon it, to the highest mark of our respect.

The science of Astronomy, Gentlemen, is pre-eminently one ot calculation and prediction—calculation of the past, and prediction of the future. The object of the first is, to extract laws and numerical elements from the phenomena that have occurred: the object of the second is, to apply these laws on the assumption of their generality, and these elements on the assumption of their invariability, to the phenomena that will occur; in order to ascertain, by comparing predicted with observed results, any error that may have been committed in these fundamental assumptions. And the history of Astronomy, in all the various forms which the science has taken, constantly presents to us, either the struggle of reducing laws and elements to agreement with new phenomena, or the anxious search for some hitherto neglected causes of discordance, or, finally, the triumph of finding that assumptions were well founded, and that the agreement of prediction with observation is sufficiently exact. The last of these it has been our good fortune to witness. We have seen a comet whose last appearance it is probable that no living man can distinctly recollect—whose period exceeds the limits of ordinary life—whose path extends into spaces far beyond any which in other parts of physical astronomy we have need to consider—we have seen it return within a day of its computed time, and have traced it through the heavens, describing nearly the path which had been laid down for it on our charts. I confess that the sight of this strange body, and the contemplation of the uniformity of the law which has guided its motions, and of the acquaintance with that law, and the power of tracing its effects,

which man has acquired, have been to me a source of intense pleasure. And I doubt not that the same gratification has been experienced by every astronomer who has been accustomed to regard his sublime science, on the one hand as the most severe exercise of the intellect, and on the other hand as the study which leads most certainly to a knowledge of the general laws of the universe.

There are, however, other points of view in which the subordination of this body to the general law of gravitation is extremely interesting. It is not merely that its period is long and its orbit extensive, but that it is a body of different kind, and moving in an orbit of very different proportions, from any of those which mankind had been accustomed to regard as belonging to our system. The most striking consideration is that derived from contemplation of the nebular hypothesis : a theory which, if not certain, is plausible, and accounts, in a most remarkable degree, for the phenomena that seemed to require some single cause to explain their general similarity. Is it true that this system of sun, planets, and satellites, was once a nebula, whose slow rotation and gradual condensation at length formed a number of bodies, bearing in their form and motions no trace of their original state? And is it true that comets are detached portions of nebula, which the want of mass has saved from the extreme degree of condensation that the planets have experienced; which, by the attraction of our sun, have at first been made to describe parabolas; and which, in some instances, perhaps from the effects of resistance when our system abounded with uncondensed nebular matter, have been made to return in orbits of limited extent? If these things be true, then I say that the subordination of these bodies to the law of gravitation is a most striking fact. They are a link, at the same time connecting the past with the present state of our system, and its present state with the state of those curious bodies which we find dispersed in all parts of the heavens. And their obedience to the law of gravitation affords a very strong presumption that this law has been unaltered since our system was one nebular chaos; and that it now holds in the nebulæ which preserve their state yet unaltered.

Be this as it may, the consideration of the form of this comet's orbit suggests a different train of ideas. Comets were once regarded as monsters, as prodigies, as having no relation whatever to the order of things either celestial or terrestrial. The discovery of Newton, more especially when it was followed out by Halley, placed them in a different rank. Of unknown origin, but bearing in their appearance the marks of a foreign race, amenable to the great law of Nature, but setting at nought the customs (if I may so speak) of the other revolving bodies--the gipsies, as I may term them, of the solar system—the very singularity and strangeness of their motion seem to hold out the prospect of rendering to science some service, which the uniformity and similarity in the motions of the planets render them incapable of giving to us. And how are these wild

bodies to be disciplined to our service? They are to be sent forth as spies; they are to go in directions in which no planets move; they are to explore spaces in which no other bodies are known to exist; and they are to return, bringing us an account, such as the physical astronomer can read, of the forces to which they have been subjected, and of the nature of the spaces through which they have passed. Have the anomalous motions of *Uranus* caused some astronomers to suspect the existence of a large planet beyond him? Then may we hope that Halley's or Olbers' comet will, in some revolution, feel its effect while far beyond our sight, and will return to our eyes still bearing, in its disturbed motions, a trace of the perturbations which it has undergone. Has it for ages past been conjectured that some matter exists in the planetary spaces, which in time may sensibly affect the motions of the most dense bodies? Then will the comparative insignificance of the comets be more likely to feel its effects.

I am speaking here, Gentlemen, of what may be hoped for the future; but in this I am fully borne out by the history of the past. There was a time, before the struggle of the rival theories of vortices and gravitation had terminated, when writers had learned to express the laws of gravitation in the language of vortices, and (in spite of the confusion of ideas which, when thoroughly examined, it was found to involve) to persuade themselves that the mutual attraction of many bodies could be explained by vortices. It was by the obvious impossibility of explaining the sun's attraction on comets moving in all directions, and to all distances, that the system of vortices was finally shaken, and the truth of gravitation established. And I do not hesitate to place in the same rank the discovery of resistance from the motions of Encke's comet. If it be objected that (so far as we can at present see) Halley's comet offers no trace of such resistance, a plausible ground of difference is not wanting. It is near the aphelion that the effects of resistance would be most sensible: the aphelion of Encke's comet is near the plane of the ecliptic, while that of Halley's comet is far from it; and if resisting matter be condensed (as the appearance of the zodiacal light leads us to imagine) near the plane of the ecliptic, then may the former of these comets experience considerable retardation, while the latter is almost unaffected by such a force. This, however, is precisely an instance of the points on which we may hope to derive information, from studying the motions of Halley's comet.

These motions are to be studied by applying, with the utmost accuracy, the best theory that we possess, and by then examining the differences which may be found to exist between the motions thus computed and the motions observed. It cannot be too strongly urged upon you, that this is the only way in which new points of physical law have been made out, and that it is the only way in which even the mathematical theory of established physical laws has been made in any degree accurate. Every lunar or planetary irregularity of importance has been discovered by observation before it has been explained by the established theory of the day; and in

many cases, nothing but a firm confidence in the accuracy of the comparison of the observations with a full developement of the previous imperfect theory, would have led the mathematician to extend his investigations so far as to form a more complete theory. The determination of the progression of the moon's apse, the history of the great inequality of *Jupiter* and *Saturn*, and that of the acceleration of the moon's motion, will sufficiently support me in this statement. With reference to the comet, the first step in the application of the theory must be, to investigate, from preceding observations, the elements of its orbit at the succeeding appearance; and the next step must be to render these elements immediately comparable with observation, by forming from them an ephemeris. And this leads me to make a few remarks on ephemerides in general. In this country, Gentlemen, it has been too much the custom to regard an ephemeris as a mere matter of convenience, serving only to assist in finding the body in the heavens. That this is one of its uses cannot be denied; but it is the lowest of its uses. To instance the most valuable periodical, with reference to planetary astronomy, that has ever been published, I mean our own *Meridian Ephemeris* (a publication of which this Society as a body, as well as various of its members as individuals, have just reason to be proud) —why is the right ascension of *Uranus* given to hundredths of seconds of time, when we know that it is four seconds in error? why is the polar distance of *Saturn* expressed to tenths of seconds of arc, when we know that it is incorrect to the amount of twenty seconds? The reason is, that, by calculating from the tables with the utmost accuracy (whatever the faults of these tables may be), we have from every observation an accurate comparison of the assumed theory with the observed fact; and we are thus in train for discovering the exact quantity of the correction which the elements require, or the exact amount of the unexplained inequalities (the *residual phenomenon*, to use the happy term of Sir John Herschel), for which new causes must be sought.

To any one who is impressed with the spirit of the foregoing remarks, it will be evident that unity of method through the whole series of observations, from the very earliest that are considered trustworthy, to the latest that the time has allowed us to make, is quite indispensable. The same original elements must be employed through the whole. In some cases, as in the instance of Halley's comet at the late appearance, where a small alteration in some of the elements produced great and irregular changes in the apparent places, it may be desirable to use for comparison an ephemeris approaching as near as possible to observation; but as the elements adopted for such an ephemeris admit of immediate comparison with the elements as computed, this proceeding forms no departure from the general rule. It is desirable also, for the avoidance of irregularities in the mathematical part, that the same mode of calculation (as far as possible) be used throughout. I am happy to find the same ideas with respect to other bodies expressed so clearly in an addition to the *Berliner Jahrbuch* for 1838 (by Encke, I believe),

that I cannot refrain from quoting them here: "For *Ceres*, *Pallas*, and *Juno*, it is absolutely necessary that a comprehensive investigation should be made; an investigation which may embrace all the observations extant, and which may not simply serve for the finding of these bodies, but may be considered as a firm foundation on which future investigations may be erected."

On the history of this comet it will not be necessary to detain you long. When Newton had shewn, in the instance of the comet of 1680, that comets move in parabolas in consequence of the attraction of the sun, and had given a method for discovering, from three observations of a comet, the elements of its orbit, Halley (probably the only person in the world who was competent to the work) exerted himself to apply this method to all the comets of which he could find observations. The result was a table of the elements of 24 comets, inserted in his *Astronomiæ Cometicæ Synopsis*, which is printed in the *Philosophical Transactions*, No. 297 (March 1705). This work was intended as the precursor of a larger, which, I believe, never appeared. He mentions that the labour of years had been employed in forming this table; that his principal object was to give the means of determining whether any future comet was the same as any of those in his list; and that he published the table at once, lest it should be lost by his death, and no other person should be able to restore it. The same paper contains his general table of parabolic motion, "dedicated," as he says, "to posterity, and which will last as long as the science of Astronomy shall exist." In examining the elements of the comets, he remarked the striking similarity between those of 1531, 1607, and 1682; and, in spite of an inequality in the intervals, he fixed upon these comets as the same, observing that the perturbations which *Jupiter* caused in the motions of *Saturn*, were such as to justify the supposition that its attraction might fully account for very large differences in the successive periods of this comet. At a later time, when the *Synopsis* was reprinted in his *Tabulæ Astronomicæ*, he remarked in the additions some still earlier appearances; which, though not described so fully as to permit of calculation of an orbit, yet bore distinctly the traces of the retrograde motion and general elements of the comet of 1682. And in the perfect confidence that the comet was really periodic, he now published tables for motion in a very long ellipse.

In 1757, when the approaching return of the comet excited general interest, Clairaut undertook to compute the alteration of its elements caused by the attraction of *Jupiter* when near to the comet. He speedily found that it would be necessary to consider the action of that planet, even when the comet was in the most remote parts of its orbit; and after computing this, he found that the effect of *Saturn's* action could not be neglected. When we consider that the analytical methods of treating the perturbations of the moon and planets were but lately introduced; that these methods required considerable alteration for application to comets; and that it was necessary to abandon integrations for quadratures;

—we must allow that the boldness of this attempt deserves the applause which it has received. In a part of the immense calculations, Clairaut was assisted by La Lande, and (it is said) by a lady, Madame Lepaute. In his work (*Théorie du Mouvement des Comètes*), some particulars of the details of computation are given. The calculations, it appears, were made entirely on printed skeleton forms; the quadratures, in their most critical parts, were checked by graphical constructions. The result as to the time of perihelion passage (announced two months before the comet was visible in France) was less than a month in error. The effects of perturbation on the node, inclination, &c. were computed afterwards.

In the present century, the calculation of the perturbations of the comet was proposed as a prize question by the Academy of Turin. The prize was awarded to the Baron Damoiseau: the details of his calculations are printed in the *Turin Memoirs* for 1817. From this time, at intervals, the subject has been taken up by several different mathematicians. Burckhardt, in the *Connaissance des Temps* for 1819, gives the elements which he obtained from Flamsteed's observations in 1682, and Messier's in 1759. The prize which the French Academy offered for the computation of the perturbations, was awarded to M. de Pontécoulant, of whose paper an *abregé* appeared in the *Connaissance des Temps* for 1833, and to which some additions were made in the volumes for 1837 and 1838. This paper has lately been published at length in the French Memoirs. In Germany, Rosenberger and Lehmann have separately made extensive calculations relating to the same subject.

I remark with pain, that from the age of Halley to the present time, no Englishman's name appears in connexion with the theory or the calculations relating to this or, indeed, any other comet. I can, however, congratulate the Society that our country, as represented by one of our members, has endeavoured, though late, to make such amends as time permitted and circumstances most urgently demanded. The ephemeris of the comet in preparation by the Superintendant of the *Nautical Almanac*, will be found, I trust, the most extensive in its plan, and the most accurate in its execution, that the world has ever seen. Valuable as it will be to the physical astronomer of the present day, it will, I hope, be much more so to those who shall discuss the observations of the year 1911. With this remark I shall conclude the history of the comet, and shall proceed to notice the methods used by different computers of the perturbations.

Halley made no calculation of the effect of disturbing forces, confining himself to the remark that *Jupiter's* attraction would, upon the whole, increase the comet's velocity, and would, therefore, increase its periodic time: a conclusion which La Lande professes his inability to understand, but which, nevertheless, is perfectly correct.

Clairaut, when he undertook to compute the comet's return, was in possession of the methods which he had used in his lunar

theory, and, probably, was not acquainted with any other. For, though Euler's general theory of the variations of elements was written, it had not yet been printed; and Lagrange's memoir on the same subject was not produced till several years afterwards. For the calculation, therefore, of the comet's perturbations, Clairaut sets out with the same differential equation for the reciprocal of the radius vector, which, till within a short time, has been universally adopted in lunar theories. Taking the solution (which can be expressed by elliptic terms, and terms depending on the disturbing force under one sign of integration), he rejects every thing depending on the square of the disturbing force, and transforms the expressions into others in terms of the excentric anomaly. He also employs, for the time, the same expression as that in his lunar theory, transformed in a similar manner. The latter requires the integration of the previously found integrals, affected with certain multipliers. As the general integration, even in the first step, was impracticable, he was compelled to resort to quadratures. But as this method is extremely laborious, he adopted it for 90° only of excentric anomaly before and after perihelion (that is, to the extremities of the minor axis). To diminish the tediousness of the numerical calculations, he suggests an expansion of the various terms depending on the planet's place by powers of the comet's excentric anomaly, and gives some formulæ for the purpose; but it does not appear (so far as I can discover) that he has used them himself. For the more distant parts of the orbit, he considers separately the effect of the planet on the sun and on the comet; and the former appears to be taken into account merely by assuming that the comet's motion will be nearly the same as if it were attracted to the centre of gravity of the sun and planet by a mass equal to the sun only, placed in that point. In the calculation of the latter some small terms are neglected. The process appears to me to require most careful examination before it can be pronounced to be sufficiently exact.

The next method, in the order of time, for computing the perturbations of comets, was that contained in a paper by Lagrange, printed in the 10th volume of the old series of *Savans Etrangers*. It is essentially founded on the method of variation of elements (the idea of which is introduced analytically, by the variation of the constants entering into the solution of a simple equation, in order to adapt the same form to the solution of a complicated equation). For the parts of the orbit near the sun, the integrations must be effected by quadratures; for the parts distant from the sun, an approximate solution is obtained by supposing the mass of the sun and planets collected at their centre of gravity, and expressions are given by the quadration of which the process is made accurate. It is also shewn, that without considerable error expressions may be found for these last-mentioned terms which will be integrable. The methods of this memoir are the basis of all which have since been employed.

The perturbations of the comets of 1807 and 1811 were dis-

cussed by Bessel and Argelander in separate treatises. Their method is that of the variation of elements, with the same abbreviation of the application of quadratures, when the comet is very distant, as that pointed out by Lagrange. The shape, however, in which the expressions are put, is better adapted for use than Lagrange's. These are the works referred to by all the German writers.

Damoiseau's method is nearly that of Lagrange. He has assumed elements for 1759, without any precise statement of his authority. The method of quadratures is applied throughout, proceeding by equal intervals of excentric anomaly. In his great memoir, the effects of only *Jupiter*, *Saturn*, and *Uranus*, are considered; but, in the notice in the *Connaissance des Temps*, 1832, the effect of the earth also is computed. The perturbations are computed from 1682 to 1835. In the first revolution, the elements are altered once; in the second, they are altered at every 30° of excentric anomaly. These alterations, as will afterwards appear, are not sufficiently frequent.

Pontécoulant's method is that of Lagrange, using quadratures throughout. The perturbations are first computed from 1682 to 1759. Burckhardt's elements are employed for these epochs: a major axis corresponding, as well as could be judged, to the first interval, being used from 1682 to 1759; the other elements of 1682 being used from 1682 to 1720; and those of 1759, from 1720 to 1759. The major axis, being thus corrected for 1759, the computation is continued to 1835, correcting these elements (in calculations of co-ordinates, &c.) at 300° (centesimal) of excentric anomaly; again at 325°, again at 350°, and again at 375°. The planets whose effects are estimated are *Jupiter*, *Saturn*, *Uranus*, and the *Earth* for a short time near the perihelion of 1759. I ought not to omit, that Pontécoulant has given abstracts and corrections of this paper in the *Connaissance des Temps* for 1833 and 1837; and that, in the volume for 1838, he has instanced Lagrange's method of integration for the distant parts of the orbit, and has shewn that it is sensibly in error.

In No. 287 of the *Astronomische Nachrichten*, is a paper by Dr. Lehmann. This mathematician has adopted, for Halley's comet, the method of quadratures by equal intervals of time. He gives a most elaborate and interesting statement of the different steps of his computations. With the view of ascertaining whether Halley's comet suffers any sensible resistance, he has computed the perturbations backwards to the appearance of 1607, and has compared the elements thus found with the observations of Longomontanus and Harriot, as discussed by Bessel. He has also computed the perturbations in the two last periods, but obtains results differing sensibly from those of other computers. The defect in the calculations appears to be that pointed out by Rosenberger, namely, that the elements are not changed often enough. It is, however, impossible for us to deny that great praise is due to this memoir.

I now come to M. Rosenberger's labours. I shall not here

mention them in the order in which they appeared, but in the order in which they relate to the comet.

In the *Astronomische Nachrichten*, No. 196, is the calculation of the elements of the orbit at the appearance of 1682, from the observations of Flamsteed, Hevelius, De la Hire, and Picard. An approximate set of elements is used for computation of places corresponding to the time of each observation; and these places are affected with the perturbations produced by all the planets. Equations of condition for changes of the elements being formed, and the observations compared with the computed places, a set of corrected elements is obtained, undoubtedly possessing great accuracy. In the expression of each, a term is introduced depending on any possible error in the major axis. It is remarkable that the results agree better with Halley's than with Burckhardt's.

In No. 180 of the same work is a similar investigation for the elements at the appearance of 1759, from the observations of Messier, Maraldi, Cassini de Thury, Bradley, Hell, Darquier, La Caille, Lulofs, Le Seur, and Jacquier. This, as well as the above-mentioned paper, contain critical remarks on the methods used by the observers, the relative value of the observations, &c. They are, undoubtedly, among the most complete papers of the kind that have ever appeared.

In the *Astronomische Nachrichten*, No. 250, is Rosenberger's investigation of the perturbations of the comet from 1682 to 1759. He begins with the expressions which suppose equal intervals of time to be used for the quadratures, and (in nearly the same manner as Damoiseau and Pontécoulant) transforms them into others implying equal intervals of excentric anomaly. But in this change a new consideration is introduced, which appears to have escaped other writers, and to which I wish particularly to call your attention. The formula expressing the relation between the differential co-efficient with respect to the time, and that with respect to the excentric anomaly, is not the same in a disturbed and in an undisturbed orbit; and, though the difference is generally insensible, it is sensible when the elements, from the proximity of a large disturbing body, are changing rapidly. This happens in both of the revolutions between 1682 and 1835. This consideration is not needed when multiplication is performed with the numerical intervals of time, but it is necessary when multiplication is performed with the numerical intervals of excentric anomaly. The effects of all the planets are estimated in the following manner: To 30° of excentric anomaly, the perturbations are computed, referring the comet's place to the sun; from 30° to 60° the perturbations of *Mercury*, *Venus*, the *Earth*, and *Mars*, are computed, referring the comet's place to the centre of gravity of the solar system; and from 60° to 300°, those of *Jupiter*, *Saturn*, and *Uranus*, are conducted in the same manner. From 300° to 330°, and then to 360°, the steps of the operations in the first part of the orbit are retraced. This paper is extremely valuable; for its explanation of the different points of the general theory; for its addition to the

accuracy of theory; and for the explanation (without the immense tabular details, which, nevertheless, it would be desirable to possess) of the order of calculations. A trifling error (regarding the application of precession) is corrected in No. 276.

In No. 276 is Rosenberger's account of his computation of the perturbations from 1759 to 1835. These are suspended at 300° of excentric anomaly, for the following reason. The perturbations produced by *Jupiter*, after this time, are so excessively great that a very accurate knowledge of the comet's orbit is necessary to calculate them well. They do not affect in any material degree the time of perihelion passage for 1835; but they will greatly affect that of 1911. For the mere purposes of general prediction, therefore, Rosenberger has taken Damoiseau's results for the last 60° of excentric anomaly; and the perturbations thus found, though not so accurate as they can be made since our knowledge of the comet's actual path, will, nevertheless, possess great accuracy. In this paper Rosenberger points out, that the effects of *Venus* and other planets, neglected by other computers, produce several days' difference in the time of the comet's return.

In No. 288 is a memoir in reply to Dr. Lehmann, which, though it adds nothing to the prediction of elements, is, nevertheless, most valuable. Rosenberger points out numerically the importance of the addition which he has made to the theory; and also the importance of frequently changing the elements in the computation. In some instances, in the first revolution, the elements were altered for every 2° of excentric anomaly. The effect of these hitherto neglected cautions, in computing the perturbations through only 16° of excentric anomaly, is an alteration of four days in the periodic time.

I have endeavoured, Gentlemen, to call your attention to the importance of observations of comets, on account of the singularity of their nature; the connexion which they seem to establish between the past and present states of our system, as well as between our system and the more distant sidereal bodies; the extent of the paths which they describe, and the unusual direction in which they move. I have pointed out that these observations cannot be made useful to the advancement of physical science, without a complete and accurate application of the theory which we now possess; and I have mentioned the advantage of unity of method through the whole extent of the computations. In a very few words I may point out how far M. Rosenberger has satisfied these demands. He has used the theory of perturbations in its most accurate form, and in one of its most delicate parts he has introduced an important correction. He has computed the perturbations of all the elements, for the first as well as the second revolution; Damoiseau and Pontécoulant having only computed the change of mean motion and epoch for the first revolution. He has included in his calculations the effect of several planets which had been totally omitted by other mathematicians. He has not only computed the perturbations, but has also, from the observations of different astronomers at the

former appearances, investigated the elements at those times, by a process of the most accurate kind. So complete are the whole of these computations, that if names were taken, not from the discoverers of these bodies, or from those who conjecture their identity, but from those who, by accurate calculations on a uniform system, combine the whole of our information relating to them, we should call this body, not Halley's, but Rosenberger's comet. I may add that, in the various memoirs to which I have referred, the theory connected with the computations is so well stated, that the study of these papers will be found most useful to the future investigator. I trust, Gentlemen, that it is unnecessary for me to say another word in defence of the adjudication of this year's medal.

(The President then, addressing the Foreign Secretary, continued thus:—)

Captain Smyth,—Transmit this medal, on the part of the Royal Astronomical Society of London, to M. Rosenberger. Assure him that his skill and his labour, though not imitated, are appreciated in this country. Say to him, that we trust he still retains the intention of extending his calculations to periods preceding and following those which he has already discussed. And convey our best wishes for his health and happiness, for the enjoyment of his well-deserved reputation, and for the vigour which may enable him to extend it by other investigations, as delicate as that already made, on other subjects of equal importance in the system of the universe.

The Meeting then proceeded to the Election of the Council for the ensuing year, when the following Fellows were elected.

President: Francis Baily, Esq. V.P. and *Treas.* R.S. F.L.S. & G.S. and M.R.I.A.—*Vice-Presidents:* George Biddell Airy, Esq. M.A. F.R.S. *Astronomer Royal;* George Dollond, Esq. F.R.S.; Thomas Galloway, Esq. M.A. F.R.S.; Edward Riddle, Esq.—*Treasurer:* John Lee, Esq. L.L.D. F.R.S.—*Secretaries:* George Bishop, Esq.; Augustus De Morgan, Esq.—*Foreign Secretary:* Captain W. H. Smyth, R.N. F.R.S. & A.S.—*Council:* Captain F. Beaufort, R.N. F.R.S.; Rev. James Challis, M.A. *Plum. Prof.*, Cambridge; Rev. George Fisher, M.A. F.R.S.; Davies Gilbert, Esq. F.R.S. L.S. & G.S.; Henry Harvey, Esq.; Lieut. Manuel J. Johnson; Rev. Robert Main, B.A.; Lieut. Henry Raper, R.N.; Lieut. William S. Stratford, R.N. F.R.S.; John Wrottesley, Esq. M.A.

ROYAL ASTRONOMICAL SOCIETY.

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The following communications were read: —

I. Results of the Observations of the Sun, Moon, and Planets, made at Cambridge Observatory in the years 1833, 1834, and 1835. By Mr. Airy, Astronomer Royal.

The author states, that he was led to undertake the further reduction of these observations, from a consideration of the general completeness of the series, the excellence of the instruments used, the care that had been bestowed on the reductions, and the form in which they are published (admitting in every case of easy verification). The results of every individual observation were in all cases compared with the places computed from tables; and the tables used for this purpose were the same throughout. He then describes the methods used in the calculations of this paper. The observations have been divided into groups of such an extent as seemed to make it *à priori* probable that the magnitude of the errors of the tables would not vary much through each group; or, at least, that, supposing the errors (independent of accidental irregularities) to be expressed in a function of the time, the term depending on the square of the time should not be sensible. The limits of the groups actually employed are given in another part of the paper. It is necessary to use short groups where the errors are rapidly and irregularly changing (as in the case of Mercury); but in other instances (as for Saturn or Uranus) a great number of observations may be combined together. But for the purpose of obtaining in all cases several separate groups, which shall be affected in different ways and in different degrees by any possible error in the planet's radius vector, the groups actually employed, though (it is presumed) sufficiently long, are shorter than would be absolutely required by the condition mentioned above. The limits of the groups being fixed, the mean of all the individual errors of the tables in right ascension (as published in the Cambridge Observations) is taken, and the mean of the days of observation (omitting fractions of a day) is taken; and this mean error in right ascension is considered as a very accurate value of the error in right ascension corresponding to the mean day. In like manner, the mean of the individual errors in north polar distance is considered as a very accurate value of the error in north polar distance corresponding to the mean of the days of those observations. The mean day for right ascension and the mean day for north polar

distance are not always the same; but one or both is altered, so as to fix upon the same adopted day for both. Thus a series of very accurate normal errors of the tables in right ascension and polar distance is obtained.

The author then states, that the next step was to find the errors in geocentric longitude and ecliptic polar distance corresponding to these errors in right ascension and north polar distance. For this purpose he has used a set of tables (which he proposes to publish in an Appendix to the Greenwich Observations), giving for every 1° of N. P. D. and for every 4^m of Æ (to about 6° on each side of the ecliptic) four numbers, P, Q, R, S, such that

$$\text{Error in longitude} = P \times \text{error of R. A. (in time)} + Q \times \text{error of N. P. D.}$$
$$\text{Error in ecliptic P. D.} = R \times \text{error of R. A. (in time)} + S \times \text{error of N. P. D.}$$

For the small planets, when beyond the limits of these tables, the geocentric longitude and ecliptic polar distance are computed from the right ascension and north polar distance on the mean day, and also from the same right ascension and north polar distance affected with their tabular errors; and the differences of the two sets of results (in longitude and in ecliptic polar distance) are taken for the errors of the tables, on that mean day, in longitude and ecliptic polar distance.

The next step is, to convert the errors of geocentric longitude and ecliptic polar distance into errors of heliocentric longitude, of ecliptic polar distance, and of projection of radius vector on the plane of the ecliptic. One of these three must remain undetermined (from the normal errors of any one mean day); and the author has chosen to leave the error of the projection of radius vector undetermined, because in general it may be supposed small, and because the comparison of the results from the errors of two or more mean days will give it with great facility. The general formulæ are (putting

R for radius vector of planet,	L for planet's heliocentric longitude,
Δ for planet's distance from earth,	λ for planet's geocentric longitude,
r for earth's radius vector,	l for earth's heliocentric longitude),

$$\text{Error of Helioc. long.} = \frac{\Delta \times \cos \text{geoc. lat.}}{R \times \cos \text{helioc. lat.} \times \cos(\lambda - L)} \times \text{error of geoc. long.}$$
$$+ \frac{\tan(\lambda - L)}{R \times \cos \text{helioc. lat} \times \sin 1''} \times \text{error of proj. of radius vector.}$$

$$\text{Error of Helioc. E. P. D.} = \frac{\Delta \times \cos \text{helioc. lat.}}{R \times \cos \text{geoc. lat.}} \times \text{error of geoc. E. P. D.}$$
$$- \sin \text{helioc. lat.} \times \cos. \text{helioc. lat.} \times \tan(\lambda - L) \times \text{error of geoc. long.}$$
$$- \frac{r \times \tan \text{geoc. lat.} \times \cos(L - l)}{R^2 \times \cos(\lambda - L) \times \sin 1''} \times \text{error of proj. of radius vector.}$$

In this state the formulæ are used for the four small planets. For the large planets, however, they are reduced to the following:—

$$\text{Error of helioc. long.} = \frac{\Delta}{R \times \cos(\lambda - L)} \times \text{error of geoc. long.}$$

$$+\frac{\tan(\lambda - L)}{R \times \sin 1''} \times \text{error of proj. of radius vector.}$$

$$\text{Error of heliocentric E. P. D.} = \frac{\Delta}{R} \times \text{error of geocentric E. P. D.}$$

which the smallness of their latitudes allows us to adopt.

For the Moon, the errors of Æ and N. P. D. are converted into errors of longitude and E. P. D., without any further calculation.

The author then states, that all the errors of Æ printed in the Cambridge Observations have been increased (before making the above calculations) by $+0^s,14$, which, for several reasons, he considers as a good expression for the mean error of right ascension in the catalogue of stars used for correcting the clock. He also mentions, that the errors in N. P. D. for 1833 and 1834 have been corrected for the error in refraction produced by the error of barometer reading described in the Addendum to the Observations for 1834.

Upon the final results he makes the following remarks. The errors of the solar tables seem to shew a gradually accelerated motion of the sun. This, however, is not certain: it is clear that the errors of the solar tables are much inferior to those of the planetary tables. The results of the observations near the equinoxes seem to shew a shifting of the equinoctial point, such as cannot be explained by an error in nutation, and of the cause of which the author professes his ignorance. The results for the planets can hardly be applied at present to the improvement of their tables, except in the case of Mercury and Venus,—the successive oppositions of Mars falling too near to the same part of his orbit, and the portion of the orbits of Jupiter, Saturn, and Uranus, described in three years, being too small. The extent, also, through which the small planets can be observed, and compared with the tables at each opposition, is so small, as scarcely to allow of more than one or two good determinations of error.

The author points out an error in the computations of the Meridian Ephemeris for Pallas, in 1835, which was discovered in the course of these calculations.

With regard to the Moon, the author considers that the results may at once be used for correction of the coefficients of the principal inequalities, and of the epochs of their arguments. He points out the discordance in the observations of different limbs of the Moon in Æ, depending, probably, on the habit of the transit-observer.

The paper is terminated by a tabular arrangement of the results.

The first part is headed, "Errors of the Tabular Geocentric Places of the Sun and Planets," and contains the following columns: 1. Number of observations of Æ in each group; 2. Number of observations of N. P. D. in each group; 3. Mean day of each group; 4. Mean error of tables in Æ; 5. Mean error of tables in N. P. D.; 6. Error of tables in longitude; 7. Error of tables in ecliptic polar distance.

The second part is headed, "Errors of the Tabular Heliocentric Places of the Planets," and contains the following columns: 1. Day; 2. Error of tables in heliocentric longitude; 3. Weight of this determination; 4. Error of tables in heliocentric E. P. D.; 5. Weight of this determination. For the errors in heliocentric longitude, the weights are computed by the formula—

$$\frac{\text{Error of geoc. long.}}{\text{Error of helioc. long.}} \times \sqrt{(\text{number of observations in A.R.})};$$

and those for the errors in heliocentric E. P. D. by the formula—

$$\frac{\text{Error of geoc. E. P. D}}{\text{First term of error of helioc. E. P. D.}} \times \sqrt{(\text{number of observations in N. P. D.})};$$

which formulæ are supposed to be sufficiently accurate. For the four small planets, each error is expressed in the following form (taking, as an instance, Pallas, 1834, March 3):—

$$\begin{array}{ll} \text{Error in heliocentric longitude} & +158''\cdot77-24790''\times\delta R \\ \text{Error in heliocentric E. P. D.} & -64\cdot21+24341\ \ \times\delta R, \end{array}$$

where δR is the error in the projection of the radius vector on the plane of the ecliptic, expressed by a fraction of the earth's mean distance from the sun. For the large planets, the term depending on δR is omitted in the error in heliocentric E. P. D.

The third part is headed "Errors of the Tabular Places of the Moon," and contains the following columns: 1. Day; 2. Errors of tables in longitude; 3. Error of tables in E. P. D.

The number of observations, of which the results are given in this paper, is as follows:—

Of the Sun,	440	observations in A.R.,	451	in N. P. D.
Mercury,	100	——	89	——
Venus,	309	——	309	——
Mars,	143	——	143	——
Vesta,	29	——	25	——
Juno,	14	——	26	——
Pallas,	31	——	30	——
Ceres,	26	——	28	——
Jupiter,	188	——	171	——
Saturn,	151	——	153	——
Uranus,	143	——	146	——
the Moon,	249	——	249	——

II. On the Effects of Errors of Adjustment in Azimuth and Level, when the Transit Instrument is used for finding Time and Latitude, by Observations on the Prime Vertical. By Lieutenant Raper, R. N.

The author observes that, in this application of the transit instrument, minute accuracy in the adjustments referred to has hitherto been considered as essential, or, at any rate, important; but, from the investigation into which he has been led on the subject, he finds (and it is a valuable peculiarity of the method) that extreme nicety in the adjustments is not requisite to the production of good practical results.

In finding the latitude by this method, the error in the result

is proportional, not to the simple power of the error in azimuth, but to the square of that error; therefore, from every correction of the azimuthal position of the instrument, the error in latitude is diminished in a rapidly increasing proportion. The error in the resulting latitude is the same as that of the level.

In finding the time, the latitude is required only approximately; and the effect of the error in level is generally too small to require attention.

Mr. Raper investigates formulæ for the effects of any given errors in the adjustments, and shews, by examples, that good practical results are compatible with very imperfect adjustments; and remarks, in conclusion, that if the intervals are measured correctly, and the instrument reversed in observing, "it is evident, from the smallness of the effects of the errors of adjustments, and the rapid convergence of the method, that a very inferior instrument is capable of affording very good results."

III. Determination of the Longitude of the Edinburgh Observatory, from all the correspondent Observations of Moon-culminating Stars, made at Greenwich and Edinburgh, between Oct. 18, 1834, and May 27, 1836. By Mr. Riddle.

The author gives, as the mean of 44 correspondent observations, $12^{m}\,44^{s}{\cdot}5$, for the longitude of the Observatory, west of Greenwich, which is $0^{s}{\cdot}9$ in excess of that adopted in the *Nautical Almanac*. The greatest difference from the mean result is $17^{s}{\cdot}1$, and the least $0^{s}{\cdot}0$.

The following is a synopsis of the data and results:

Number of stars observed	Number of observations.	Resulting difference of meridians.			
		Mean.	Nearest the mean.	Least.	Greatest.
		m s	m s	m s	m s
1	7	12 45·17	12 47·3	12 37·9	12 53·9
2	15	43·73	45·3	27·4	60·8
3	12	43·17	43·1	28·1	56·7
4	10	46·16	45·8	35·6	52·9

IV. Occultation of Mars by the Moon, on February 18, 1837. By Mr. Baily, President of the Society.

"Mars was seen to touch the moon at $8^{h}\,45^{m}\,2^{s},6$, and totally disappeared at $35^{s},6$. It was first seen to reappear at $9^{h}\,54^{m}\,0^{s},6$, and totally emerged at $26^{s},6$. The times here recorded are the true *sidereal* times. The telescope made use of was a 3½ feet refractor, with a power of 80. Place of observation, N. Lat. 51° 31′ 26″, and W. Long. from Greenwich $30^{s},0$. The planet appeared of a fine *yellow* colour, both at its ingress and egress. No projection was observed."

"The atmosphere had been very cloudy, with much rain and wind during the whole of the evening. But a little after 10 o'clock (mean time), or about ¾ of an hour before the occultation took place,

the clouds dispersed and left a beautifully clear sky; at the same time discovering a most brilliant *aurora*, or rather stream of light, which extended from the horizon in the west, through the zenith, almost down to the horizon in the east. This light was of a fine rose colour, being most vivid and brilliant in the western horizon; and, as it approached the zenith, was evidently impaired by the strong light of the moon, then near her full, and high on the meridian. The *aurora* appeared very steady, and unaccompanied by any coruscations. It lasted till about ½ past 11 o'clock, and wholly disappeared before midnight. By subsequent accounts which have been received, it was seen in the western parts of Ireland, and in Scotland."

V. Occultation of Mars by the Moon. By Sir T. M. Brisbane, Bart.

Place of observation, Makerstoun, Lat. 55° 34′ 45″ N. Long. 10^m 4^s west of Greenwich.

	h.	m.	s.	
First contact	10	32	9,9	Mean time at the place.
Disappearance	10	32	19,9	
Reappearance	11	44	26,5	
Last contact	11	44	36,5	

Very clear and fine at immersion and emersion.

"On quitting the Observatory after the immersion, at about 10^h 35^m, I was struck by a most extraordinary red appearance in the sky, extending as far west as the constellation of Orion, and as far east as that of Leo. It was principally north of the moon—none of it very near her; and might have been at least 10 degrees in breadth, of almost a deep red colour. I found all the servants out, looking at it with surprise and astonishment. It was followed next day by snow, rain, and a gale of wind. Observed also a very beautiful lunar halo, of nearly 5′ in diameter: the colour principally orange; but it did not last many minutes."

VI. Occultations. By Mr. Snow.

1837. Jan. 13. *o* Piscium.

	True sid. time. h.	m.	s.	
Disappearance at the moon's dark limb ...	5	29	20,1	... good.
Reappearance at the moon's bright limb ...	6	19	32,3	... a little uncertain.

Place of observation, Lat 51° 25′ 16″ N. Long. 18^s west of Greenwich.

1837. Feb. 18. Mars.

	True sid. time. h.	m.	s.
First contact with moon's dark limb	8	44	36
Disappearance	8	45	14
Reappearance	9	53	2
Last contact with moon's bright limb	9	53	32

Every thing very favourable.

The observations were made with a 45-inch refractor, adjusted on Mars with a power of 80.

Place of observation, Mr. Snow's Observatory at Ashurst, Lat. 51° 15′ 58″ N. Long. 1^m 9^s,3 west of Greenwich.

"In addition to the above interesting phenomenon, a very remarkable Aurora Borealis became visible at 10 o'clock. It commenced with a very fine deep red arch, extending over the zenith from the east and west, and slanting away, as it were, at an altitude of about 30° towards the horizon in the N.E. and N.W. Its appearance was magnificent, although the moon was very bright, and not far from the meridian. When it was first noticed, the clouds that were driving off towards the east were deeply tinged with the red colour. After undergoing some changes, difficult to describe, it grew fainter at twenty minutes before 11. A white arch then appeared in the usual situation of the Aurora, giving out white streamers at a few minutes before 11. This latter arch was not well defined. At ten minutes after 11 the Aurora had almost entirely disappeared, excepting a reddish patch between Arcturus and Regulus, which in ten minutes more was no longer to be seen. After this the wind, which during the day had been at S.W., shifted to the N.W., with a cloudless sky, and remained so until the morning. The stars, however, became gradually very ill defined; and during the whole of the next day and night there fell unceasing torrents of rain, with a gale of wind from the south-west."

VII. Stars observed with the Moon, at the Royal Observatories of Greenwich and Edinburgh, and the Observatories of Cambridge and Blackheath (Mr. Wrottesley's), in Jan. and Feb. 1837.

1837.	Object.	Apparent Right Ascension.			
		Greenwich.	Edinburgh.	Cambridge.	Blackheath.
		h m s	h m s	h m s	h m s
Jan. 1	κ Virginis...	14 4 11,35	...	...	...
	λ Virginis...	14 10 16,89	...	...	...
	Moon II..	14 23 36,48	...	..	...
11	λ Piscium...	23 33 42,91	...	23 33 42,89	...
	Moon I....	23 50 22,18	...	23 50 21,09	...
13	ι Piscium...	...	0 54 28,86	...	...
	Moon I....	...	1 29 21,96	1 28 55,35	...
	γ^1 Arietis ...	...	1 44 35,42	1 44 35,37	...
	ξ^1 Ceti	...	2 4 22,06	2 4 21,79	...
14	γ^1 Arietis ...	...	1 44 35,31	1 44 35,63	...
	ξ^1 Ceti	...	2 4 21,65	2 4 21,85	...
	Moon I....	2 17 27,62	2 17 53,50	2 17 26,94	...
	ε Arietis ...	2 29 34,18	2 29 34,14	2 29 34,19	...
	σ Arietis ...	2 40 12,30	2 40 12,38	2 40 12,26	...
16	g Arietis ...	...	3 14 41,77	...	...
	η Tauri......	...	3 37 48,46	...	...
	Moon I....	...	3 58 35,91	...	...
	υ^1 Tauri......	...	4 16 34,23	...	...
	τ Tauri......	...	4 32 28,59	...	...
17	υ^1 Tauri......	..	4 16 34,18	...	...
	τ Tauri......	...	4 32 28,59	...	...
	Moon I....	...	4 51 42,11	...	...
	β Tauri......	...	5 16 0,12	...	...
	ζ Tauri......	...	5 27 55,16	...	...
21	ϕ^2 Cancri *...	...	8 16 56,05	...	...
	Moon II..	...	8 31 54,76	...	...
	ζ^1 Cancri ...	...	8 45 54,16	...	...

* *Middle of double star.*

1837.	Object.	Apparent Right Ascension.			
		Greenwich.	Edinburgh.	Cambridge.	Blackheath.
		h m s	h m s	h m s	h m s
Jan. 23	η Leonis ...	...	9 58 27,25	...	...
	Moon II..	...	10 11 20,98	...	..
	ρ Leonis ...	...	10 24 14,24	...	...
	k Leonis ...	...	10 37 47,74	...	...
Feb. 12	Moon I....	...	3 40 5,36	3 39 36,48	...
	ω^2 Tauri	...	4 7 43,22	4 7 42,93	...
	υ^1 Tauri......	...	4 16 33,96	4 16 33,54	...
13	ω^2 Tauri......	4 7 43,22	4 7 43,27	4 7 43,10	...
	υ^1 Tauri......	4 16 33,92	4 16 33,98	4 16 33,80	...
	Moon I....	4 32 44,03	4 33 12,61	4 32 42,92	4 32 43,79
	ι Tauri......	4 53 21,82	4 53 21,78	...	4 53 21,73
	n Tauri......	...	5 9 29,68	5 9 29,59	5 9 29,76
14	ι Tauri......	4 53 21,73	...	4 53 21,87	4 53 21,86
	n Tauri	5 9 29,62	5 9 29,71	5 9 29,55	5 9 29,72
	Moon I....	5 27 16,24	5 27 45,60	5 27 15,15	5 27 16,04
	C Tauri......	5 43 5,71	5 43 5,58	5 43 5,66	5 43 5,81
	η Geminor..	...	6 5 2,97	6 5 2,97	6 5 3,11
15	η Geminor..	...	6 5 3,22	...	...
	Moon I....	...	6 23 4,44	...	...
	ι Geminor..	...	6 33 55,14	...	...
	τ Geminor..	...	7 0 46,44	...	...
16	ι Geminor..	...	6 33 54,69	...	...
	τ Geminor..	...	7 0 46,42	...	...
	Moon I....	...	7 18 7,92	...	...
17	β Geminor..	7 35 21,03	...	7 35 21,08	7 35 21,36
	φ Geminor..	7 43 31,95	...	7 43 31,75	7 43 32,06
	Moon I....	8 11 26,58	8 11 54,83	8 11 25,59	8 11 26,78
	δ Cancri ...	8 35 25,90	8 35 25,97	8 35 26,03	8 35 26,33
	ρ^4 Cancri ...	8 45 54,48	...	8 45 54,28	8 45 54,57
18	δ Cancri ...	...	8 35 25,85	8 35 26,24	...
	ρ^4 Cancri ...	...	..	8 45 54,55	...
	Moon I....	9 3 13,68	9 3 40,41	9 3 12,99	9 3 13,68
	λ Leonis ...	9 22 25,94	...	9 22 26,03	9 22 25,83
	ο Leonis ...	9 32 27,89	...	9 32 28,03	9 32 27,92
19	λ Leonis ...	...	9 22 26,01	...	...
	ο Leonis ...	...	9 32 27,96	...	...
	Moon I....	...	9 53 11,07	...	...
	γ Leonis ...	...	10 11 0,11	...	...
	ρ Leonis ...	...	10 24 14,71	...	...
20	γ Leonis ...	...	...	10 10 59,95	...
	ρ Leonis ...	10 24 14,83	...	10 24 14,78	...
	Moon II..	10 42 21,75	...	10 42 21,19	...
	χ Leonis ...	...	...	10 56 37,61	...
	ι Leonis ...	...	...	11 15 26,76	...
21	χ Leonis ...	...	...	10 56 37,64	...
	ι Leonis ...	...	11 15 26,58	11 15 26,77	...
	Moon II..	...	11 28 48,56	11 28 24,14	...
	β Virginis...	...	...	11 42 13,63	...
24	ζ Virginis...	13 26 24,30	...	...	...
	Moon II..	13 47 26,14	...	...	...
	κ Virginis...	14 4 13,17	...	...	...
25	Moon II..	14 37 51,70	...	...	...
	20 Libræ ...	14 54 33,07	...	...	...

VIII. Occultations observed at Blackheath, from April 1836, to February 1837, both inclusive. By Mr. Wrottesley.

This list contains the true sidereal time of the phenomenon, the name of the observer, the description of the telescope, and the power used for each observation.

A new Telescope, made by M. Plössl, of Vienna, and belonging to Mr. Talbot, was exhibited: it is called a *dialytic* telescope, and is formed on the principle announced by Mr. Rogers, in the third volume of the *Memoirs* of this Society, page 229.

Mr. Baily laid on the table a specimen of some delineations of the Stars in the vicinity of the North Pole, made with Steinheil's *Astrograph.* This is an ingenious instrument, by which a person can see, *at the same time,* the stars and the copy of them; and thus, by simultaneous observations, obtain the greatest accuracy. The instrument itself has not yet been introduced into this country.

ROYAL ASTRONOMICAL SOCIETY.

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The following communications were read: —

I. On the Declinations of the principal Fixed Stars, deduced from Observations made at the Cape of Good Hope, in the years 1832 and 1833. By Mr. Henderson.

These observations were made with a mural circle, constructed by Mr. Thomas Jones, which is in all respects similar to those at the Royal Observatory at Greenwich, except that in the circle at the Cape the axis revolves on Y bearings placed at its extremities. The diameter of the circle and focal length of the telescope is exactly 6 feet; the object glass is 4 inches in diameter; and of the two eye-glasses, magnifying 94 and 123 times, the latter was almost invariably used,—the lower power being employed only on faint objects. Besides the usual system of 1 horizontal and 5 vertical fixed wires, there was another horizontal one movable by means of a micrometer screw: this latter, however, was seldom used.

From a careful examination of the instrument, Mr. Henderson found either that the divided limb was of an elliptical figure, or that the divisions were not equidistant; or that both these circumstances affected the instrument. He also ascertained that the pivots of the axis were of an elliptical or irregular shape, that the centre of the pivot was not concentric with the centre of the divided limb, and that the supports of the axis underwent occasional variations of position upon the pier. But Mr. Henderson overcame all these sources of discordance by proper correctional methods of reduction and of observation. The details of his examination of the circle are contained in the 8th volume of the *Memoirs* of this Society.

In reducing the observations, Mr. Henderson made use of M. Bessel's table of refractions, given in his *Tabulæ Regiomontanæ*; and adopted 9″,25 as the coefficient of lunar nutation, and 20″,50 as that of aberration. And, in deducing the declinations from the observations, he followed, as far as possible, the method of M. Bessel, explained in Section VII. of the *Konigsberg Observations*, except in the general use of upper and lower culmination of stars near the Pole, which, for obvious reasons, could not be so universally adopted at the Cape. He, therefore, had recourse to those stars, most frequently observed on both sides of the zenith, whose zenith distances were less than 45°. They are 39 in number, and the observations amount to 1374. By different comparisons of each of these stars he obtained the variations of position of the circle. Previously, however, he investigated the value of the

numerical coefficient in the thermometrical factor for correcting the refraction in Bessel's table; it being desirable to ascertain whether his value would suit the climate of the Cape, and the meteorological instruments in use there. The details of this investigation are then given; whereby it appears that a slight modification of Bessel's factor is required: but so small in amount, and so little affecting the results, that Mr. Henderson deemed it expedient to retain the original value unaltered.

Mr. Henderson then enters into the investigation of the probable errors of the results of the observations at different zenith distances, —that is, the combined effects of errors of observation, of reading off, of the estimated position of the instrument, of refraction, and of the reduction to the assumed epoch. The result of this inquiry is, that the probable errors of a single observation, from observations of certain stars at a given zenith distance, are as under, viz.:—

Prob. error = 0″,44	by 272 obs. of	3 stars at	0° 19′	zenith distance.
= 0 ,52	— 411	— 12	— 24 20	——
= 0 ,78	— 293	— 15	— 63 21	——
= 1 ,03	— 218	— 11	— 76 5	——
= 2 ,02	— 102	— 11	— 83 12	——

Near the zenith, these probable errors appear to be as small, if not smaller, than those found in Europe; but, nearer the horizon, they seem to be greater at the Cape than in Europe; which is, no doubt, occasioned by the greater tremor of the stars at low altitudes in the warm climate of the Cape.

The next subject of inquiry was the comparison of the *direct* with the *reflected* observations; and from which it appears that the Cape circle exhibited the same peculiarity which has been noticed in several others,—namely, that the reflected observations give *greater* zenith distances than the direct ones. The reflecting fluid was mercury contained in a wooden trough, about 15 inches long, 4 inches broad, and filled to the depth of about half an inch. Mr. Henderson remarks, that the reflected observations are not affected with a greater fluctuating error than the direct ones.

Having, by means of these various methods and corrections, obtained his horizontal and zenith points, Mr. Henderson ultimately deduces the latitude of the place equal to 33° 56′ 3″,25 from 503 observations above, and 435 observations below, the pole, of 27 stars whose double transits were comprised between 27° and 85° of zenith distance. From the same observations he endeavoured to ascertain whether M. Bessel's coefficient of refraction required any alteration, as above alluded to.

Being thus in a condition to deduce the declinations of the stars from the observations, the next point to be ascertained was, whether the reduced observations made the distance between the south pole and the equator exactly 90°. The polar point had been ascertained; but, in order to determine the exact position of the equator, a continued series of solar observations, both in right ascension and declination, was requisite. This could not be accomplished during Mr. Henderson's stay at the Cape, for reasons which he has satis-

factorily explained. He, therefore, had recourse to the declinations of the fundamental stars given by Bessel, Struve, and Airy, for determining the position of the equator. On thus comparing the declinations of the stars between the parallel of $-10°$ and $+10°$, he found that the Cape declinations about the equator are too northerly by

0″,57 from comparison with Bessel,
0 ,16 from comparison with Struve,
0 ,11 from comparison with Airy.

The mean of these ($=0''{,}28$) is the quantity by which the Cape *direct* observations make the distance too great between the pole and the equator. The catalogue of declinations then follows.

II. Observations on Halley's Comet. By Mr. Maclear, Astronomer at the Cape of Good Hope.

Mr. Maclear, for his extra meridian observations, employed a 45-inch achromatic, by Dollond, $3\frac{1}{4}$ inches aperture, mounted on an equatorial stand, and, for its size, an excellent instrument. In some of the first observations, the ring micrometer was used; but the greater part were made with the wire micrometer, comparing the comet with stars passing through the same field. The time was taken from a chronometer carefully compared by coincident beats with the transit clock. There are determinations of the comet on 10 days by the ring micrometer, and on 32 with the wire micrometer.

From Feb. 16 to May 5, 1836, the comet was observed on the meridian with the mural circle of 6 feet and the transit of 10 feet. There are complete determinations with both instruments on 33 days and 10 days on which it was observed with one of the instruments. Such a series is unprecedented in cometary astronomy. The stars, 117 in number, used by Mr. Maclear and by Sir John Herschel in their comparative observations, have all been determined afresh with the meridian instruments. A catalogue of these stars, with the Astronomical Society's constants for their reduction to apparent places, accompanies this communication.

The faintness of the comet compelled Mr. Maclear to resort to various expedients. On the 22d of April and 5th of May, a strong thread was stretched across the sliding eye-piece of the transit, so as to pass pretty closely to the wires, and parallel to them. By letting in the light, this thread was made to coincide with the wires; and, then, stopping out the light, the comet was visible, and its passage over the strong thread seen and noted. This mode of observing a faint object may at times be of great utility.

As Mr. Maclear found the comet too faint to be observed with the circle telescope when illuminated, he drew a line with ink on the field glass of his eye-piece, which is so near the wires as to shew very little parallax between this ink-line and the wires. The observation of the comet was made on the ink-line, and the dif-

ference between this and the fixed wire of the telescope measured by the micrometer before stirring the eye-piece.

On March 7th, the comet getting still fainter, Mr. Maclear removed his spider web-lines, and substituted strong silver wires. He now brought the upper edge of the fixed wire to bisect the comet; and then, keeping the circle unmoved, bisected following stars with the micrometer wire, getting one star to the north, and another to the south of the comet, when possible. Precautions were taken to muffle the lamps, and keep the observer in a state of darkness.

The observations were continued with the silver wires, until the comet became scarcely visible from faintness and moonlight, when the silver wires were removed, and fine Bermuda cobwebs inserted. In the subsequent observations of the comet, the bisection was but indifferent, as the fine wires required illumination. A little light was let in occasionally to see their position; and this was often repeated, so that the observation was a sort of trial and error. As the first varnish was bad, Mr. Maclear procured fresh varnish from the composition of an old Electrophorus dissolved in spirit of wine, which answered very well. To make the last observation, on May 5, a black patch on the eye-piece was brought on the comet, and then the distance of the patch from the wires measured by the micrometer: a contrivance which he had adopted in some of the preceding observations.

Mr. Maclear confined his attention chiefly to the actual places of the comet, "knowing," as he says, "that the powerful instrumental means at the command of, and directed by the hand of, his distinguished neighbour, Sir John Herschel, would explore and gather every thing valuable relating to its physical construction." Mr. Maclear did, however, observe it frequently with a powerful reflecting telescope of 13½ inches aperture, made by the late Sir W. Herschel; and his memoir is accompanied by four sheets of drawings of the comet, executed apparently with great fidelity, certainly with great spirit, by Mr. Charles Piazzi Smyth.

*** His Majesty's Government has been pleased to order that the two papers here noticed be printed at the public expense.

III. Occultation of Mars by the Moon, February 18, 1837. By Professor Quetelet.

The observations were made at the Brussels Observatory (Lat. 50° 51′ 10″, 7 N. Long. 17^m 29^s,7 East of Greenwich), with an equatorial (focal length not stated) of 4 inches aperture, and power 144.

	h.	m.	s.	
First contact	11	14	36,1	Mean time at Brussels.
Disappearance	11	15	5,6	
Reappearance	12	25	5,0	
Last contact	12	25	29,0	

M. Quetelet states that the first and third observations were rather doubtful, but the second extremely satisfactory.

M. Quetelet notices the appearance of a brilliant aurora during the occultation.

IV. Description (illustrated by Drawings) of a Spring Level. By Mr. Donkin.

The author states that the principle of this instrument was suggested, several years since, by the sight of Hardy's Noddy. The Noddy, as is well known, consists of an adjustable weight carried by a vertical stalk, whose lower end is connected with its pedestal by a spring, forming a continuation of the stalk. When the pedestal is so placed that the stalk is vertical, if the stalk be disturbed from its vertical position the gravity of the weight tends to move it still further from its vertical position, while the elasticity of the spring tends to restore it to its vertical position. The force, therefore, which actually restores the stalk to its vertical position is the excess of the latter over the former. By sliding the weight higher on the stalk, the effect of its gravity is increased; and thus the force actually efficient in raising the stalk may be made as small as we please. The object of this adjustment, in Hardy's application of the Noddy, was to make its vibrations isochronous with those of the clock pendulum. In Mr. Donkin's application, the object is to make the restoring force as small as may be considered safe and convenient.

The instrument being in this state, suppose the pedestal to be tilted through a small angle, and consequently the bottom of the spring to be inclined through the same angle. The effect will be, that the force of the weight tends to press the stalk from its vertical position; and, therefore, as the restoring force is extremely small, it will cause the stalk to deviate through a large angle. If, then, a microscope be carried by the pedestal, in such a position that the relative motion of the stalk can be observed, that motion will give a very large measure of the inclination of the pedestal, and will, therefore, enable us to ascertain the amount of the inclination with very great accuracy. This is the principle of Mr. Donkin's instrument.

In the actual construction, the weight, instead of a ball (as in Hardy's Noddy), is a broad disk, vibrating within a box, so as to leave a very small passage for the air which is displaced by its vibration. This arrangement is made for the purpose of bringing the disk to rest in a short time. At the top of the disk is a small frame carrying a cobweb, which is observed through a roof of plate-glass with a micrometer-microscope. The metal used for the disk and spring (which are in one piece) is a composition of nickel and copper, which is found to be not magnetic.

The instrument, as first constructed by Mr. Donkin, was carried by a lever, whose end was raised by a screw with graduated head. The intention was that the screw should be turned till the cobweb, as seen in the microscope, should be brought to a determinate constant position, and that the graduations of the screw-head should measure the inclination. It appears better, however, to give up all idea of measuring by the foot-screws, and to effect the measure entirely by the micrometer of the microscope.

After the reading of Mr. Donkin's paper, the Astronomer Royal stated to the meeting that he had carefully examined the instrument, with the view of ascertaining its delicacy, and that he had come to the conclusion that it would exhibit as minute a quantity as the Greenwich mural circles in ordinary observations.

Mr. Francis Baily presented to the Society an original portrait of Professor Schumacher, of Altona, painted by Professor Jensen.

V. Stars observed with the Moon, at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of March, 1837.

Date.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
Mar. 13	τ Tauri	...	4 32 27,81	...
	Moon I. ...	...	5 6 18,79	...
	β Tauri	...	5 15 59,46	...
	C Tauri	...	5 43 5,26	...
14	β Tauri	...	5 15 59,47	5 15 59,45
	C Tauri	5 43 5,24	5 43 5,14	5 43 5,24
	Moon I. ...	6 1 41,49	6 2 11,16	6 1 40,35*
	ϵ Geminorum	6 33 54,47	6 33 54,50	6 33 54,72
15	ϵ Geminorum	...	6 33 54,44	...
	Moon I. ...	...	6 57 51,72	...
	ι Geminorum	...	7 15 36,35	...
	κ Geminorum	...	7 34 36,60	...
16	ι Geminorum	...	7 15 36,38	...
	κ Geminorum	...	7 34 36,62	...
	Moon I. ...	...	7 52 19,87	...
	ϕ^2 Cancri }	...	8 16 55,98	...
	double }	...	8 16 56,06	...
18	Moon I. ...	9 34 41,84	9 35 7,97	...
	π Leonis	9 51 36,98	...	...
	α Leonis	9 59 42,38	...	...
19	π Leonis	...	9 51 37,06	9 51 37,11
	α Leonis	...	9 59 42,27	9 59 42,66
	Moon I. ...	...	10 23 21,01	10 22 55,09
	k Leonis	...	10 37 48,26	10 37 48,49
	c Leonis	...	10 52 19,05	10 52 19,13
20	k Leonis	...	10 37 48,18	...
	c Leonis	10 52 19,30	10 52 18,99	...
	Moon I. ...	11 9 38,43	11 10 2,72	...
	υ Leonis	11 28 37,50	11 28 37,37	...
	β Virginis ...	11 42 13,58	11 42 13,50	...
22	η Virginis	...	...	12 11 35,41
	γ^1 Virginis	...	12 33 25,45	12 33 25,74
	Moon II. ...	...	12 44 21,09	12 43 55,89
	θ Virginis	...	13 1 32,12	13 1 32,36
	α Virginis	...	13 16 38,04	13 16 38,08
23	θ Virginis ...	...	...	13 1 32,40
	α Virginis ...	13 16 38,19	...	13 16 38,01
	Moon II. ...	13 31 31,21	...	13 31 30,43
	x Virginis ...	...	...	13 41 3,17
	κ Virginis ...	...	...	14 4 13,77
26	π Scorpii	15 49 1,25	...	...
	β^1 Scorpii	15 55 59,13	...	...
	Moon II. ...	16 13 6,83	...	...
	α Scorpii	16 19 26,32	...	...

* [Apparently 1s in *defect*. S.]

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *May* 12, 1837. No. 7.

The following communications were read: —

I. On Astronomical Refraction near the Horizon. By Mr. Henderson.

The results, mentioned in this paper, were obtained by Mr. Henderson from observations, made with the mural circle at the Cape of Good Hope, of the apparent zenith distances of stars which passed the meridian of that place at greater zenith distances than 85°, for the purpose of procuring additional materials for illustrating the subject of astronomical refractions near the horizon. The stars selected were those lying on both sides of the zenith; and, from the known declinations of the stars and the latitude of the observatory, the true zenith distances of the stars were computed: the differences between the true and observed zenith distances shewed the observed refractions. Five stars were chosen to the north, and eleven to the south, of the zenith: 73 observations were made of the former, and 113 of the latter. The refractions are computed in each case from the tables of Bessel and Ivory; those of the former being increased in the ratio of 1 to 1·003282: and the differences of each table from the observed refraction are set down against its respective observation. The general result shews that, except in the case of four or five stars, the tables of Ivory require less correction than those of Bessel. Mr. Henderson concludes his paper by remarking "that astronomers in the north-"ern hemisphere are now enabled to determine, from their own "observations, the refractions of southern stars as well as of "northern; the declinations of many of the southern stars having "been ascertained with the requisite accuracy."

II. Observations of Halley's Comet, made at the Observatory at Madras. By Mr. J. G. Taylor.

These observations, fourteen in number, were made on the meridian with the 5-feet transit instrument, and 4-feet mural circle; and are a valuable addition to the several observations that have been made, in the southern hemisphere, of this remarkable body. The observations extend from February 19 to March 21, 1836, both inclusive: and Mr. Taylor remarks, that the right ascensions may be depended upon to 1 second of time, and the declinations to 20 or 30 seconds of space; the extreme faintness of the comet

(which would not allow the wires to be illuminated) preventing more accurate results.

III. Observations of the Solar Eclipse of May 15, 1836. By Mr. Shearman.

These observations were made in Camden Street, Camden Town, with a 4-feet Gregorian reflector, of $5\frac{1}{2}$ inches aperture, and power of 65.

First observed contact, at	5^h	24^m	21^s	true sidereal time.
End of the eclipse	8	12	25	

These times were ascertained with a small transit instrument; and Mr. Shearman thinks that they are true within one second. The final separation of the moon from the sun's disc was exceedingly well marked, and the lunar irregularities could be observed quitting the apparent contact for an instant, after the true line of the two orbs might be pronounced to have separated. Mr. Shearman made several experiments on the effect of the sun's rays on various substances, which are given in detail.

IV. Observations of the Annular Eclipse of the Sun, May 15, 1836. By Lieut. C. Hopkins, R.N.

The place of observation was East Percy Street, North Shields; and the observations were made with a $4\frac{1}{2}$-feet achromatic telescope: the times were,

Beginning, or first contact	1^h	43^m	16^s	mean time.
End, or last contact	4	28	55	

Lieut. Hopkins states, that at $3^h\ 3^m\ 59^s$ the sun was seen all round, outside the moon; but he does not give the time of the commencement or end of the annular appearance, nor does he state how he corrected his chronometer.

V. On the position of the Ecliptic, as inferred from Observations made with the Cambridge Transit and Mural Circle, in 1835. By Mr. Airy, Astronomer-Royal.

The number of observations of the sun, used for this determination, is 114 transits and 129 polar distances, in each of which both limbs were observed. The method of treating these observations is similar to that already employed by Mr. Airy, in Vol. VIII. and IX. of the *Memoirs* of this Society. Tables are given, shewing the monthly means of the errors in the sun's place, and the equations of condition by which the results are obtained. From those expressions, it appears that the sun's place (as determined from observation) is north of the assumed ecliptic (whose inclination is that assumed in the *Nautical Almanac*, and the equinox that assumed in the catalogue of stars) by the quantity

$$-0''{\cdot}769 \cos \odot - 0''{\cdot}233 \sin \odot + 0''{\cdot}389$$

The first term shews, that the right ascension of the stars, by which the sun's right ascension has been determined, ought to be

diminished by $\frac{0''\cdot 769}{\sin \text{obl.}}$, or $\frac{0'',769}{15 \sin \text{obl.}}$, or $0^s,13$. The place of the equinox, thus defined, is intermediate between Bessel's and Pond's, but rather nearer to the latter. The second term shews, that the obliquity of the *Nautical Almanac* ought to be diminished by $0''\cdot 233$. The meaning of the third term is, that the sun moves (independently of perturbations) in a small circle, whose distances from the north pole of the ecliptic is $90^\circ - 0''\cdot 389$. Or, it may be expressed thus: the summer obliquity is greater than that of the *Nautical Almanac*, by $0''\cdot 156$; and the winter obliquity is less than the summer obliquity by $0''\cdot 778$.

On a former occasion Mr. Airy thought that the explanation of this anomalous term might be sought in the imperfection of the tables of refraction, or from an erroneous reading of the barometer at the Cambridge observatory. The latter, however, has been corrected in the present case; and Mr. Airy is therefore of opinion, that the refractions in Bessel's tables ought to be increased, not only to reconcile the observations of circumpolar stars, but also those of the solstices. On the whole he considers that we may, without risk of sensible error, increase the obliquity of the *Nautical Almanac* by $0''\cdot 10$; in which case the mean obliquity on January 1, 1835, will be $23^\circ\ 27'\ 38''\cdot 90$.

VI. On the Geographical Longitude of the Observatory of Leyden. By Dr. Kaiser.

This determination of the difference of longitude between Leyden and Paris has been deduced from 43 observations of 10 different occultations of stars by the moon, made at Leyden, and 17 other observatories, whose assumed differences of longitude from Paris are given in the paper: and it is remarkable, that there is not a single observation, out of the 43 adduced, that has been made at Paris; so that the ultimate result is, in fact, mixed up with any errors which may exist in the assumed differences of longitude. The observations at Leyden were made with a 7-feet achromatic telescope, by Dollond; and the times were determined by equal altitudes made with one of Troughton's sextants. The computations, from which the results are deduced, are then given; as also the several equations of condition for eliminating the errors; and the final result obtained is, that Leyden is situate $8^m\ 35^s,97$ east from Paris, the probable error of the mean being $0^s,19$.

VII. Description of a Pendulum Artificial Horizon, to be attached to a Sextant or Quadrant for the purpose of observing Altitudes by Day or Night at Sea. By Lieutenant Becher, R.N.

A small pendulum carries an arm nearly at right angles to it, springing from the point of suspension, at which the whole rests on an agate point. At the end of this arm is a vane, perpendicular to the plane of the pendulum and arm, and having its upper edge cut horizontal, which forms the visible horizon. This apparatus is

placed in a tube laid in the direction of the telescope, and attached to the sextant beyond the horizon glass; the point of suspension rests on an arch within the tube, the rest of the pendulum hanging below, the vane being at the end nearest the horizon glass, its upper edge bisecting the space in the tube.

A disc is placed in the lower half of the tube, between the fixed horizon glass and the vane of the pendulum, and close to the latter without touching it; in the upper edge of the disc is a small aperture or notch. A lens is placed in the end of the tube next the horizon glass, in order to shew distinctly the edges of the vane and this aperture; at the other end is ground glass.

The sextant being held with its plane vertical, the reflected image is brought down to the horizontal edge of the vane, and the sextant is then oscillated in its own plane till the edge of the vane is seen exactly fair with the upper edge of the disc, or just covering the aperture. If the vane be above the aperture, this is discovered by the light appearing through two small holes in the disc. As the line of sight may not be exactly horizontal, its *error*, which is constant, is found by comparing the altitude obtained with that shewn by another instrument.

The pendulum moves in a small cistern, containing oil, to diminish its vibrations; on turning up the face of the sextant to read off, the oil runs into another cistern. For observing at night, a small lamp is hung before the outer end of the tube.

On observing with the instrument in a steam vessel, the extreme error was 7′ 48″. The latitude, as found on board a small cutter in Sea reach by the altitudes of the moon and of *Jupiter*, agreed within 2′.

The error in observing is generally in excess.

VIII. Mr. Baily read an extract from a letter, announcing that M. Argelander had sent a paper to the Imperial Academy at St. Petersburg, relative to the motion of the solar system; in which it is stated that our system is moving to a point very near that which was supposed by the late Sir Wm. Herschel, viz.

Æ =	260° 50′	.... probable error	3° 28′
Dec. =	+31 7	.... ——	2 20

This result is deduced from the proper motion of 390 stars, all of which exceed 0″·1 per annum, as determined in his catalogue of 560 stars published in the year 1835.

*** Amongst the Presents this evening, the president mentioned the singular clock made by the celebrated Mr. John Harrison, which was presented to the Society by Mr. Barton of the Royal Mint, and who has promised to forward a description of the same.

IX. List of Moon-culminating Stars observed at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of April, 1837.

Date.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
April 9	Moon I. ...	...	4 41 47,10	...
	η Tauri	...	5 9 28,70	...
	β Tauri	...	5 15 59,04	...
10	η Tauri	...	5 9 28,86	...
	β Tauri	...	5 15 59,18	...
	Moon I. ...	...	5 38 26,31	...
11	μ Geminorum	...	6 13 5,65	...
	Moon I. ...	...	6 35 5,72	...
	δ Geminorum	...	7 10 23,12	...
12	δ Geminorum	...	...	7 10 23,27
	Moon I. ...	...	...	7 30 7,56
	φ Geminorum	...	...	7 43 31,39
14	Moon I. ...	...	...	9 14 44,61
	o Leonis	...	...	9 32 27,78
	π Leonis	...	...	9 51 37,08
15	o Leonis	9 32 27,62	9 32 27,29	...
	π Leonis	9 51 36,60	9 51 36,37	...
	Moon I. ...	10 3 33,20	10 3 58,58	10 3 32,35
	γ Leonis	10 10 59,85	10 10 59,64	10 11 0,04
	ρ Leonis	10 24 14,56	10 24 14,37	10 24 14,74
17	η Leonis	...	11 7 21,01	...
	τ Leonis	...	11 19 34,14	...
	Moon I. ...	...	11 37 5,03	...
	ι Virginis ...	...	11 56 55,71	...
	η Virginis ...	...	12 11 35,48	...
19	γ^1 Virginis ...	...	12 33 25,39	12 33 25,96
	δ Virginis ...	12 47 25,23	12 47 24,87	12 47 25,46
	Moon I. ...	13 10 5,96	13 10 31,62	13 10 5,37
	α Virginis ...	13 16 38,25	...	13 16 38,58
	ζ Virginis ...	13 26 25,10	...	13 26 25,20
20	α Virginis ...	13 16 38,48	...	13 16 38,39
	ζ Virginis ...	13 26 25,05	...	13 26 25,22
	Moon I. ...	13 59 44,21*	...	13 59 43,21
	Moon II....	14 1 57,21	...	14 1 56,41
	λ Virginis ...	14 10 19,59	...	14 10 19,63
	α^2 Libræ	...	...	14 41 53,88
21	α^2 Libræ	14 41 53,93	...	...
	Moon II....	14 55 10,93	...	...
23	Moon II....	16 54 55,60	...	...
	θ Ophiuchi ...	17 12 1,47	...	...
24	p Sagittarii .	17 37 19,68	...	...
	Moon II....	18 0 33,35	...	...
	φ Sagittarii...	18 35 29,39	...	...

* The correction $-0^s,03$ for defect of illumination has been applied.

REGULATIONS FOR THE LIBRARY.

I. That the President and Secretaries shall have the superintendence of the Library, and be a permanent committee for that purpose.

II. That a Book shall be kept in which shall be entered the title of every book borrowed, the name of the Fellow borrowing the same, and the date on which it is lent; which entry shall be signed by the person to whom the book is delivered. And, on the return of such book, the Assistant-Secretary shall insert opposite to such entry the date on which the book has been returned, and give in writing, if required, an acknowledgment of its return. And if, on the return of such book, the Assistant-Secretary shall perceive that it has sustained any damage during its loan, he shall make a note of the particulars, and report the same to the Committee.

III. That no book shall be lent or taken out of the Library before its presentation to the Society shall have been announced at one of the ordinary meetings; except during the vacation, when any member of the permanent Committee may give permission to that effect.

IV. That every book shall be returned, free of expense, on or before the expiration of a calendar month from the day on which it was borrowed; but if a Fellow is desirous of keeping it beyond that period, the term may be extended to another month, provided no other Fellow has applied for the loan of it.

V. That in case of any book, map, or other work, borrowed from the Library, being lost or damaged, the Fellow by whom it was borrowed shall be held responsible for the loss or damage; and if it belonged to any set, shall be held bound to make good the whole set to the satisfaction of the Council.

VI. That when any book shall not have been returned to the Library at the end of the month for which it was borrowed, it shall be the duty of the Assistant-Secretary to send by post a letter to the Fellow by whom it was borrowed, requiring its immediate return, or the renewal of the loan of it, and to report to the Council all failures in complying with such requisition.

VII. That every Fellow who shall have transgressed the third, fourth, or fifth regulation, or may refuse to comply with the terms therein contained, shall be precluded from borrowing any book from the Library without special permission of the Council.

VIII. That no person, who is not a Fellow of the Society, shall be permitted to borrow any book, or to have access to the Library, without permission in writing from one of the members of the permanent Committee.

Royal Astronomical Society, Somerset House,
May 12, 1837.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *June* 9, 1837. No. 8.

The following communications were read:—

I. On the non-existence of the star 42 *Virginis*. By Mr. Baily, President of the Society.

The star, which is designated by Flamsteed as 42 *Virginis*, has given a great deal of trouble to astronomers, from the circumstance of its not being now to be found in the position indicated in the British Catalogue. There is a star in Zach's Zodiacal Catalogue, called 42 *Virginis*, which was observed by him at Seeberg, and by Messrs. Barry and Henry at Manheim, and which is the star from which the position in the catalogue of the Astronomical Society has been deduced. But this star, when carried back by precession to the year 1690, although agreeing very well in declination, differs upwards of 3′ in right ascension from Flamsteed's star: and it is remarkable that this star also is not now to be seen. Sir John Herschel was the first to point out the non-existence of this star; since which time it has been in vain sought for at public and private observatories. The present Astronomer Royal has, at my request, repeatedly looked out for it; and although, in the Cambridge Observations, there are two or three very minute stars which are designated as 42 *Virginis*, yet the position of neither of them will agree, in right ascension and declination, either with Flamsteed or Zach. The truth I believe to be that the star does *not* exist; and that its insertion in the British Catalogue has arisen from some error in the computation of 31 *Boötis*, which was observed by Flamsteed on April 25th, 1693, at $11^h\ 33^m\ 18^s$. It will be seen, by a reference to the 2d vol. of the *Historia Celestis*, that, amongst other stars observed on that day, in the constellation *Boötes*, there were the three following, viz. at $11^h\ 15^m\ 4^s$, $11^h\ 15^m\ 40^s$, and $11^h\ 15^m\ 46^s$. These stars are not inserted by Flamsteed in the British Catalogue, but I have given them in my edition of that Catalogue, at the end of my Account of Flamsteed; being Nos. 1968, 1970, and 1971: and I have there stated (p. 584), "that all the stars in *Boötes* observed on that day have been reduced by Flamsteed, except these three; and I find, in the *original* observation-book, a marginal note by Flamsteed, that these three stars belong to the constellation *Virgo*, which is probably the reason why they were omitted." In consequence, afterwards, of some unaccountable error in determining the right ascensions of these stars, Flamsteed considered them to be the stars which he has designated as 22, 23, and 24 *Virginis*; but which stars consequently do not exist. This preliminary statement is necessary for explaining my reason for supposing that 42 *Virginis*

also does not exist. For, as this star is in nearly the same parallel of declination as the other three, which were presumed to be in *Virgo*, we may fairly assume that Flamsteed considered them all to be situate in that constellation; an opinion which is further confirmed by some marginal computations opposite this very star in the original manuscript book. Comparing, therefore, the *differences* of right ascension and declination of the 4 stars in *Boötes*, which were observed by Flamsteed, as above mentioned, and which are known to exist, with the *differences* of the 4 pseudo stars in *Virgo*, above mentioned, of which no other observations can be found in Flamsteed, than those above alluded to, and which stars cannot now be discovered, we have strong reason to believe that the introduction of these last-mentioned stars into the British Catalogue has arisen from error of computation. This will be more fully shewn by the following statement, where the numbers in the first column refer to the numbers in my edition of the British Catalogue:—

No.	Star.	AR.	Difference.	Dec.	Difference.
	The Four Stars in *Boötes* that do exist.				
		° ′ ″	° ′ ″	° ′ ″	° ′ ″
1968	Boötis	212 2 35		+9 51 55	
1970	...	212 11 35	0 9 0	9 40 0	0 11 55
1971	...	212 13 5	0 1 30	7 14 35	2 25 25
2000	31 ...	216 36 30	4 23 25	9 30 10	2 15 35
	The Four Stars in *Virgo* that do not exist.				
1748	22 Virginis	185 15 10		+9 54 0	
1751	23 ...	185 24 10	0 9 0	9 40 30	0 13 30
1752	24 ...	185 25 40	0 1 30	7 16 5	2 24 25
1785	42 ...	189 49 20	4 23 40	9 30 30	2 14 25

These differences in right ascension are almost identical, there being only a difference of one second in time in the last of them; which might easily have arisen from some correction for azimuthal or clock error, not now known. With respect to the declinations, I suspect that there is an error of 1′ in 24 *Virginis;* and that 7° 16′ 5″ should be 7° 15′ 5″: in which case they also would be identical. For, with respect to the declination of No. 1968, I would remark, that it has been deduced from the mean of 3 observations, made at an interval of ten years, and that if the difference on April 25th, 1693, were alone taken, they would be more in accordance. Besides, a difference of 30″ is very likely to have arisen in many of Flamsteed's detached computations, from the circumstance of his having, to that amount, altered, from time to time, the assumed latitude of the observatory, without making the requisite corrections, in all cases, in the declinations of the stars. My opinion therefore is, that the star, designated as 42 *Virginis* by Flamsteed, does not exist; and that the insertion of it in the British Catalogue has arisen from an erroneous computation of the observation above mentioned. But what is become of the star seen by Baron Zach at Seebeg, and by Messrs.

Barry and Henry at Manheim? Neither of these astronomers states the number of times that it was observed, nor are the dates of the observations given; so that we do not know whether they all saw it at the same time, or at different periods: and thus we are left without any clue to the solution of the difficulty. Zach has, in the *Monath. Corres.* for March 1804, p. 239, alluded to his own observation, and he calls the star "ausserst kleinen stern:" its right ascension corresponds very well with the star which Mr. Airy calls 42 *Virginis* (No. 1.), in the Cambridge Observations for 1834. But, as the declination will not agree with that of Messrs. Barry and Henry, it is highly probable that they observed a totally different star, and that thus the confusion has arisen. Flamsteed's number, however, should in future be discontinued, until the star be accurately identified.

II. A method of clearing the lunar distance from the effects of parallax and refraction. By Mr. Templeton.

Mr. Templeton's solution is of the class in which the true distance is found by means of corrections applied to the apparent one. But as, on account of the distortion of the discs from refraction, the arc of measured distance will not, in general, be strictly a portion of that joining the centres of the objects, Mr. T., in order to avoid the minute error which neglect of this consideration may sometimes introduce, proposes to clear, not what is commonly called the apparent distance of the centres, but that of the points whose distance is measured; and then, by applying the semi-diameters to that reduced distance, to find the true central distance.

It is hence necessary to apply a correction to the altitudes of the centres, found from observations in the usual way, to obtain the altitudes of the points whose true distance it is proposed to compute,—a correction which Mr. T. finds first approximately by means of a Table, and afterwards, if necessary, with greater correctness from the formula $c = \frac{s}{\sec \phi}$, where c is the correction, s the semi-diameter, and ϕ the angle at the object in the triangle whose angular points are the zenith, and the means between the true and apparent places of the objects.

Putting ☽ and ⊙ for half the sum of the true and apparent altitudes of the sun and moon, D for half the sum of the true and apparent distance, S for $\frac{☽ + ⊙ + D}{2}$, ☽′ and ⊙′ for the respective correction of the objects in altitude, and ϕ, ϕ' for the angles at the base of the triangle, whose angular points have been indicated above; then for computing ϕ and ϕ', Mr. T. gives the following convenient formulæ, which we are not aware have been noticed before:—

$$\tan^2 \frac{\phi}{2} = \cos S \ \sec (S - D) \times \sin (S - ☽) \operatorname{cosec} (S - ⊙)$$

$$\tan^2 \frac{\phi'}{2} = \cos S \ \sec (S - D) \div \sin (S - ☽) \operatorname{cosec} (S - ⊙)$$

And $\frac{☽'}{\sec \phi} + \frac{⊙'}{\sec \phi'}$ is the correction to be applied to the apparent, to obtain the true distance.

To facilitate the application of his formulæ in practice, Mr. T. has computed four tables, specimens of which are given in his paper. The first, which must be rather extensive, is a Table of triple argument (the arguments being the altitudes and apparent distance), and gives roughly the correction of the central altitudes adverted to above, and half the correction of distance; the corrections, when + being in red, and when — in black ink, in the manuscript. Hence are obtained the data for computing $\tan\frac{\varphi}{2}$ and $\tan\frac{\varphi'}{2}$, and consequently $\frac{☽'}{\sec\varphi} + \frac{☉'}{\sec\varphi'}$ approximately; and hence an approximate solution of the problem is obtained, and a more accurate result may be had by a repetition of the process, with the data corrected from the result of the first solution.

By the second table, log sec φ, and log sec φ', is taken by inspection with $\log\tan^2\frac{\varphi}{2}$ and $\log\tan^2\frac{\varphi'}{2}$ as arguments, so that φ and φ' themselves do not appear in the errors of computation.

The third table gives the correction of the parallax in altitude, and the fourth gives the difference between the refraction due to the altitude of the centre, and that of the point observed.

III. On the Node and Inclination of the Orbit of Venus. By Mr. Main.

The author having undertaken the correction of the elements of the orbit of Venus, from a comparison of the Cambridge observations of 1833, 1834, and 1835, and the Greenwich observations of 1836, with the Berlin Ephemeris for 1833, and the Nautical Almanac for 1834, 1835, and 1836, the magnitude of the errors in north polar distance naturally attracted his attention to the node and inclination of the orbit. The first portion of the paper contains an account of the values of these elements adopted by Lindenau and others, in which the author points out several discordances. The second part contains the method pursued by the author in ascertaining the corrections due to those elements.

The errors of the tables in heliocentric ecliptic north polar distances were furnished by Mr. Airy, and consist of 13 in 1833; 11 in 1834; 12 in 1835 and 14 in 1836.

By means of these errors and the formula

$$-\delta E = \frac{\sin 2\beta}{\sin 2I} . \delta I - \tfrac{1}{2} \sin 2\beta . \cot(l' - ☊)\, \delta ☊$$

in which E denotes the hel. ecliptic N. P. D.,
β — the hel. latitude,
l' — the hel. longitude,
☊ — the longitude of the node,
I — the inclination of the orbit.

the author forms 50 equations of condition for the determinations of δI and δ☊. He then applies the weights of the observations to the equations, and deduces, by the method of least squares, the

following corrections for the node and inclination used in the calculations of the Nautical Almanac, viz. :

$$\text{Correction of node} = -32''\cdot 9$$
$$\text{———— inclin.} = +\ 3\ \cdot 9$$

The author concludes his paper with a table shewing the effect of the substitution of these corrections, in altering the original errors of the Tables.

IV. Occultation of ω' Cancri by the moon, on December 24th, 1836, at Ashurst; and of c Tauri, on April 10th, 1837, at Dulwich. By Mr. Snow.

V. Occultation of Mars by the moon, on February 18th, 1837, at Marischal College, Aberdeen. By Mr. John Cruikshanks.

VI. Detailed Observations of the Solar Eclipse of May 15th, 1836, and Immersion of Jupiter's 1st Satellite, on August 20th, 1837, at Shooter's Hill. By Colonel Hodgson.

VII. Observations of the Solar Eclipse of May 15th, 1836, at Halifax. By Mr. J. Waterhouse.

VIII. Observations of the Solar Eclipse of May 15th, 1836, at Tranby. By Mr. Samuel Cooper.

IX. On Napier's Five Circular Parts. By Mr. R. Abbatt.

X. List of Moon-culminating Stars observed at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of May, 1837.

Date.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
May 9	Moon I. ...	...	...	7 7 9,99
	β Geminorum	...	...	7 35 20,04
12	Moon I. ...	9 43 29,60	...	9 43 28,92
	α Leonis	9 59 41,94	...	9 59 41,94
	γ Leonis	10 10 59,51	...	...
15	ξ' Virginis ...	...	11 36 54,05	...
	β Virginis ...	...	11 42 13,49	...
	Moon I. ...	...	12 2 33,53	...
	η Virginis ...	...	12 11 35,38	...
	γ' Virginis ...	...	12 33 25,69	...
16	η Virginis ...	12 11 35,35	12 11 35,27	12 11 35,57
	γ' Virginis ...	12 33 25,59	12 33 25,60	12 33 25,75
	Moon I. ...	12 48 26,76	12 48 51,80	12 48 26,02
	θ Virginis ...	13 1 32,52	...	13 1 32,52
	α Virginis ...	13 16 38,43	...	13 16 38,37
17	θ Virginis ...	13 1 32,43	...	...
	α Virginis ...	13 16 38,41	...	...
	Moon I. ...	13 36 48,26	...	...
19	α^2 Libræ	14 41 54.15	14 41 54,23	...
	20 Libræ	14 54 34.69	...	...
	Moon I. ...	15 24 51,64	15 25 22,76	...
	β' Scorpii	...	15 56 0,00	...
23	h^2 Sagittarii...	19 26 48 63	...	...
	Moon II....	19 51 26,64	...	...

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ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *November* 10, 1837. No. 9.

The President, on taking the chair, announced that, since the last Meeting of the Society, in June, the Council had prepared an Address of Congratulation to her Majesty, on her Accession to the Throne: that the same had been presented, and was published in the *London Gazette* of the 25th of July last; and that her Majesty had been graciously pleased to become the Royal Patroness of the Society.

Among the numerous presents to the Library, laid on the table this evening, was the magnificent work of Struve, on the micrometrical measures of double and multiple stars, made at the Dorpat Observatory, from 1824 to 1837, with Fraunhofer's great telescope; accompanied with a Report to the President of the Imperial Academy of Sciences at St. Petersburg, detailing the nature of the work, and the principal results which had been deduced.

The following communications were read: —

I. On the Parallax of α Lyræ. By the Astronomer Royal.

The author commences with stating, that after the discussions as to the sensible annual parallax of α Lyræ, which have been conducted with so much ability and ardour, and in which the opposite opinions have been founded on so many well-chosen observations, it would be useless now to express an opinion, except it were based on more numerous and more excellent observations, reduced with greater attention to accuracy than in former instances. He states, therefore, that the whole number of observations employed was 184, made entirely in the year 1836, and, in general, distributed uniformly over the year (with the exception of the month of February, in which no observations could be obtained): that the observations were divided equally between the two circles, and that nearly half of them were by reflection: that the telescopes have been in the same position on the circle during the whole year, with the exception of a few days at its commencement: that the zenith points have been determined independently every day: and that the six microscopes of each circle have been read for every one of these observations, as well as for every observation assisting to determine the zenith point He then states, as an innovation on the practice of Mr. Pond, that a *correction for runs* has been introduced, determined from examination made every week; the necessity for which arises from the circumstance that it is practically impossible to adjust the microscopes, so that five turns of

their micrometers shall correspond exactly to the interval between two divisions on the limb, and whose importance in this investigation depends on its periodic character varying with the temperature, and (with some regularity) with the season of the year. The author states his belief that, with this correction, the only appreciable defect of the mural circle is removed; and that it is thus superior to any other instrument which has been employed in this investigation. The author then gives the results in the form of equations, founded each on the mean of a group of observations; each set of observations (Troughton, by direct vision; Troughton, by reflection: Jones, by direct vision; Jones, by reflection) being divided into four groups, and an equation being obtained from each group, expressing the polar distance in terms of the correction of the coefficient of aberration, and the coefficient of parallax. Taking the mean or the results, with each circle, by direct and reflected vision, the coefficient of parallax from Troughton's circle appears to be $+ 0''\cdot2$; and that from Jones's circle $- 0''\cdot1$. The author concludes from this, that the annual parallax is too small to be sensible to our best instruments. The coefficient of aberration ($20''\cdot36$) appears, from the observations with Troughton's circle, to require no sensible alteration; from the observations with Jones's circle it appears to require the increase $0''\cdot4$. The north polar distance of α Lyræ, for Jan. 1, 1836, deduced from the whole of the observations, is $51^\circ\ 21'\ 53''\cdot73$.

II. The constant quantity of the Moon's Equatorial Horizontal Parallax, deduced from observations made at Greenwich, Cambridge, and the Cape of Good Hope, in 1832 and 1833. By Professor Henderson, Astronomer Royal for Scotland.

The author remarks that two methods have been adopted for determining the constant of the Moon's Equatorial Horizontal Parallax;—the first founded on the theory of gravity, which assigns a relation between the Moon's distance and the duration of her periodic revolution, the force of terrestrial gravity, and the magnitude of the Earth; and the second, on corresponding observations of the Moon's Declination, as affected with parallax, made at places remote from each other, on the earth's surface.

The principal determinations of the value of the constant of parallax are the following:—Burg, in his Lunar Tables, has adopted $57'\ 1''\cdot0$: in Burckhardt's Lunar Tables, the value adopted is $57'\ 0''\cdot5$. Damoiseau, assuming the Moon's mass to be 1-74th of the Earth's, has found $57'\ 0''\cdot9$; and Plana, in his *Theorie de la Lune*, assuming the mass to be 1-87th, has computed the constant to be $57'\ 3''\cdot1$. On substituting in this calculation 1-79·9th for the Moon's mass (which corresponds to the coefficient of Lunar Nutation $9''\cdot25$), Mr. Henderson finds for the constant of parallax $57'\ 2''\cdot0$.

The determinations of the Moon's parallax by the second method, have hitherto been founded on the observations made in the middle of the last century by Lacaille, at the Cape of Good

Hope, compared with the corresponding observations made in Europe. These observations were calculated by Lacaille, Lalande, and Du Sejour; and their results, reduced to the same hypothesis of the Earth's compression, namely, 1-300th, are as follow:—Lacaille, 57′ 4″·6; Lalande, 57′ 3″·7; and Du Sejour, 57′ 6″·0.

M. Olufsen has lately calculated the same observations, and, also, several others made by the same astronomers, but not formerly used; and his result, reduced to the same hypothesis of the Earth's compression, is 57′ 2″·76.

The observations of the Moon's declinations made by Mr. Henderson, with the mural circle, at the Cape of Good Hope, in 1832 and 1833, combined with the corresponding observations made at the observatories of Greenwich and Cambridge, afford other data for the determination of the parallax.

Mr. Henderson remarks, that though by observing the same stars at two observatories on the same day, the difference of the Moon's apparent declinations is obtained free from the effect of errors in the assumed declinations, yet, as the relative declinations of the principal stars are now known with great accuracy, the differences of the Moon's apparent declinations, as observed at two observatories, may be deduced from the observations of different principal stars, almost, if not altogether, as accurately as from observations of the same stars. Accordingly, whenever there has been a deficiency of Moon culminating stars observed in declination, the Moon's declinations have been obtained from comparison with the observations of such of the principal stars as were observed, their declinations being taken from a catalogue constructed from a careful comparison of the most recent and best authorities, and all the observations being reduced on a uniform system, by the application of the same corrections, and of Bessel's Refractions.

After these remarks, the author proceeds to give the formulæ by means of which the parallax may be deduced from the observations. A table follows of the quantities required for forming the equations of condition, and then the equations themselves, which are in number forty-three. In order to diminish the errors of the tabular parallaxes, these quantities are taken both from Burckhardt's and Damoiseau's Tables; and the equations formed are solved from each set of computed parallaxes separately. The final equation deduced by the method of least squares is,

$$\partial K = \left.\begin{matrix} +\,0{\cdot}66\ (\text{Burckhardt}) \\ +\,1{\cdot}56\ (\text{Damoiseau}) \end{matrix}\right\} + 5062''\,\partial c - 0''{\cdot}05\,\partial t - 0''{\cdot}12\,\partial s - 0''{\cdot}14\,\partial s',$$

where ∂K denotes the correction of the assumed constant of equatorial parallax, ∂c the correction of the assumed compression, ∂t the correction of the assumed difference of longitude, and ∂s and $\partial s'$ quantities introduced for the purpose of estimating the effect of constant differences that may exist in the values of the Moon's semidiameter, given by the observations at the Cape and Greenwich, and the Cape and Cambridge, respectively.

Applying this correction to the tabular constant K (= 57′ 0″·5

for Burckhardt's Tables, and 57′ 0″·9 for Damoiseau's), the constant of parallax is,

$$57' \, 1''{\cdot}16 + 5062'' \, \delta c - 0''{\cdot}05 \, \delta t - 0''{\cdot}12 \, \delta s - 0''{\cdot}14 \, \delta s'$$

by comparison with the first Table; and

$$57' \, 2''{\cdot}46 + 5062'' \, \delta c - 0''{\cdot}05 \, \delta t - 0''{\cdot}12 \, \delta s - 0''{\cdot}14 \, \delta s'$$

by comparison with the second. The mean of both is

$$57' \, 1''{\cdot}81 + 5062' \, \delta c - 0''{\cdot}05 \, \delta t - 0''{\cdot}12 \, \delta s - 0''{\cdot}14 \, \delta s'$$

If the earth's compression were assumed to be 1-290th, the constant would be increased by $5062'' \left(\frac{1}{290} - \frac{1}{300}\right) = 0''{\cdot}58$; and if the compression were assumed to be $\frac{1}{310}$, the constant would be diminished by 0″·54. The coefficients of δt, δs, and $\delta s'$, are so small, that the final result is very little affected by any probable values these quantities can have.

The most probable value of the constant of parallax deduced from Mr. Henderson's observations is, therefore, 57′ 1″·8; the corresponding value of the Moon's mass is $\frac{1}{78{\cdot}9}$; and of the coefficient of lunar nutation 9″·28.

III. List of Moon-culminating Stars observed at the Royal Observatories of Greenwich and Cambridge, in the month of June, 1837.

Date.	Object.	Apparent Right Ascension. Greenwich. h m s	Cambridge. h m s
June 10	ρ Leonis	...	10 24 14,17
	Moon 1 L.	...	10 57 43,61
11	Moon 1 L..	...	11 42 33,33
	ο Virginis ...	...	11 56 55,55
14	74 Virginis ...	13 23 31,23	...
	ο Virginis ...	13 37 17,27	...
	Moon 1 L..	14 3 15,02	14 3 14,19
	λ Virginis ...	14 10 19,74	14 10 19,89
	α² Libræ	14 41 54,13	14 41 54,33
15	λ Virginis ...	14 10 19,61	...
	α² Libræ	14 41 54,18	14 41 54,24
	Moon 1 L..	14 56 46,66	14 56 45,69
	κ Libræ	15 32 36,05	15 32 36,17
	b Scorpii	...	15 41 13,56
17	α Scorpii	...	16 19 28,17
	τ Scorpii	...	16 25 47,29
	Moon 1 L..	...	17 0 21,19
19	σ Sagittarii...	18 45 12,05	18 45 12,07
	Moon 2 L..	19 22 24,95	19 22 24,16
	59 Sagittarii...	...	19 46 58,91
	c Sagittarii...	...	19 52 40,28
21	ζ Capricorni .	21 17 23,27	...
	Moon 2 L..	21 33 57,01	...
	ι Aquarii	21 57 39,52	...

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ROYAL ASTRONOMICAL SOCIETY.

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The following communications were read:—

I. Observation of the Solar Eclipse of May 15, 1836, at the Observatory of San Fernando. By M. Cerquero.

II. Moon Culminating Stars observed at San Fernando in 1835 and 1836. By M. Cerquero.

III. List of 285 Stars of the Society's Catalogue which are Double. By Mr. Holehouse.

IV. On a very Ancient Solar Eclipse observed in China. By R. W. Rothman, Esq.

This is the famous eclipse which has been so much discussed by the Jesuit missionaries, De Mailla, and Gaubil; which has been vaunted as an irrefragable proof of the antiquity of the Chinese empire and science; and for failing to predict which, the unlucky astronomers, Ho and Hi, were punished with death.

The particulars are given in the *Histoire Générale de la Chine*, translated from the Chinese by Moyriac de Mailla, and the *Observations Mathematiques*, &c. published by Souciet. It seems that a certain Chinese history, the *Chou-King*, said to be of the highest antiquity, but without date, contains a statement to the effect that, towards the beginning of the reign of Tchong-Kang, on the first day of the third Moon of Autumn, there was an eclipse of the Sun in the constellation *Fang* (Scorpio). Another chronicle, less ancient, but of which the date is still anterior to 460 B.C., states that the eclipse took place in the fifth year of Tchong-Kang, on the first day of the ninth month, and adds cyclic characters for the day and year, corresponding, according to some of the chronologers, to October 13, 2128 B.C. The chronologers, however, are not agreed with respect to the year of the eclipse. Some refer it to 2159 B.C.; others, as just stated, to 2128; and Gaubil, by whom the eclipse was calculated, and who cites the calculations of three other Jesuits in proof of the accuracy of his own, says it took place, on the 12th of October, in the year 2155. Freret, on the authority of the calculations of Cassini, refers it to the 23d of September, 2007. On account of these discrepancies, and the uncertainty occasioned by the imperfections of the Tables employed in the former calculations, Mr. Rothman undertook to calculate the eclipse anew, from the more accurate Tables now existing.

He states, that for the Sun he employed Delambre's Tables, and for the Moon the Elements of Damoiseau; and that he has found the eclipse took place on the 13th of October, 2128 B.C., the instant of the greatest phase being $12^h\ 8^m\ 47^s$ mean time from midnight at the place of observation, and the magnitude 10·5 digits;—a result agreeing entirely with the indications of the Chinese chronicle.

The reader may see the arguments for and against the authenticity of this observation, in Delambre, *Histoire de l'Astronomie Ancienne*, tom. i. p. 350, *et seq.*

V. On the Repetition of the Cavendish Experiment, for determining the Mean Density of the Earth. By the President.

It is well known to most of the Members of the Society, that the Council has long had it in contemplation to repeat the celebrated and interesting experiment of the late Mr. Cavendish for determining the Mean Density of the Earth, and that a Committee was appointed more than two years ago to consider of its practicability. The object is now in a fair way of being accomplished, Her Majesty's government having been pleased to grant the sum of 500*l.* towards defraying the requisite expenses. The apparatus is, at this moment, in the course of being erected, and, as soon as it is completed, the experiments will be commenced.

During the time, however, that the subject has been in agitation in this country, it appears that the same experiment has been undertaken by M. F. Reich, Professor of Natural Philosophy in the *Académie des Mines*, at Freyberg, in Saxony. The details of the experiment are given in a Memoir, read by him at the German Scientific Association, which met at Prague in the month of September last. Whether this Memoir has yet been published, the Author has been unable to ascertain; but an abstract of it has appeared in some of the foreign journals, from which the following particulars are collected.

The method followed by M. Reich appears to have been exactly the same as that of Cavendish. The apparatus was erected in a large room under the buildings of the *Académie*, the windows of which were carefully closed up, and other precautions taken to preserve a uniform temperature. The torsion-balance, carrying two small leaden balls at the extremities of its arms, was encased in a wooden box, of dimensions just sufficient to allow room for the oscillations. To avoid currents of air, the oscillations were observed by means of a telescope fixed outside the door of the room in which the apparatus was placed, and directed on a mirror attached to the extremity of the arm, and illuminated by a lamp, also placed outside the room. The masses, whose attraction was to be measured, were spheres of lead, weighing 45 kilogrammes, or 695,061 grains. They were suspended by brass rods to a beam, movable about a vertical axis; and which, by means of cords and pullies, the observer, without entering the room, could bring into any required position, with reference to the direction of

the arms of the torsion-balance. It was found, however, most convenient, to use only one of the spheres. The principal correction required is for the *moment* of the arm of the balance. This was computed by a method similar to that which was employed by Gauss for determining the *moment of inertia* of his magnetic bars.

Nearly two years were consumed in the necessary preparations; but when completed, M. Reich was enabled to perform the experiments during the three months of June, July, and August, 1837. Each observation required the determination of three quantities:—the distance of the centres of the large and small spheres, the time of the oscillations, and the deviation of the arm of the balance. The distance varied from 6·62 to 7·49 inches; the duration of the oscillations, from $6^m\ 41^s$, to $6^m\ 50^s$; and the deviation from ·236 to ·315 of an inch. The greatest source of error, is in the determination of the deviation; the position of the arm being subject to some anomalous variations caused, probably, by slight currents of air in the interior of the wooden case. This source of error could only be eliminated by increasing the number of observations; but the differences of the partial results actually obtained were so small that the mean result may be regarded as sufficiently approximate.

The number of observations was 57. The mean of the whole gives the density equal to 5·44, a result which is almost identical with that of Cavendish. M. Reich, also, used for the attracting mass a sphere of cast-iron, of the same diameter as the leaden one, and weighing 30 kilogrammes, or 463,373 grains. Five observations with this sphere gave the density = 5·43.

The public will look with much interest for further particulars respecting these experiments, in order that they may be examined more in detail. The only innovations on the method of Cavendish appear to have consisted in using only one of the great spheres in the same experiment, and in the mode of observing the deviation of the arm of the balance. The arm itself appears to have been nearly of the same length as that used by Cavendish, but we are not informed of its weight, nor of the weight of the small balls. The large spheres, however, were much inferior to those of Cavendish, their diameters being only $7\frac{1}{4}$ inches, and weight less than 2-7ths of those used by Cavendish. The employment of the cast-iron sphere is a new feature in the experiment, but it does not appear that the small balls were changed.

Mr. Baily concludes with remarking that, though these experiments are, on the whole, confirmatory of the general result obtained by Mr. Cavendish, they do not interfere with the plan the council of the Society had in contemplation, which was not merely to repeat the original experiment in a precisely similar manner, but also to extend the investigation by varying the magnitude and substance of the attracting masses, by trying their effect under considerable differences of temperature, and by other variations that may be suggested during the progress of the inquiry.

VI. Moon-culminating Stars observed at Greenwich, Cambridge, and Edinburgh, in the months of July, August, and September, 1837.

Date.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
*June 11	ι Leonis	...	11 15 26,46	...
	Moon 1 L.	...	11 42 57,66	...
14	Moon 1 L..	..	14 3 41,87	...
	λ Virginis ...	...	14 10 19,47	...
	α² Libræ	...	14 41 54,09	...
15	λ Virginis ...	...	14 10 19,64	...
	α² Libræ	..	14 41 53,99	...
	Moon 1 L..	...	14 57 16,20	...
	α Libræ	...	15 32 36,17	...
	b Scorpii......	...	15 41 13,71	...
18	θ Ophiuchi...	...	17 12 2,97	...
	Moon 2 L..	...	18 12 39,43	...
July 7	γ Leonis	...	10 10 58,81	10 10 58.98
	Moon 1 L..	...	10 41 21,78	10 40 56,92
10	γ¹ Virginis ...	...	...	12 32 25,31
	Moon 1 L..	12 54 25,16	...	12 54 24,52
	α Virginis	13 16 37,98	...	13 16 38,20
11	α Virginis	...	...	13 16 38,14
	Moon 1 L..	...	...	13 41 10,61
14	π Scorpii	...	...	15 49 2,69
	β¹ Scorpii	...	...	15 56 0,57
	Moon 1 L..	...	...	16 27 14,02
	A Ophiuchi ...	...	...	17 5 22.33
	θ Ophiuchi ...	...	...	17 12 2,95
15	A Ophiuchi ...	17 5 22,72	...	...
	θ Ophiuchi ...	17 12 2,89	17 12 2,97	...
	Moon 1 L.	17 33 45,02	17 34 21,40	17 33 43,79
	γ² Sagittarii ...	17 55 23,40	...	17 55 23,36
	δ Sagittarii ...	18 10 36,58	18 10 36,84	18 10 36,66
16	γ² Sagittarii ...	...	...	17 55 23,17
	δ Sagittarii ...	...	...	18 10 36,60
	Moon 1 L.	...	..	18 43 57,03
	τ Sagittarii ...	...	...	18 56 48,67
	h² Sagittarii ...	...	...	19 26 50,01
18	π Capricorni	...	20 18 2,05	...
	ψ Capricorni	...	20 36 29,15	...
	Moon 2 L. .	...	21 4 56,72	...
	ζ Capricorni	...	21 17 24,01	...
	δ Capricorni	...	21 38 4,95	...
19	Moon 2 L..	22 6 46,39	...	...
	σ Aquarii	22 22 3,39	...	...
	δ Aquarii	22 46 1,96	...	...
24	Moon 2 L.	...	...	2 30 46,46
	ε Arietis	...	...	2 49 55,30
	δ Arietis	...	...	3 2 20,31

* For Greenwich and Cambridge, in the month of June, see page 94.

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
Aug. 8	Moon 1 L. .	...	14 11 56,88	. .
	α^2 Libræ	...	41 41 53,71	...
10	Moon 1 L.	...	...	15 59 39,53
	α Scorpii	...	...	16 19 27,82
11	α Scorpii	16 19 27,66	...	16 19 27,55
	Moon 1 L.	17 1 51,01	...	17 1 49,51
	θ Ophiuchi ...	17 12 2,80	...	17 12 2,82
	γ^2 Sagittarii . .	17 55 23,35	...	17 55 23,29
12	θ Ophiuchi ...	...	...	17 12 2,96
	γ^2 Sagittarii ...	...	...	17 55 23,32
	Moon 1 L.	18 8 51,09	...	18 8 50,08
	ϕ Sagittarii ...	18 35 31,28	...	18 35 31,34
	σ Sagittarii ...	18 45 12,37	...	18 45 12,41
13	ϕ Sagittarii ...	18 35 31,28	18 35 31,00	18 35 31,17
	σ Sagittarii ...	...	18 45 12,60	18 45 12,33
	Moon 1 L.	19 18 19,98	19 18 56,83	19 18 18,61
	59 Sagittarii . .	19 46 59,37	19 46 59,36	19 46 59,58
	c Sagittarii ...	19 52 40,93	19 52 40,84	19 52 40,85
14	59 Sagittarii ...	19 46 59,52	19 46 59,50	19 46 59,68
	c Sagittarii ...	19 52 40,91	19 52 40,57	19 52 40,99
	Moon 1 L.	20 27 1,80	20 27 37,35	20 27 0,55
	η Capricorni	...	20 55 10,12	20 55 10,02
	ζ Capricorni	...	21 17 24,14	21 17 24,35
15	η Capricorni	...	...	20 55 10,44
	ζ Capricorni	...	...	21 17 24,42
	Moon 1 L.	...	...	21 32 17,71
	Moon 2 L.	...	...	21 34 46,39
	θ Aquarii ...	...	...	22 8 16,66
16	ι Aquarii	...	21 57 40,46	...
	θ Aquarii	...	22 8 16,41	...
	Moon 2 L...	...	22 36 3,48	...
	λ Aquarii	...	22 44 9,38	...
	ψ^3 Aquarii	...	23 10 31,64	...
17	λ Aquarii ...	22 44 9,11	...	22 44 9,60
	ψ^3 Aquarii ...	23 10 31,48	...	23 10 31,71
	Moon 2 L.	23 32 16,22	...	23 32 15,40
	κ Piscium ...	23 39 36,36	...	23 39 36,34
	τ Piscium ...	...	...	23 53 38,77
20	ϵ Piscium	1 36 50,00	...	...
	Moon 2 L...	2 10 48,76	...	...
21	ι Arietis	2 49 56,15	...	...
	Moon 2 L ..	3 3 53,45	...	...
	β Arietis	3 14 43,50	...	...
	η Tauri	3 37 50,07	...	...
26	α^2 Geminorum	...	...	7 24 12,46
	Moon 2 L.	...	...	7 42 40,99
Sept. 8	Moon 1 L.	...	...	17 41 11,68
	λ Sagittarii ...	...	...	18 17 57,11
9	Moon 1 L...	...	18 48 17,39	...
	h^2 Sagittarii...	...	19 26 49,82	...

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Edinburgh.	Cambridge.
		h m s	h m s	h m s
Sept. 10	π Sagittarii ..	...	...	19 0 6,92
	h² Sagittarii ...	19 26 49,76	...	19 26 49,88
	Moon 1 L. .	19 54 52,23	...	19 54 50,98
	π Capricorni	20 18 1,96	...	...
	ψ Capricorni .	...	...	20 36 29,20
11	π Capricorni	...	20 18 1,82	...
	ψ Capricorni	...	20 36 28,96	...
	Moon 1 L	...	21 0 44,30	...
	ζ Capricorni	...	21 17 24,05	...
	δ Capricorni	...	21 38 5,09	...
12	ζ Capricorni .	...	...	21 17 24,42
	δ Capricorni .	...	...	21 38 5,44
	Moon 1 L. .	...	...	22 2 3,90
	σ Aquarii	...	...	22 22 4,10
13	σ Aquarii	...	...	22 22 4,16
	δ Aquarii	...	...	22 46 2,94
	Moon 1 L. .	...	...	23 0 23,53
	ψ³ Aquarii......	...	...	23 10 31,96
	η Piscium	...	...	23 39 36,78
14	ψ Aquarii......	...	23 10 31,67	...
	η Piscium......	...	23 39 36,64	...
	Moon 2 L. .	...	23 58 40,60	...
	η Ceti	...	0 21 46,44	...
15	η Ceti	...	0 21 46,08	0 21 46,37
	δ Piscium	...	0 40 16,53	0 40 17,05
	Moon 2 L. .	...	0 52 29,60	0 52 0,61
	μ Piscium	...	1 21 41,97	...
	ν Piscium	...	1 33 0,05	...
22	ε Geminorum	6 33 56,08	...	...
	Moon 2 L. .	7 23 41,88	...	7 23 41,55
	κ Geminorum	...	...	7 34 37,81

London: J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *January* 12, 1838. No. 11.

The following communications were read:—

I. Occultations of δ Arietis, August 21; *C* Tauri, August 24; δ Arietis, October 15; *f* Aquarii, November 6; and Immersion of ψ' Aquarii, November 7, 1837. By R. Snow, Esq.

II. On the difference of Longitude between the Greenwich and Paris Observatories. By Edward J. Dent. Communicated by the Astronomer Royal.

Mr. Dent having lately had occasion to visit the French capital, resolved to take advantage of the opportunity which thus presented itself of ascertaining, by the transit of chronometers, the difference between the meridians of the Greenwich and Paris Observatories, the astronomers-royal of the respective countries, (G. B. Airy, Esq. and M. Arago), offering every assistance requisite to the completion of the object.

Twelve chronometers were selected for the purpose, the rates and errors of which were ascertained by daily comparison with the Observatory clocks during seven days before they were taken abroad; during fourteen days, while they remained at the Paris Observatory, and during seven days after they were brought back to Greenwich. In travelling, they were placed in a wooden box, and packed in horse-hair. The route pursued was by coach to Dover, whence the water was crossed in a sailing-boat to Boulogne, and the journey to Paris completed by the diligence.

Between the last comparison of the chronometers made at Greenwich, and the first at the Observatory of Paris, there was an interval of seventy-two hours. After a space of fourteen days, the instruments were returned by the diligence to Boulogne, whence they were conveyed by a steam-vessel to Greenwich. On this journey, only forty-nine hours elapsed between the comparisons at the two Observatories. It may be remarked, that in passing through the paved towns, both in England and France, the chronometers were exposed to severe concussion.

In this experiment the whole difficulty turns on determining the rate of the chronometers during the transit from the one station to the other. Mr. Dent employs two methods for this purpose,—the first of which may be explained as follows: suppose the chronometer gaining; the error of the chronometer from mean time being observed when it was taken from the Observatory at Greenwich,

and also when it was brought back, the difference of the two errors gives the number of seconds t gained during the interval of its absence. But t may be considered as made up of two parts,—the first consisting of the number of seconds gained during the journey to Paris and back; and the second, of the number of seconds gained during the fourteen days it remained in the Paris Observatory. The last of these is known from the daily comparisons made at Paris with the Observatory clock; subtracting it, therefore, from t, there remains the number of seconds gained while travelling; and this being again divided by the number of days occupied in the two journeys, the result gives the mean daily *travelling rate* to be applied as a correction to the difference of mean time at the two places, as shewn by the watch. The difference of longitudes, found in this manner, from the mean of all the observations, is $9^m\ 21^s,14$.

The second method adopted consists in obtaining the mean daily rate of each chronometer during the seven days previous to its removal from Greenwich, and during the first seven days after arriving at the Paris Observatory, and assuming the mean of these two to be its mean travelling-rate in the journey from Greenwich to Paris. In like manner the comparison of the mean daily rate of the seven days before leaving the Observatory at Paris, and of the seven days after returning to Greenwich, give the travelling-rate during the journey back. By this method the difference of meridians by the chronometers in the journey from Greenwich to Paris is $9^m\ 22^s,08$; and in the journey from Paris to Greenwich, $9^m\ 20^s,49$. The difference is $1^s,59$; but the mean of the two is $9^m\ 21^s,28$; differing from the result obtained from the other method only by 14-hundredths of a second.

Mr. Dent has given the official errors and rates received from the two Observatories, arranged in a tabular form; thus supplying the information necessary to admit of the statements being placed in any other point of view.

III. Observations of the Lunar Eclipse on the 13th October, 1837, made with a view to ascertain the practical advantages of the method of determining the Longitude by that phenomenon, suggested by the late Captain Kater, for a fixed station, and particularly of an adaptation of that method to Nautical purposes, proposed by Captain Beaufort, Hydrographer to the Admiralty. By Captain Basil Hall, R.N. Communicated by Captain Beaufort.

In April 1833, Captain Kater read a notice to the Astronomical Society, of a method which had occurred to him for determining the longitude, by observing, at successive intervals, the angular breadth of the uneclipsed portion of the Moon's disc, first, on the Moon's entering the earth's shadow, and, secondly, on her emerging from it.* He proposed to take a series of measures with a micrometer, attached to a telescope of convenient power, and to

* *Monthly Notices of Royal Astronomical Society*, for April 1833.

record the time of each measure, as the Moon entered the earth's shadow, and on its emerging from the shadow, noting the times at which the *same* measures were found to obtain in both cases. By then taking the mean between each pair of the times when the same breadth of the uneclipsed part of the Moon was observed, a series of *middle times* of the eclipse would be obtained, each of which, as he conceived, would be at least as good as that obtained by taking the mean between the first and last contact of the Moon with the dark shadow,—and between the first and last total immersion of the Moon in the dark shadow,—methods heretofore employed for obtaining the longitude by a lunar eclipse.

Captain Kater gives the result of his observations on the lunar eclipse of 2d of August, 1822, and the result certainly shews that, in such hands as his, and on a fixed station (where alone a micrometer can be used), an approximation to the truth may be made, which is wonderfully near, considering the nature of the phenomenon. As the micrometer, however, cannot be used on board ship, Captain Beaufort suggested that the measures in question might be made at sea with a sextant, and Captain Hall felt anxious to put this proposal to the test of experiment. On the occasion of the recent lunar eclipse, therefore, he prepared his nautical instruments, viz. a chronometer and sextant, at the Royal Observatory of Edinburgh, for the particular observations alluded to, and also for observing the beginning and end, in the usual way. The eclipse was a total one, and every circumstance proved favourable, so that the method, he conceives, had the fairest chance possible.

The results of the observations made by Captain Hall on this occasion (the whole of which are recorded) are as follows:—

The longitude of the Observatory at Edinburgh, as obtained by the mean of the times of the beginning and end, was erroneous by $-9^s,5$, while that obtained from the mean of the observed times of the total immersion and the first reappearance of the Moon from the dark shadow, was in error $+9^s,0$; the mean of the two, that is, of the four observations, being very near the truth. The telescope used was the telescope of the sextant, magnifying about 8 or 10 times, held in the hand, without a stand.

For the purpose of testing the new method, seventeen pairs of measurements were made with the sextant and the same telescope; and the times being recorded by a half-seconds chronometer, a series of observed middle times of the eclipse was obtained; and the resulting longitude, by the mean of the whole 34 observations (making ing 17 pairs) is erroneous by $+9^s,2$. The greatest error of observation in any one pair is $+53^s$, and the least $+0^s,5$.

Captain Hall states, it would have been easy to have taken a considerably greater number of observations; but he was desirous, in the first place, not to hurry any part of the experiment, and in the next, not to avail himself of the peculiarly favourable circumstances of the occasion, but rather to adopt a course which might readily be followed at sea under circumstances of less convenience.

The following table shews the separate results of each pair of measurements of the uneclipsed part of the Moon's disc, 1st, on its entering; and, 2dly, on its emerging from the dark shadow. The numbers in the seventh column are the differences between those in the sixth and $11^h\ 17^m\ 2{\cdot}6^s$, the mean time at Greenwich of the middle of the eclipse; and those in the last column are the differences between those in the seventh and $12^m\ 43{\cdot}6^s$, the true difference of longitude between Greenwich and Edinburgh.

No. of Pairs.	Obsd. Angle subtended by the uneclipsed Pt.	M. T. at Edin. of the 1st Set of Observations.	M. T. at Edin. of the 2d Set of Observations.	Sum of the observed Times.	½ Sum, or the obsd. M. T. at Edin. of the Middle of the Eclipse.	Observed Difference of Longitude.	Errors of Observation.
	° ′ ″	h m s	h m s	h m s	h m s	m s	s
1	0 28 20	9 22 1	12 45 1	22 7 2	11 3 31·0	13 31·6	+ 48·0
2	27 0	24 21	42 31	6 52	3 26·0	13 36·3	+ 53·0
3	25 0	27 56	40 21	8 17	4 8·5	12 54·1	+ 10·5
4	23 0	30 31	37 51	8 22	4 11·0	12 51·6	+ 8·0
5	21 10	33 24	34 13	7 37	3 48·5	13 14·1	+ 30·5
6	19 50	37 21	31 26	8 47	4 23·5	12 39·1	− 4·5
7	18 55	40 31	28 11	8 42	4 21·0	12 41·6	− 2·0
8	16 50	43 41	25 6	8 47	4 23·5	12 39·1	− 4·5
9	15 30	47 6	22 41	9 47	4 53·5	12 9·1	− 34·5
10	12 30	50 21	18 6	8 27	4 13·5	12 49·1	+ 5·5
11	11 30	53 6	15 26	8 32	4 16·0	12 46·6	+ 3·0
12	10 10	56 16	12 46	9 2	4 31·0	12 31·6	− 12·0
13	8 40	59 21	9 16	8 37	4 18·5	12 44·1	+ 0·5
14	7 20	62 21	6 41	9 2	4 31·0	12 31.6	− 12·0
15	5 30	65 21	2 51	8 12	4 6·0	12 56·6	+ 13·0
16	4 10	68 21	11 59 36	7 57	3 58·5	13 4·1	+ 20·5
17	3 30	70 21	57 11	7 32	3 46·0	13 16·6	+ 33·0
				Mean =	11 4 9·8	12 52·8	+ 9·2

The above results seem to shew, that the old method of observing, viz. by noting the beginning and end, and also the first and last total immersion, is at least as good as the new method suggested by Captain Kater. But this circumstance, Captain Hall remarks, ought not, of course, to prevent navigators from taking as many pairs of measurements as possible, in case of losing the other phenomena, or in order to verify the results.

London: J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *February* 9, 1838. No. 12.

Report of the Council of the Society to the Eighteenth Annual General Meeting, held this Day.

This Society has now terminated the eighteenth year of its existence; during which period, it must be evident to the most inattentive observer that the state of Astronomy has been materially improved, both in theory and practice. That some portion (if not a very considerable part) of this improvement has been effected by the exertions and co-operation of the Society, there can be but little doubt: and this consideration, it is hoped, will operate as a powerful motive to the several members to assist in the advancement of a science which requires the aid of so many labourers to follow out, with the requisite detail, the various branches of which it consists.

Since the last anniversary, the Society has to deplore the loss of his late Majesty King William the Fourth, who was not only the Patron of the Society, but who also granted the Charter, under the authority of which they now assemble. On the accession of her present Majesty to the throne, the Council presented an Address of condolence; and at the same time requested that her Majesty would be pleased to become the Patroness of the Society: a request with which she has most graciously complied.

The Treasurer of the Society has laid before the Auditors the accounts of Receipt and Expenditure during the past year, and has furnished an abstract of the same for the use of the Members at large: all of which have been found correct, and approved by the Auditors. The result is stated below, together with an estimate of the assets and present property of the Society as nearly as the same can be ascertained.

RECEIPTS.

	£.	s.	d.
Balance of last year's Account	116	16	5
1 year's dividend on 500*l.* Consols	15	0	0
½ year's dividend on 1495*l.* 4*s.* 6*d.* New 3½ per Cents	26	3	3
½ year's dividend on 1515*l.* 19*s.* 4*d.* New 3½ per Cents	26	10	7
For 2 silver medals	3	14	0
Sale of Memoirs, by Harrison	25	16	0
On account of arrears	84	0	0
10 admission fees	21	0	0
6 compositions	126	0	0
7 first year's contributions	12	12	0
78 annual contributions, for 1837-1838	163	16	0
	£621	8	3

PAYMENTS.

J. Moyes, for paper and printing the Meridian Ephemeris for 1836	32	11	6
Cubitt and Co. for carpenter's work	23	18	8
Engrossing the Address to the Queen	1	10	0
J. Rumfitt, for binding books, &c.	4	16	0
Engraving medals, &c.	2	4	0
Harrison and Co. for stationery	6	4	0
84*l.* 6*s.* Stock in the New 3½ per Cents, for 4 compositions	84	0	0
1 year's salary to Assistant Secretary	100	0	0
Commission on sale of Memoirs, and collection of contributions	15	5	6
Coals, candles, wood, &c.	13	15	8
Tea, sugar, cakes, &c.	13	13	0
Porterage, cleaning rooms, &c.	12	0	6
Arrears of taxes in 1836	6	9	2
Assessed taxes (2 years)	6	5	0
Poor's rates	3	6	8
Land tax	1	11	3
R. Lloyd, for paper and copper-plate printing	8	16	4
Balance in the Treasurer's hands	285	1	0
	£621	8	3

The assets and present property of the Society may be estimated as under: viz.

				£.	s.	d.
Balance in the hands of the Treasurer				285	1	0
Arrears, February 1, 1838.						
1 contribution of five years' standing	£10	10	0			
3 ——— of four ditto	25	4	0			
3 ——— of three ditto	18	18	0			
6 ——— of two ditto	25	4	0			
38 ——— of one ditto	79	16	0			
3 admission fees	6	6	0			
3 first year's contributions	4	4	0			
				170	2	0
Carried forward				455	3	0

		£. s. d.
Brought forward		455 3 0
4 admission fees and first year's contributions at four guineas each, from new Fellows	16 16 0	
4 ditto ditto, at three guineas	12 12 0	
		29 8 0
£500 Consolidated 3 per Cents. } £1515. 19s. 4d. New 3½ per Cents. } valued at		2000 0 0
		£2484 11 0

1 gold medal unappropriated.
Various astronomical instruments, books, prints, &c.
The unsold Memoirs of the Society.

Three of the instruments above alluded to, are now in the Apartments of the Society: viz.

The *Harrison* clock.
The *Owen* double portable circle.
The *Owen* quadruple portable sextant.

Three others are, for the purpose of safe custody, placed as under: viz.

A brass quadrant, said to be Lacaille's, at the Royal Society.
The *Fuller* Theodolite, at Mr. T. Jones's.
The Standard scale, at Mr. Baily's.

The remainder are lent, during pleasure, to the several parties under mentioned: viz.

The *Beaufoy* circle, } to the Rev. M. Ward.
The *Beaufoy* clock, }
The *Beaufoy* transit, to Mr. T. Jones.
The other *Beaufoy* clock, to Col. Pasley, R.E.
The *Lee* circle, to Capt. Smyth, R.N.
The *Wollaston* telescope, to Professor Schumacher.
The two invariable pendulums to Col. Chesney.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year: viz.

	Compounded.	Annual Contributors.	Non-resident.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1837	67	131	95	6	299	36	335
Since elected	3	15	...	...	18	3	21
Deceased, &c.	−2	−2	−1	...	−5	...	−5
Resigned	...	−5	...	...	−5	...	−5
Removals	+3	−3	...	...	...	...	...
February 1838	71	136	94	6	307	39	346

Amongst the losses by death, since the last anniversary, the Council have to regret that of Henry Thomas Colebrooke, Esq., one of the founders of this Society, and who, during the years 1823 and 1824, officiated as its President. The whole life of Mr. Colebrooke was one of great activity and usefulness. At a very early age, and when engaged in the laborious duties attending his successive official situations in the East Indies, he was ever zealous in the pursuit of literature and science, and through the whole course of a long life, never neglected any opportunity of promoting and advancing them. The principal details of Mr. Colebrooke's active life have been so recently set forth in the short memoir of him, given in the Annual Report of the Royal Society, that it appears almost unnecessary to state here what would be only a repetition of that which has been already so excellently done. The Council, however, cannot omit this opportunity of expressing their unanimous concurrence in the sentiments with which that memoir concludes: viz. that "when we take into account Mr. Colebrooke's great acquirements in mathematics and philosophy, and in almost every branch of literature, combined with the most accurate and severe judgment, and also his great public services in situations of the highest trust and responsibility, we shall not hesitate to pronounce him one of the most illustrious of that extraordinary succession of great men, who have adorned the annals of our Indian empire."

Another of our losses is that of Dr. John Louis Tiarks, F.R.S., who was well known to many of the gentlemen present, as an excellent astronomer, and an amiable man. He was born in the Duchy of Oldenburg, in May 1789, and completed his education in the University of Gottingen, where he principally distinguished himself by his advancement in mathematics. About the year 1808, he settled at Hamburg; but, two years afterwards, dreading his liability to the French conscription, he passed over into England, where he procured a situation in the library of Sir Joseph Banks. This he retained till, by the interest of that kind patron, he received the highly responsible appointment of British Astronomer to the commission named, under the treaty of Ghent, for settling that important question, the American Boundary Line.

Mr. Hassler, an Associate of this Society, being chosen on the part of the United States as their Astronomer, the Commissioners proceeded, during several years, to survey the contested points, in which service they underwent many hardships and privations, but executed the duties with great accuracy. Notwithstanding, however, their zealous endeavours to arrange the business intrusted to their management, it involved so many delicate circumstances, that, unable to settle some essentials, it was agreed by the respective Governments, to submit the matter to the arbitration of the King of the Netherlands. In consequence of this resolution, Dr. Tiarks repaired to the Hague, in 1828, to give the necessary information. The decision of the Monarch was rejected by the United States; and, in 1834, their President having addressed the British Court

on the subject, Dr. Tiarks was summoned from Germany to London. Here his reports were again scrutinized; and, though they are admitted to be clear, distinct, and conclusive, the question still remains unsettled, and an extent of about 10,000 square miles still remains disputed territory.

In the intervals which this important service admitted, the admirable efficiency of Dr. Tiarks as a practical astronomer, recommended him, through the Board of Longitude, to the Admiralty, who intrusted him with several important scientific missions. In the summer of 1822, he established the position of Funchal by a mean of fifteen chronometers, carried from Greenwich to Falmouth, thence to Madeira, and back by the same places. By the results obtained, it appeared that the longitude of Pendennis Castle was considerably to the westward of that assigned it by the great Trigonometrical Survey; which unexpected discovery led to further investigation. In the year 1823, he carried on some operations, with twenty-six chronometers, betweeen Dover, Falmouth, and Portsmouth; in which he fully pointed out that an error of nearly four seconds of time, pervaded the longitudes of some of the trigonometrical stations of the national survey. The details of this, and an explanation of the cause, was published in an excellent paper which is printed in the Philosophical Transactions for 1824. In the following year he proceeded, with no fewer than thirty-six chronometers, to Altona, Heligoland, and various stations in the North Sea; on which occasion he was accompanied by Sir Humphry Davy, who was desirous of such an opportunity of observing the mechanical effect of his protectors on the copper of the Comet, the steamer which conveyed them.

Dr. Tiarks left this country, for the last time, in March 1835, and was engaged in the revision of some calculations affecting the charities of Oldenburg. In the spring of last year, he was attacked by paralysis, which overcame his already shattered constitution. He expired on the first of May, in the forty-eighth year of his age, at Jever, his native town, regretted by a numerous circle of friends. As to the value in which he was held by this Society, it will not be forgotten that, in the Tenth Annual Report of the Council to the General Meeting, it was said,—"The numerous communications that have, from time to time, been received from Germany, and the valuable astronomical works that are constantly issuing from the press of that country, render an acquaintance with the German language almost essential to the prosecution of science. The difficulty which the Council would otherwise have experienced, has been greatly removed by the cordial and very valuable assistance of a gentleman deeply versed in every branch of the science, who has, on all occasions, been ready in favouring the Council with translations of such papers as they have, from time to time, required. To mark their sense of this obligation, and as a small tribute of respect, they have agreed, 'That it be recommended to the general meeting to present to Dr. Tiarks a copy of the Memoirs, handsomely bound, for his kind and ready assistance at all times in

translating various foreign papers for the use of the members, and for other valuable services rendered to the Society.'" It is almost needless to add, that this recommendation of the Council was unanimously approved of, and adopted by the members to whom it was addressed.

The very recent decease of the Rev. Thomas Catton, of St. John's College, Cambridge, has prevented the Council from obtaining such information respecting his life as might be interesting to the meeting. It is well known that he was one of the earliest members of this Society, and took much interest in all subjects connected with astronomy, particularly in the establishment of the Observatory at Cambridge. The small observatory in his own college, was intrusted to his care; and though placed in an inconvenient situation, he always took great interest in shewing it to those who were engaged in astronomical pursuits.

Amongst the Associates, the Council are sorry to announce the recent decease of Professor Moll, of Utrecht. Professor Moll was born in Amsterdam, the 18th of January, 1785, and died in the same city on the 17th of January, 1838. About the year 1800 he was placed as a junior clerk, at Amsterdam, in a mercantile house of the greatest respectability, for the purpose of acquiring a general knowledge of mercantile affairs; and, although a young gentleman of independent fortune, performed the duties of the office with great credit to himself. The whole of his leisure time was devoted to the study of mathematics, astronomy, &c. His father, perceiving the turn of his mind, allowed him to relinquish commercial pursuits, and follow his inclination for study. This change occurred about the year 1806. He then commenced a regular course of study, under his great friend and patron, Professor Van Swinden, of Amsterdam. He afterwards studied at the University of Utrecht, and lastly at Paris.

In December 1812, he was appointed Professor of Mathematics and Natural Philosophy at the University of Utrecht. And in addition to those duties, he was intrusted by his Majesty the King of the Netherlands with many important charges. During a considerable period he had the superintendence of the *Waterstaat*, comprising all the measures requisite for the protection of that country from inundation. He also had, during a long period, the care of the chronometers and other instruments belonging to the navy. He was one of the three examiners of the naval officers previous to their appointment. In 1835, he was charged by his Majesty to direct and report upon a series of observations on the tides along the whole of the Dutch coast, which report was transmitted to Professor Whewell, of Cambridge.

Among the papers he sent to England for publication, there was one to Sir David Brewster, on the invention of the telescope. Another, the result of his experiments upon the velocity of sound, published in the Phil. Trans. of the Royal Society for 1824. His paper upon the comparison of the British weights with the French and the Dutch, was published in the Journal of the Royal Institu-

tion, August 1831. He also communicated to this Society, in the year 1821, a memoir relative to the annular eclipse of the Sun, which took place in the preceding year.

His Sovereign, several years ago, conferred upon him the order of knighthood. The citizens of Edinburgh, on the meeting of the British Association in 1834, honoured him with the freedom of their city; and the University that of a doctor's degree. This he had also previously acquired in Holland. In the year 1825 he was offered, but declined, the professor's chair at Leyden, which is considered higher in rank and emolument than that of Utrecht. This caused the heads of the University and City of Utrecht, to testify their sense of this mark of his attachment, by placing at his disposal a considerable sum of money for the extension of his collection of astronomical and other instruments. This valuable collection, together with his library, he has, by will, left to the University. His knowledge of the English language, and his great facility of writing and speaking it, is well known to all who had the pleasure of his acquaintance or correspondence. He also had an equal command of the French and German languages. Those persons who had ever enjoyed the honour of his friendship, must have witnessed the great extent and variety of his knowledge, his wonderful power of memory, and all those social qualities which rendered him a most agreeable companion; and those who knew him most intimately can bear testimony to the kindness of his heart.

The tenth volume of the Memoirs of the Society is now in the press, and it is hoped will soon be completed. It will contain, amongst other valuable papers, two which are the results of observations made at the Observatory at the Cape of Good Hope: one by Mr. Henderson, on the declination of the principal stars in the southern hemisphere; and the other by Mr. Maclear relative to the re-appearance of Halley's comet after its perihelion passage. These two papers, although forming a portion of the forthcoming volume, have been printed wholly at the public expense: an evident proof, amongst others that will be mentioned in the course of this Report, of the encouragement which her Majesty's Government is disposed to give to science, when favourable opportunities offer for their assistance and support.

The Council have great pleasure in announcing to the meeting that Dr. Lee, the Treasurer, has made a voluntary grant to the Society of the valuable Advowson of Hartwell, in Buckinghamshire; and that the deed of gift, conveying the same, has been duly enrolled in the Court of Chancery, and subsequently presented to the Council. The Council duly appreciate the motive which led Dr. Lee to take this liberal step; and the Society must perceive in this, as on the occasion of a former munificent donation, an earnest desire, not only to promote the original objects of the Institution, but also to add to the comfort and enjoyment of those who are associated with us.

A present of a different kind has also been made to the Society,

by the President, of an original painting of Professor Schumacher, by M. Jensen, of Copenhagen. This picture is placed in the room in which you are now assembled, and will often call to mind the many obligations that the science owes to the distinguished character which it represents.

The Council have also great pleasure in calling the attention of the meeting to the portrait of the President, Mr. Francis Baily, presented to the Society by the subscribers, and now placed in the meeting room. Those who are acquainted with the origin and progress of the Astronomical Society, well know that its existence, growth, and present flourishing condition, are chiefly due to the guardian care and constant superintendence of Mr. Baily. Every volume of our Memoirs, every investigation or publication which we have undertaken, every Report which has been drawn up in our name, bears witness to his talents and activity, and to the useful direction of his powers. Let us hope that we may long enjoy his presence; and that similar temper, energy, and good sense, may perpetually preside over our deliberations and conduct.

For the last two years, the Council have expressed a desire of seeing the interesting experiments of Mr. Cavendish repeated, for determining the mean density of the earth; accompanied with a hope that some steps might soon be taken for putting it into execution. A fair prospect is now held out that this important object will be accomplished, her Majesty's Government having appropriated the sum of £500 towards carrying it into effect. The apparatus is nearly finished; and the President, at whose house it is now in the course of being erected, will commence the experiments as soon as it is completed. Although there is no reason to question the accuracy and intelligence with which Mr. Cavendish conducted his experiments, nor the general result which he deduced therefrom, yet, in a matter of so much importance to the theoretical and practical philosopher, it has been considered highly desirable to have those experiments not only repeated, but *extended*, with all the advantages which modern improvements in the art of fabricating instruments for such a delicate inquiry afford.

The Council have great pleasure in announcing two undertakings that must be interesting to every practical astronomer. These are, the reduction of the stars in the *Histoire Céleste*; and the recomputation, and considerable extension, of the Society's Catalogue of Stars. The funds necessary for the accomplishment of these objects (£500 for each), were voted by the British Association for the Advancement of Science, at their meeting at Liverpool, in September last; and the requisite steps have already been taken for carrying them into execution. The *Histoire Céleste*, in its present state, is almost a closed book to the scientific public, and cannot be satisfactorily consulted, even by the experienced astronomer, without the employment of much time and computation; on which account it is seldom appealed to, except on special occasions. By the proposed reduction, however, every star in that immense

collection will be rendered immediately available, and applicable to the purposes of the astronomer, not only by the usual arrangement in the order of right ascension, but also (it is hoped) by the addition of the annual precession of each star, and the *secular variation* of such precession. The extension of the Catalogue of this Society will consist in adding upwards of three or four thousand stars to the present number; so that it will contain nearly the whole of the stars, in both hemispheres, that are not less than the sixth magnitude, and many of the seventh, including every star that is known to have, or suspected of having, proper motion, or any other peculiar circumstance of position or discordance. The reductions will be made to the epoch 1850; and the Catalogue will contain not only the usual constants for obtaining the apparent places of the stars, but also the proper motions (when attainable) and the *secular variations* of the precessions.

Another subject, connected with the advancement of astronomy, that was brought before the same meeting at Liverpool, was the reduction of all the lunar observations made at the Royal Observatory at Greenwich. These observations have never yet, as a *whole*, been made available to the perfecting of the tables of the moon; and, although the Association, who had (as just mentioned) already voted considerable sums for astronomical purposes, did not judge it expedient to incur this very great additional expense, yet they appointed a Committee to wait on the Chancellor of the Exchequer, with a request that he would adopt and promote the object in view. The deputation was very favourably received, and there is every reason to believe that the object in question will be attained; thus affording another and a splendid instance of the cordial co-operation of Government in the advancement of science, when the importance and value of the subject is clearly and distinctly stated.

At the anniversary meeting in 1833, the Council announced the completion and printing of an extensive Catalogue of Northern Stars, deduced from the valuable observations of the late Mr. Groombridge. Prior to its publication, however, an examination of the work, in its printed form, led to the suspicion that sufficient care had not been taken in obtaining the final reductions. The Lords Commissioners of the Admiralty, therefore, appointed a Committee to examine into this matter; and, in consequence of their report, which fully confirmed the suspicions above alluded to, their lordships were pleased to direct that the whole of the computations should be revised. This has accordingly been done, under the superintendence of the present Astronomer Royal; and, as the errors and omissions were found to be too numerous, and of too much importance, to be inserted in a list of errata, the Catalogue, in its amended form, has been wholly reprinted, and will now very shortly be published. The astronomical world is highly indebted to their lordships, not only for their attention to the wants of science, in ordering the Catalogue, in the first instance, to be completed, but more especially in their readiness to remove every ground of suspicion from a work that must be so highly useful to astronomers,

and which, in its present state, will reflect so much credit on the labours of Mr. Groombridge.

As Mr. Groombridge had expressed a wish, during his lifetime, that the MS. observations, from which the Catalogue has been deduced, should be deposited with this Society, and as the Council has consented to take charge of them, the Lords Commissioners of the Admiralty have been pleased to direct that they should be placed in the custody of this Society; and the thanks of the Society are, consequently, due to their lordships for their ready acquiescence with Mr. Groombridge's request.

The Ephemeris of Halley's Comet, mentioned in the two preceding Reports, together with the equations of conditions for correcting the elements of the assumed orbit by observation, has been published during the past year, as part of the Nautical Almanac for 1839. While this truly remarkable work was yet incomplete, the Council gave an opinion on the zeal which had prompted the undertaking, and the talent and energy which had characterised its progress; for the justification of which they can now confidently appeal to the published record. In once more declaring their opinion that in no one instance have such powerful means ever been furnished to the observer, for making his observations of a comet available to the determination of its orbit, the Council remember, with increased satisfaction, the long connexion of Lieutenant Stratford with this Society, and congratulate him and his assistants upon the admirable results which they have produced.

In the Annual Report of the Council last year, it was announced that the Assistant Secretary was engaged in making a catalogue of the books in the library. This has since been completed, and the Council have directed it to be printed, and distributed to such members as may wish to have a copy. The work is now in the press, and will soon be finished.

The Council regret that they have to announce the resignation of the Assistant Secretary, Mr. Epps, who has hitherto filled that office with so much credit to himself, and so much zeal for the welfare of the Society. Mr. Epps will carry with him the good wishes of every member, and an earnest desire that he may long enjoy, in health and happiness, the new situation to which he has been invited.

In the last Report, the Council also alluded to the expected arrival of the astronomical and pendulum observations of the late lamented Lieutenant Murphy, who died during the Euphrates expedition under Colonel Chesney. These observations have since been received and transmitted to the Council by the Board of Control, with a request that this Society would examine and report on them. The Council, therefore, desirous of assisting, as much as possible, in furthering the objects of science, and of shewing every mark of respect to the talents and exertion of their late respected fellow-labourer, readily acceded to this request. The astronomical observations h[illegible] ently been placed in the hands of the Rev. R. Sh[illegible] he pendulum observations in the hands

of Mr. Baily, who have severally undertaken to examine and reduce them.

Titles of Papers read before the Society, between February 1837 *and February* 1838.

1837.

Mar. 10. Results of the Observations of the Sun, Moon, and Planets, made at Cambridge Observatory, in the years 1833, 1834, and 1835. By the Astronomer Royal.

On the Effects of Errors of Adjustment of Azimuth and Level, when the Transit Instrument is used for finding Time and Latitude, by Observations on the Prime Vertical. By Lieut. Raper.

Determination of the Longitude of the Edinburgh Observatory, from all the correspondent Observations of Moon-culminating Stars, made at Greenwich and Edinburgh, between October 18, 1834, and May 27, 1836. By Mr. Riddle.

Occultation of Mars by the Moon, on Feb. 18, 1837. By Mr. Baily, President of the Society.

Occultation of Mars by the Moon, observed at Makerstown. By Sir T. M. Brisbane. With Observations of the Aurora on the same night.

Occultations of *o Piscium*, Jan. 13; and of *Mars*, Feb. 18, 1837. By Mr. Snow. With Observations of the Aurora Borealis on the same night.

Observations of Stars with the Moon at the Observatories of Greenwich, Edinburgh, and Cambridge, in January and February 1837.

Occultations observed at Blackheath, from April 1836 to February 1837. By Mr. Wrottesley.

At the Meeting on this evening a new Telescope, made by M. Plössel, of Vienna, and belonging to Mr. Talbot, called a *dialytic* Telescope, formed on the principle announced by Mr. Rogers, in Vol. III. of the Memoirs of the Society, was exhibited.

Mr. Baily laid on the table a specimen of some delineations of Stars in the vicinity of the North Pole, made with Steinheil's *Astrograph*. An ingenious instrument, not before introduced into this country.

April 14. On the Declinations of the principal Fixed Stars, deduced from Observations made at the Cape of Good Hope, in the years 1832 and 1833. By Mr. Henderson.

Observations on Halley's Comet, from Feb. 16 to May 5, 1836, made at the Cape of Good Hope. By Mr. Maclear; accompanied with Drawings by Mr. Charles Piazzi Smyth.

1837.

Occultation of *Mars* by the Moon, Feb. 18, 1837, observed at the Brussels Observatory, by Professor Quetelet, the Director.

Description of a Spring Level. By Mr. Donkin, accompanied with Drawings.

Stars observed with the Moon at the Observatories of Greenwich, Edinburgh, and Cambridge, in March 1837.

At the Meeting on this evening, Mr. Baily presented to the Society an original Portrait of Professor Schumacher, painted by Professor Jensen.

May 12. On Astronomical Refractions near the Horizon. By Mr. Henderson.

Observations of Halley's Comet, from Feb. 19 to March 21, 1836, made at the Observatory, Madras. By Mr. T. G. Taylor.

Observations of the Solar Eclipse, May 15, 1836. By Mr. Shearman.

Observations of the Annular Eclipse of the Sun, May 15, 1836. By Lieut. Hopkins, R.N. at North Shields.

On the position of the Ecliptic, as inferred from Observations made with the Cambridge Transit and Mural Circle, in 1835. By the Astronomer Royal.

On the Longitude of the Observatory of Leyden. By Dr. Kaiser.

Description of a Pendulum Artificial Horizon, to be attached to a Sextant or Quadrant. By Lieut. Becher, R.N.

Mr. Baily read an extract from a letter, announcing that M. Argelander had deduced, from the proper motion of 390 Stars, that the Solar System is moving to a point very near that which was supposed by the late Sir W. Herschel.

Stars observed with the Moon at the Observatories of Greenwich, Edinburgh, and Cambridge, in April 1837.

Amongst the Presents this evening, was the singular Clock made by the celebrated Mr. John Harrison, which was presented to the Society by Mr. Barton of the Royal Mint.

June 9. On the non-existence of the Star 42 *Virginis*. By Mr. Baily, President of the Society.

A Method of clearing the Lunar distance from the effects of parallax and refraction. By Mr. Templeton.

On the Node and Inclination of the Orbit of *Venus*. By Mr. Main.

Occultation of ω^1 *Cancri*, Dec. 24, 1836, at Ashurst; and of *c Tauri*, April 10, 1837, at Dulwich. By Mr. Snow.

1837.

Occultation of *Mars*, Feb. 18, 1837, at Aberdeen. By Mr. Cruikshanks.

Detailed Observations of the Solar Eclipse of May 15, 1836, and Immersion of *Jupiter's* 1st Satellite, August 20, 1837, at Shooter's Hill. By Colonel Hodgson.

Observation of the Solar Eclipse of May 15, 1836, at Halifax. By Mr. J. Waterhouse.

Observations of the Solar Eclipse of May 15, 1836, at Tranby. By Mr. Samuel Cooper.

On Napier's Five Circular Parts. By Mr. R. Abbatt.

Stars observed with the Moon at the Observatories of Greenwich, Edinburgh, and Cambridge, in May 1837.

Nov. 10. On the parallax of α *Lyræ*. By the Astronomer Royal.

The constant quantity of the Moon's Equatorial Horizontal parallax, from Observations made at Greenwich, Cambridge, and the Cape of Good Hope, in 1832 and 1833. By Professor Henderson, Astronomer Royal, Scotland.

Stars observed with the Moon at the Observatories of Greenwich and Cambridge, in June 1837.

Dec. 8. Observations of the Solar Eclipse of May 15, 1836, at the Observatory of San Fernando. By M. Cerquero.

Stars observed with the Moon at San Fernando in 1835 and 1836. By M. Cerquero.

A List of 285 Stars of the Society's Catalogue, which are Double. By Mr. Holehouse.

On a very ancient Solar Eclipse observed in China. By R. W. Rothman, Esq.

On the Repetition of the Cavendish Experiment for determining the Mean Density of the Earth. By the President.

Stars observed with the Moon at the Observatories of Greenwich, Edinburgh, and Cambridge, in July, August, and September, 1837.

1838.

Jan 12. Occultations of δ *Arietis*, August 21; C *Tauri*, August 24; δ *Arietis*, October 15; *f Aquarii*, November 6; and Immersion of ψ^1 *Aquarii*, November 7, 1837. By R. Snow, Esq.

On the difference of Longitude between the Greenwich and Paris Observatories, ascertained by Chronometers. By Mr. E. J. Dent. Communicated by the Astronomer Royal.

Observations of the Lunar Eclipse on October 13, 1837. By Capt. Basil Hall, R.N.

List of Public Institutions, and of Persons, who have contributed to the Society's Library, &c. since the last Anniversary.

American Philosophical Society.
Society of Arts.
Royal Asiatic Society of London.
Asiatic Society of Bengal.
Editor of the Athenæum Journal.
Royal Academy of Sciences of Berlin.
British Association.
Royal Academy of Sciences of Brussels.
Cambridge Philosophical Society.
Royal Society of Edinburgh.
Institution of Civil Engineers.
L'Académie des Sciences de l'Institut de France.
Le Bureau des Longitudes de France.
Society of Geneva.
Royal Geographical Society.
Geological Society of London.
Honourable East India Company.
Royal Irish Academy.
Italian Society at Modena.
Linnean Society of London.
Royal Society of London.
Imperial Academy of Sciences of St. Petersburg.
United Service Museum.
Zoological Society of London.

G. B. Airy, Esq. Ast. Roy.
Professor Amici.
F. Baily, Esq.
W. H. Barton, Esq.
MM. Beer and Mädler.
M. Biot.
M. Cerquero.
Dr. Ceschi.
Professor Challis.
S. H. Christie, Esq.
Samuel Cooper, Esq.
A. De Morgan, Esq.
G. Dollond, Esq.
Professor Encke.
Dr. Fitton.
W. Frend, Esq.
T. Galloway, Esq.
Capt. Basil Hall, R.N.
Professor Hassler.
G. Hutchison, Esq.
G. Innes, Esq.
Professor Kupffer.
Dr. Lamont.
Professor Littrow.
J. Lockhart, Esq.
J. W. Lubbock, Esq.
Professor Moll.
Professor Plana.
Professor Quetelet.
Professor Rigaud.
J. Robotham, Esq.
Professor Santini.
Professor Schumacher.
Lieut. Stratford, R.N.
R. Taylor, Esq.
John Williams, Esq.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected.

President: Francis Baily, Esq. V.P. and *Treas.* R.S. F.L.S. & G.S. and M.R.I.A.—*Vice Presidents:* George Biddell Airy, Esq. M.A. F.R.S. *Astronomer Royal;* Thomas Galloway, Esq. M.A. F.R.S.; Lieut. Henry Raper, R.N.; John Wrottesley, Esq. M.A. —*Treasurer:* John Lee, Esq. LL.D.F.R.S —*Secretaries:* George Bishop, Esq.; Augustus De Morgan, Esq.—*Foreign Secretary:* Captain W. H. Smyth, R.N. F.R.S. & A.S.—*Council:* Timothy Bramah, Esq.; Rev. James Challis, M.A. *Plum. Prof.*, Cambridge; Lieut. W. T. Denison, R.E.; Rev. George Fisher, M.A. F.R.S.; Davies Gilbert, Esq. V.P.R.S. L.S. & G.S.; Sir J. F.W. Herschel, K.G.H. M.A. F.R.S.; Rev. Robert Main, M.A.; Edward Riddle, Esq.; William Simms, Esq.; Lieut. W. S. Stratford, R.N. F.R.S.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. IV. *March* 9, 1838. No. 13.

The following communications were read :—

I. Immersion of λ *Cancri* at Moon's dark limb, March 6, 1838. By R. Snow, Esq. Corrected sidereal time, $13^h\ 14^m\ 54^s$.

II. Extract of a letter from Sir John Herschel to the President, giving an account of a remarkable increase of magnitude of the star η in the constellation *Argo*, observed by him at the Cape, December 16–17, 1837.

"I have just observed a very remarkable phenomenon, the developement of which I am watching with much interest. It respects the nebulous star η in the constellation *Argo*, No. 1281 of the Catalogue of the Astronomical Society; marked in that catalogue as of the second magnitude. As such, or rather as intermediate between the first and second, as a very large star of the second magnitude, or a very small one of the first, I have always hitherto observed it, having, in some cases, equalised it with *Fomalhaut*; in others placed it intermediate between α and β *Crucis*, nearly equal with the latter, &c.; nor have I at any time had reason to suppose its magnitude variable. To-night, however, being at work on my classification of the southern stars in order of their magnitudes, I was much astonished to find its magnitude superior, not only to that of *Fomalhaut* and α *Crucis* (with which stars it no longer admits of a moment's comparison), but even to that of *Aldebaran*, *Procyon*, α *Eridani*, α *Orionis*, and little if at all inferior to that of *Rigel*.

"This was my own judgment, and that of several persons whom I called to my assistance, in the early part of the night, when η was low and *Rigel* high in the heavens. At the time I write, they have about equal altitudes, and the comparison is decidedly in favour of η, which is, in fact (*Sirius* and *Canopus* excepted), the most brilliant star now visible; α *Centauri* being too low for fair comparison, and veiled with some degree of haze.

"This remarkable increase of magnitude has come on very suddenly, as my attention has frequently of late been drawn to this star in the lower part of its diurnal circle, while watching with some impatience its progress towards the meridian, at a reasonable hour of the night, that I might resume and complete, before my departure hence, a very elaborate monograph of the wonderful nebula which surrounds it. A few evenings before the full moon

just passed, in particular, I remember to have noticed it with this view; and had it then been what it now is, a star of the first class, it could not have passed unremarked.

"Whether it be now at its maximum, and about to decrease by insensible degrees; whether, like *Algol*, but in a much longer time, it remains as it were dormant through the greater part of its period, and runs through its phases of increase and decrease in a small aliquot portion of the whole; or whether, lastly, it be on the point of blazing forth with extraordinary splendour, so as possibly to outshine its brilliant neighbours, α *Centauri* and *Canopus*, it is useless to conjecture, and observation will soon determine."

III. Value of the Mass of *Uranus*, deduced from Observations of its Satellites, made at the Royal Observatory of Munich during the year 1837. By Dr. F. Lamont, Director of the Royal Observatory.

In the course of the year 1837, a few favourable nights were employed in taking observations of the satellites of *Uranus*, with a view of calculating the value of the planet's mass; and, though the object has not been satisfactorily attained, owing to the difficulty of the observations, and the present unfavourable position of the orbits of the satellites, the result is not without interest, as it leads to the conclusion that the true value of the mass of *Uranus* is considerably smaller than that which is generally adopted.

The instrument used in the observations was a refractor constructed at Munich, having a focal length of 15 feet, and an aperture of 10½ inches, Paris measure. The fact of its having served to measure the distances of the satellites of *Uranus* is sufficient evidence of its superior power. Dr. Lamont acknowledges, however, that, notwithstanding the great optical power of the telescope, he has not, as yet, been able to discover more than three of the satellites, namely, the second, the fourth, and the sixth.* The sixth was observed only once, and the observation is of consequence omitted, as being of no use in the present inquiry.

The measures were obtained by using a parallel wire-micrometer of Fraunhofer's construction, having the wires, not the field, illuminated. Instead of the lamps usually employed, a light placed at a distance was reflected on the wires by a small mirror. Dr. Lamont remarks, that the use of a mirror is greatly to be preferred to lamps, because, in addition to its being more convenient, it affords in the measurement of faint objects a peculiar advantage, in enabling the observer to direct the illumination to any part of the wires, and with any degree of intensity that may be required.

But, however carefully the illumination may be managed, it would be impossible to bisect with a wire any of the satellites of *Uranus*, and, accordingly, Dr. Lamont had recourse to another

* The satellites are named in the order of their distances from the planet, so that those which are here termed the *second* and *fourth*, correspond respectively to the *first* and *second* of Sir W. Herschel.

method, which he has frequently adopted in similar cases. Placing the fixed wire so as to bisect the disc of the planet, he moved the micrometer until the satellite appeared exactly in the middle of the space between both wires. The measure being repeated on the opposite side of the fixed wire, in order to eliminate the Zero point, the difference of the two readings gave the quadruple distance of the satellite.

The table of observations given by the author contains the sidereal time of each observation; the mean Paris time, including aberration; the observed angles of position; the observed distances, and the number of measures taken at each observation. The number of observations of the second satellite was 11, and of the fourth, 15.

Although the observations furnish sufficient proof of the elliptic motion of the satellites, any attempt to investigate the elliptic elements from the few data obtained in the present unfavourable situation of the orbits, would be unavailing. Dr. Lamont, therefore, assumes the satellites to move in circular orbits, in a plane having, as computed by Sir W. Herschel, an inclination of 101° 2′ to the ecliptic, the longitude of the ascending node being 165° 30′; and on this hypothesis proceeds to compute from the observations the distances and times of revolution of the two satellites. The results of the computation are as follows:

	Distance.	Periodic Time.
Second satellite	31″·35	8^d,705886
Fourth ditto	40 ·07	13 ,463263

Having found the distances and periods of revolution, it remains to compute the value of the Planet's mass. It is found, however, that the values derived from both satellites exhibit a considerable difference, as might indeed be expected, when it is considered that the mean distances are the result of a small number of observations calculated upon the gratuitous supposition of circular orbits. On diminishing the radius of the second satellite by 0″·79, and augmenting that of the fourth by the same quantity, in order to make the distances accord with the periods of revolution, the value of the mass of *Uranus* is found $= \frac{1}{24605}$, being less by one-fourth part than that obtained by M. Bouvard, from the perturbations produced by the planet.

Dr. Lamont remarks that, in giving this value he is too well aware of the uncertainty of the data on which it rests to attach any particular weight to it; but, though he considers the measures obtained by him in 1837 as being unfit, until combined with further observations, to give a value of the mass of *Uranus* that might, with confidence, be employed in the theory of the planetary motion, there is one purpose they will serve at present, namely, to enable us to judge whether a given value of the mass of *Uranus* can be regarded as probably true or not by its agreement or disagreement with them.

Bouvard's value, which is generally adopted, is $\frac{1}{17918}$. Computing from this the mean distances of the two satellites, the distance of the planet from the earth being assumed = 19·223, he finds the mean angular distance of the second satellite = 33″·96, and that of the fourth = 45″·42. The difference of these values from those computed from the observations, is +2″·61 and +5″·35. Now, without entering into the theory of probable errors, it will readily be granted, he conceives, on considering the differences of the individual errors from the mean, and even allowing a probable error for eccentricity, that scarcely an error of 2″, much less one exceeding 5″, can be attributed to the distances obtained from observation. The supposition of a constant error in the instrument can scarcely be admitted; for, on measuring distances that had otherwise been determined with precision, no constant error has been found to exist.

In conclusion, the author states that he considers it certain that the value of the mass of *Uranus*, at present used in the theory of the planetary perturbations, ought to be greatly diminished, though the precise proportion in which this should be done, cannot at present be assigned. Considering the difficulty of the observations, and the small number of nights in which measures of so much delicacy can be made, it will not be possible, within the period of several years, to deduce the true value of the planet's mass from the elongations of the satellites.

IV. Stars observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the months of October, November, and December, 1837.

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Cambridge.	Edinburgh.
		h m s	h m s	h m s
Oct. 3	Moon 1 L	...	15 22 29,20	...
	β^1 Scorpii	...	15 55 59,83	...
8	c Sagittarii ...	...	19 52 40,22	19 52 40,10
	σ Capricorni	20 10 1,44	20 10 1,48	20 10 1,19
	Moon 1 L. .	20 34 31,12	20 34 30,00	20 35 4,52
	η Capricorni	20 55 9,95	20 55 10,06	20 55 9,88
	ζ Capricorni	21 17 23,81	21 17 24,21	21 17 24,02
9	η Capricorni	20 55 9,94	20 55 10,15	...
	ζ Capricorni	21 17 23,98	21 17 24,29	...
	Moon 1 L. .	21 35 41,73	21 35 41,19	...
	ι Aquarii	21 57 40,54	...	...
	θ Aquarii	22 8 16,59	...	...
11	λ Aquarii	22 44 9,49	...	22 44 9,53
	ψ^3 Aquarii ...	23 10 31,86	23 10 32,09	23 10 31,74
	Moon 1 L.	23 28 51,44	23 28 50,50	23 29 20,36
	n Piscium ...	23 39 37,11	23 39 37,08	23 39 36,82
	τ Piscium ...	...	23 53 39,39	23 53 39,20

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Cambridge.	Edinburgh.
		h m s	h m s	h m s
Oct. 12	n Piscium	...	23 39 37,05	23 39 36,71
	τ Piscium	23 53 39,08	23 53 39,43	23 53 39,00
	Moon 1 L.	0 22 28,47	0 22 27,52	0 22 56,77
	Piscium	0 39 53,52	0 39 53,73	0 39 53,30
	ε Piscium	0 54 32,70	0 54 32,58	0 54 32,31
13	Piscium	...	0 39 53,77	0 39 53,29
	ε Piscium	...	0 54 32,74	0 54 32,33
	Moon 2 L.	...	1 17 58,54	1 18 27,17
	η Piscium......	...	1 22 49,83	1 22 49,24
	γ^1 Arietis	...	1 44 39,17	1 44 38,89
14	η Piscium	...	1 22 49,61	...
	γ^1 Arietis	1 44 39,04	1 44 39,29	...
	Moon 2 L...	2 12 0,60	2 12 0,07	...
	σ Arietis	...	2 40 16,04	...
	ε Arietis	...	2 49 57,63	...
16	A^2 Tauri	3 55 44,84	...	...
	Moon 2 L.	4 5 7,93	...	...
18	Moon 2 L.	6 3 13,57	...	...
	ε Geminorum	6 33 56,84	...	...
21	γ Cancri	...	...	8 33 52,78
	Moon 2 L..	...	...	8 49 41,71
	ξ Cancri	...	...	9 0 0,51
	λ Leonis	...	...	9 22 26,50
Nov. 3	σ Sagittarii ...	...	...	18 45 11,21
	Moon 1 L..	19 10 58,80	...	19 11 33,38
	h^2 Sagittarii ...	19 26 48,86	...	19 26 49,10
	c Sagittarii ...	19 52 39,69	...	19 52 39,90
4	h^2 Sagittarii ...	...	...	19 26 48,82
	c Sagittarii ...	...	...	19 52 39,96
	Moon 1 L..	...	...	20 15 34,59
	ψ Capricorni	...	...	20 36 28,41
	η Capricorni	...	...	20 55 9,59
5	ψ Capricorni .	...	20 36 28,61	20 36 28,44
	η Capricorni	...	20 55 9,59	20 55 9,56
	Moon 1 L.	21 16 0,89	21 15 59,74	21 16 31,95
	ε Capricorni	21 27 59,31	21 27 59,71	...
	δ Capricorni	21 38 4,94	21 38 4,90	21 38 4,93
6	δ Capricorni .	21 38 4,84	...	...
	Moon 1 L.	22 13 25,92	...	...
	σ Aquarii	22 22 3,71	...	...
	λ Aquarii ...	22 44 9,24	...	...
7	σ Aquarii	22 22 3,68	22 22 3,79	...
	λ Aquarii	22 44 9,27	...	...
	Moon 1 L..	23 7 47,87	23 7 47,02	...
	n Piscium......	23 39 36,63	23 39 36,77	...

The anonymous star observed (at Greenwich) on October 12, A. S. C. 79, has an erroneous right ascension in the *Nautical Almanac*, derived from an erroneous right ascension in the Astronomical Society's Catalogue. See the *Cambridge Observations*, 1831.

On October 16, A^2 *Tauri* was observed by mistake instead of A^1 *Tauri*.

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Cambridge.	Edinburgh.
		h m s	h m s	h m s
Nov. 8	*n* Piscium......	23 39 36,72	...	...
	Moon 1 L..	0 0 12,46	0 0 11,64	...
	t Piscium	...	0 17 6,53	...
	Piscium	...	0 39 53,75	...
9	*t* Piscium	...	...	0 17 5,94
	Piscium	...	...	0 39 53,36
	Moon 1 L.	...	...	0 52 25,70
	μ Piscium......	...	...	1 21 42,32
	ν Piscium	...	...	1 33 0,55
10	*μ* Piscium......	...	1 21 42,60	...
	ν Piscium	...	1 33 0,93	1 33 0,68
	Moon 1 L..	...	1 44 21,50	1 44 50,63
	*ξ*¹ Ceti	...	2 4 25,68	...
	γ Arietis	...	2 29 38,07	2 29 38,02
11	*ξ*¹ Ceti	...	...	2 4 25,70
	γ Arietis	...	..	2 29 38,10
	Moon 1 L..	...	..	2 38 58,17
	δ Arietis	...	...	3 2 22,86
	g Arietis	...	...	3 14 45,68
12	*δ* Arietis	...	3 2 23,05	3 2 22,92
	g Arietis	3 14 45,58	...	3 14 45,83
	Moon 2 L...	3 37 15,80	3 37 15,24	3 37 46,18
	A¹ Tauri	...	3 55 7,87	3 55 7,98
	*υ*¹ Tauri	...	4 16 37,99	4 16 37,90
16	*δ* Geminorum	7 10 26,44	...	...
	Moon 2 L..	7 33 2,67	...	...
17	Moon 2 L..	8 27 28,11	...	...
	ξ Cancri	9 0 1,68	...	...
19	Moon 2 L..	10 6 11,84	10 6 11,42	10 6 36,29
	ζ Leonis	10 24 15,70	10 24 16,02	10 24 15,69
	k Leonis	10 37 49,18	10 37 49,42	10 37 49,25
20	*ζ* Leonis	...	...	10 24 15,39
	k Leonis	...	10 37 49,35	10 37 48,85
	Moon 2 L.	10 51 40,68	10 51 39,92	10 52 3,91
	n Leonis	11 7 21,86	11 7 22,24	11 7 21,92
	τ Leonis	...	11 19 35,24	11 19 35,07
Dec. 2	*ψ* Capricorni	...	20 36 28,22	...
	Moon 1 L..	...	20 57 38,25	20 58 11,60
	ζ Capricorni ..	...	21 17 23,38	...
	γ Capricorni ..	...	21 31 5,39	21 31 5,26
3	*ζ* Capricorni ..	...	...	21 17 23,16
	γ Capricorni ..	...	...	21 31 5,27
	Moon 1 L.	...	...	21 56 59,24
	θ Aquarii ...	...	...	22 8 15,82
	σ Aquarii ...	...	...	22 22 3,35
4	*θ* Aquarii ...	...	...	22 8 15,75
	σ Aquarii ...	...	...	22 22 3,28
	Moon 1 L.	...	...	22 51 54,18
	*ψ*³ Aquarii ...	...	...	23 10 31,27

1837.	Object.	Apparent Right Ascension.		
		Greenwich.	Cambridge.	Edinburgh.
		h m s	h m s	h m s
Dec. 5	ψ^2 Aquarii ...	...	...	23 10 31,31
	κ^1 Piscium ...	...	...	23 18 37,04
	Moon 1 L.	...	...	23 44 2,08
	s Piscium ...	...	...	23 57 2,14
	t Piscium ...	...	...	0 17 5,64
6	*s* Piscium ...	...	...	23 57 2,27
	Moon 1 L...	...	...	0 34 47,32
8	Moon 1 L.	...	2 17 10,98	...
	π Arietis......	...	2 40 16,28	...
	ρ Arietis......	...	2 49 58,21	...
9	π Arietis......	...	...	2 40 16,15
	ρ Arietis......	...	...	2 49 58,16
	Moon 1 L..	...	...	3 11 59,56
	η Tauri	...	...	3 37 52,46
	A¹ Tauri	...	...	3 55 8,19
10	η Tauri	3 37 52,67	...	...
	A¹ Tauri	3 55 8,36	...	...
	Moon 1 L.	4 8 23,66	...	...
11	τ Tauri	...	...	4 32 32,18
	ι Tauri	...	...	4 53 25,64
	Moon 1 L. .	...	...	5 7 56,27
	Moon 2 L. .	...	...	5 10 20,29*
	l Aurigæ......	...	...	5 28 14,84
12	*l* Aurigæ......	...	...	5 28 14,72
	C Tauri	...	...	5 43 9,30
	Moon 2 L...	...	...	6 10 9,36
	ι Geminorum	...	...	6 33 58,26
13	ι Geminorum	...	6 33 58,74	...
	ω^1 Geminorum	...	6 52 33,44	...
	Moon 2 L...	...	7 8 24,45	...
15	δ Cancri	8 35 28,76	...	...
	Moon 2 L...	8 57 16,76	...	...
	q Cancri	9 9 56,38	...	...
	λ Leonis......	9 22 28,46	...	...
18	Moon 2 L...	11 17 3,77	...	...
	υ Leonis	11 28 38,80	...	...
	ν Virginis	11 37 31,50	...	...
20	η Virginis ...	...	...	12 11 36,65
	γ Virginis ...	...	...	12 33 26,54
	Moon 2 L...	...	...	12 44 40,30
	θ Virginis	...	...	13 1 33,26
	α Virginis ...	...	...	13 16 38,95
31	θ Aquarii......	...	...	22 8 15,83
	Moon 1 L...	...	...	22 33 26,54
	λ Aquarii......	...	...	22 44 8,82
	ψ^3 Aquarii	...	...	23 10 30,96

* 0s,02 to be added for defect of illuminated disc.

Errata in Notice for December 1837, Cambridge Observations :—

Sept. 8, Moon 1 L. *for* $17^h\ 41^m\ 11^s,68$ *read* $17^h\ 41^m\ 11^s,60$.
10, Moon 1 L. — $19^h\ 54^m\ 50^s,98$ — $19^h\ 54^m\ 51^s,08$.

The following Notice is inserted at the Request of the ASTRONOMER *ROYAL.*

IMMEDIATELY after sending out a number of copies of Groombridge's Catalogue, I discovered that some errors had been committed in the nomenclature of the stars, with reference, chiefly, to their accordance with the corrections made by Mr. Baily in *Flamsteed's British Catalogue.* These errors arose from the omission of a comparison which was supposed to be fully included in another. I have since made a collation; and I think it probable that the following list contains the whole of these errors.

Groombridge, No. 462 and 463	constitute	59	Andromedæ.
1172	...... is	7	Lyncis.
1297 and 1298	are	20	Lyncis.
1478	is *not*	7	Ursæ Majoris.
1984	is *not*	22	Canum Venaticorum.
2185	is	8	Ursæ Minoris.
2780	is *not*	19	Lyræ.
3196	is *not*	44	Cygni.
3412	is *not*	68	Cygni or Bradley 2775.
3427	is	68	Cygni, Bradley 2775.
4240	is		Bradley 3216.
4241	is		Bradley 3217.

G. B. AIRY.

March 9, 1838.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *April* 13, 1838. No. 14.

The following communications were read :—

I. On the Correction of the Mean Distance, Eccentricity, Epoch, and Longitude, of the Aphelion of the Orbit of Venus, by Errors of Heliocentric Longitude, derived from the Cambridge Observations of the Years 1833, 1834, and 1835, and the Greenwich Observations of 1836. By Mr. Main.

The author states, that being furnished by the Astronomer Royal with his computed errors of heliocentric longitude of the planet *Venus*, derived from four years' unintermitted observations, he undertook to correct the above-mentioned elements by means of them. The tabular errors of longitude were derived by comparing the observed right ascensions and north polar distances with those interpolated from the *Berlin Ephemeris* for 1833, and with the *Nautical Almanac* for the remaining years.

It appears that Lindenau's uncorrected elements were used for the computations of the places of *Venus* in both those works, though a difference between the errors of the aphelion deduced from the observations of 1835 and 1836, and those of the two preceding years, prevented the author from combining all the equations, and induced him, at first, to suppose that a correction had been made in it between the years 1834 and 1835.

Lindenau corrects Lalande's elements, reduced to the epoch 1750, by means of Bradley's Observations. They are given by him as follow :—

Epoch (for meridian of Seeberg)	46° 18′ 18″·8
Eccentricity	0·00688480
Aphelion	308° 12′ 9″·2

By thirty equations, he (Lindenau) finds that the epoch requires a correction of $-3''\cdot7$; the eccentricity, a correction of $+0\cdot00003140$, and the aphelion, a correction of $+493''\cdot6$; and these quantities, when substituted, satisfy his equations pretty well.

His corrected elements he then states to be,

Epoch	46° 18′ 15″·1
Eccentricity	0·00691620
Aphelion	308° 3′ 55″·6

It is evident from this, that the correction has been applied to

the aphelion with the wrong sign, which will make Lindenau's value used in the tables incorrect by double the amount of the correction, or by 16′·5 nearly, supposing his correction well determined.

A similar error seems to have been committed in correcting the aphelion by the Seeberg, &c. Observations, for another epoch, 1808.

The results of the present calculations shew this for the years 1833 and 1834 (though the error is less in amount than that above stated), but not for the remaining years; and the difference between the corrections of the aphelion for 1834 and 1835, is nearly the quantity entailed by Lindenau's error.

The calculations are made as follow:

Let l = true long. of a planet in its orbit
l' = long. measured on ecliptic
M = mean anomaly measured from aphelion
ϵ = epoch of mean longitude
R = projection of rad. vector on ecliptic, in units of earth's mean dist. from sun
β = helioc. latitude
a = semi-axis major, in units of earth's mean distance
e = eccentricity
n = mean annual motion.

Then the following equations result from differentiating, &c. the usual expressions for l' and R, reducing, and substituting the approximate values of a, e, &c. for *Venus:*

$$\delta l' = (1-2e\cos M)\,\delta\epsilon - 206264{\cdot}79\sin M\,(2-5e\cos M)\,\delta e + 2e\cos M\,\delta\varpi$$

$$\text{and}\ \frac{\delta R}{\cos\beta} = (1+e\cos M)\,\delta a + 0{\cdot}72333\,(2e\sin^2 M+\cos M)\,\delta e$$
$$-0{\cdot}0000000239976\sin M\times(\delta\epsilon-\delta\varpi)$$

The values of M were formed from Lindenau's tabulated values of the mean longitude and longitude of the aphelion for each given day, and $\delta l'$ and δR, then carefully tabulated. Equating the above values of $\delta l'$, and those given by the Astronomer Royal, and eliminating δR, fifty equations of condition arise, which, when multiplied by the weights assigned to each, are combined by the simple method of making the coefficient of each variation separately the greatest possible by addition.

The combined equations are given separately for each year, to the end that any suspected error may be more readily detected, and the solutions given for each year separately. They are as follow:

	1833.	1834.	1835.	1836.
δa =	+0·0000203	−0·0000047	+0·0000368	+0·0000236
δe =	−0·0000262	−0·0000223	−0·0000280	+0·0000186
$\delta\epsilon$ =	− 2″·1	− 4″·9	− 3″·6	− 4″·1
$\delta\varpi$ =	−587 ·7	−501 ·3	+328 ·4	−67 ·3

The work is given in as detailed a shape as possible, from the author's conviction of the difficulty of avoiding errors in so long and troublesome a series of calculations, and from his wish that

every facility may be given for a re-examination of any part of it, if thought desirable.

II. Moon culminating Observations made at Rio Janeiro and Valparaiso in 1836. By Captain F. W. Beechey, R.N.

From these observations, compared with the corresponding observations made at Greenwich, Cambridge, Edinburgh, Paris, and the Cape of Good Hope, and with the *Nautical Almanac*, Captain Beechey has deduced the following results.

Longitude of Fort S. Antonio, Valparaiso	71° 39′ 36″·7 W.
Longitude of Anhatomirim, Brazil	43 09 13 ·5 W.

III. Times of Emersion of the first and second Satellites of *Jupiter*, observed at Greenwich Hospital Schools, April 9th, 1838. By E. Riddle, Esq.

First satellite	$7^h\ 16^m\ 8^s$,
Second satellite	7 16 49.

The predicted times in the *Nautical Almanac* are:

First satellite	$7^h\ 16^m\ 0^s,7$
Second satellite	7 16 41 ,5

The first being $7^s,3$, and the second $7^s,5$, earlier than the observed times.

These phenomena deserve notice merely from their happening within so short a time of each other, and from their having been observed earlier in the evening than the superintendent of the *Nautical Almanac* anticipated that they could be seen with advantage in this neighbourhood,—as is inferred from the absence of the usual notifying asterisks in the Almanac.

With the telescope employed, however, — a fine achromatic of 3½ inches' aperture, and 48 inches' focal length, recently made and mounted equatorially by Mr. Simms — the observations were made without difficulty.

IV. Longitude of the Edinburgh Observatory, computed from the corresponding Moon Culminating Observations made at Edinburgh and Greenwich, from August 24, 1836, till the end of 1837. By Mr. Riddle.

The number of observations is 62, and the mean of the whole, allowing each observation weight proportional to the number of stars observed, gives the longitude = $12^m\ 44^s,7$.

In the *Notices* for March 1837, are given the results deduced from all the corresponding observations of the same class, made at the two Observatories under the direction of the present astronomers, before the date of the first in this list. The mean result of these preceding observations was $12^m\ 44^s,5$, differing only $,2^s$ from the present determination.

V. Eclipses of *Jupiter's* Satellites, observed at Edinburgh. By Professor Henderson.

VI. Lunar Occultations observed at Edinburgh. By Professor Henderson.

Date.	True Sidereal Time.	Object.	Phenomenon.	Remarks.
	h m s			
1836. May 26	16 51 12,3	θ Virginis	Im. at dark limb	
1837. Feb. 18	8 23 9,7	Mars	Total im. at do.	
Mar. 16	6 44 43,4	ω^2 Cancri	Im. at ditto	
Oct. 12	20 19 36,1	10 Ceti	Ditto	
Nov. 5	20 48 30,2	35 Capricorni	Ditto	
9	2 22 35,8	73 Piscium	Ditto	
11	2 4 9,7	π Arietis	Ditto	Observation a little uncertain from flying clouds and brightness of moon, which was nearly full.

VII. Immersion of 47 *Geminorum* at Moon's dark limb, April 1, 1838. By R. Snow, Esq. Corrected sidereal time, $14^h\ 25^m\ 4^s,73$.

VIII. The President read an extract of a letter from Mr. Henderson relative to the remarkable increase of magnitude, in η *Argus*, recently noticed by Sir John Herschel, as mentioned at the last meeting of the Society. Mr. Henderson states that the star is not to be found at all in Ptolemy's catalogue, although the bright stars of the Cross and the Centaur, which culminated as low at Alexandria, are inserted in it. From this circumstance he infers that, at this remote period, the star was not very bright. It is not in Bayer's maps; and in Halley's catalogue it is said to be of the *fourth* magnitude, which is less than some of the neighbouring stars that in modern times cannot compete with it. It would thus appear that the star has for a long period been increasing in brightness; and it will be remarkable if it should surpass the brightest at present known.

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *May* 11, 1838. No. 15.

The following communications were read :—

I. On the Constant of Lunar Nutation, as given by the Circle Observations of Mr. Pond, from 1812 to 1835. By T. R. Robinson, D.D., Fellow of the Royal Astronomical Society, &c.

Astronomers, till a late period, have acquiesced in the value assigned to the Constant of Lunar Nutation by its illustrious discoverer, which, in fact, is nearer the truth than many of those which have since been given as improvements of it. We possess, indeed, only two other direct determinations which can be considered of greater authority; the first being that of the Baron Von Lindenau, and the second that of Dr. Brinkley, the late bishop of Cloyne.

Von Lindenau's result, which is universally adopted by the astronomers of Germany, is derived from the Right Ascensions of *Polaris*, in which the effect of nutation is magnified about thirty-three times by its vicinity to the Pole. The observations made before 1750 do not possess the requisite accuracy; but, from that time to 1815, an interval including three revolutions of the node, he was able to find materials for 800 equations. The earlier are Bradley's; the intermediate are from Maskelyne; and the last, his own, Bessel's, Carlini's, and Piazzi's. Grouping this mass into 64, and applying to them the method of minimum squares, he finds for the Constant of Nutation, $8''\cdot97718$.

Although this work of Von Lindenau is certainly well worthy of its author's fame, it is liable to some objections. The Solar Nutation (corresponding to 9·64) is certainly too small; and the terms depending on 2 ☊ and 2 ☽ cannot be neglected so near the Pole. It is also obvious, that these 800 equations are scarcely sufficient to determine five unknown quantities with extreme precision. Some doubt may be excited by variety of observers and instruments; but the weightiest objection is the uncertainty of transit observations very near the Pole, which

is so great as to balance the advantage resulting from the large coefficient of nutation. Even in the latest of the observations employed, differences of three and four seconds of time are not uncommon.

The value of the Constant found by Dr. Brinkley, and generally adopted by British astronomers, is 9·25. This result was deduced by Dr. Brinkley from his own observations with the Dublin Circle (*Phil. Trans.* 1821). They include only half a revolution of the node; but he connects them by Bessel's precession and his own proper motions. The number of observations is 1618; the solar nutation used is 0″·48, and the terms 2 ☊ and 2 ☽ are also omitted by him; but their effect must be almost insensible.

The difference between this result and that of Von Lindenau, nearly three-tenths of a second, is far too considerable to be tolerated in the present state of Astronomy; and it was suggested by Mr. Baily, that the observations made at Greenwich with the mural circle, embracing a period of more than twenty years, might remove the uncertainty. Influenced by the wish of this distinguished astronomer, the British Association, at its session of 1834, entrusted the task to the author of the present paper, and afforded, by a pecuniary grant, the means of executing it.

An observed declination is effected with

1. Index error,
2. Refraction,
3. Precession,
4. Proper Motion,
5. Aberration,
6. Nutation;

for which it must be corrected before it can be compared with another made at a different epoch, and the elements of these corrections require explanation in the first instance.

Index Error.—The Mural Circle, at the time of its invention, was supposed to be independent of any reference to the horizon, the pole being its zero. This point is determined by observations of circumpolar stars that are *conjugate* (that is, combined superior and inferior passages separated by so small an interval that there can be no suspicion of any change in the instrument). From 1812 to 1825, therefore, the index error is deduced from conjugate observations of *Polaris*, which, during that period, are sufficiently numerous for the purpose. During this period the telescope was often shifted on the circle, and the microscopes were altered. In a very few instances, when conjugate sets could not be obtained for some time, the permanence of the index correction was inferred from the series of polar distances.

About the year 1825, Mr. Pond first began to use a horizontal point, obtained by combining direct and reflected observations. After 1825, therefore, the correction is taken from the polar and horizontal points conjointly. For this purpose it was necessary to connect them; and, in order to have a uniformity of elements, Dr. Robinson deduced the horizontal point from the reflected

observations of Polaris alone, using such as are synchronous with conjugate observations. He thus obtained for the polar distance of the Greenwich horizon from

133 pairs above the pole	128° 31′ 21·976″
126 pairs below	22·315
Mean	128 31 22·15

a quantity but 0″·24 greater than what Pond obtained from his observations of *γ Draconis* with the circle and zenith sector, allowing for the differences of the refractions used. The difference between this and the horizontal points given in the Greenwich Observations for Troughton's Circle is assumed as the index correction for the horizon zero. In combining these with the polar corrections, both are considered of equal weight, as the reflected observations do not yield in accuracy to the direct.

2. *Refraction.* — The table used for refraction is slightly changed from Brinkley's. Brinkley had found, that for his Observatory and instruments, the Constant of the French tables gave, with the interior thermometer, perfect results. Dr. Robinson found it necessary to use the exterior thermometer; but, in consequence, it was no longer possible to involve in the factor for temperature, the correction of the barometer for the expansions of its scale and mercury. A slight variation was also made in the Constant to allow for the variation of gravity, as the data from which it is derived belong to latitude 45°. It must be apparent, however, that from the considerable altitude of the stars employed, any uncertainty of refraction can produce no error in the final results.

3, 4. *Precession. Proper Motion.* — Bessel's precession requires no change. His proper motions must also be very exact; but, as the observations, in general, include a cycle of the Moon's nodes, they have not been relied on, but introduced as unknown quantities into the investigation. In two cases, the proper motions have been deduced by comparing Mr. Airy's places, given in the Greenwich Observations, 1836, with Bradley's, in the *Fundamenta Astronomiæ.*

5. *Aberration.*—The value of the Constant of Aberration 20·36 has been adopted, which is that of Mr. Baily's Catalogue, and established, as the author thinks, on very strong evidence. The reasons for adopting this value are founded on the following determinations: — 1. Struve's value (*Dorpat Observations*), deduced from 693 observed Right Ascensions of Circumpolar Stars, 20·3493. 2d. Value given by Brinkley in the Investigation of the Constant of Solar Nutation (*Trans. Royal Irish Academy, Vol. XIV.*); the observations having been recomputed by Dr. Robinson, after substituting the true value of the solar nutation for that used by Brinkley, and adding to the stars the two noticed by Mr. Richardson (*Mem. R. Astr. Society, Vol. IV.* p. 69). Combining the partial results with weights proportional to the number of observations, the corrected value from 3341 observations is found to be 20·3508.

3d. The value given by Von Lindenau, deduced from 1577 observations of *Polaris*, 20·4486. 4th. Delambre's value, deduced from 1000 observations of the first satellite of *Jupiter*, 20·255. The mean of all, combined according to the numbers, is

Const. of Aberration = 20·3591, from 6611 observations.

Mr. Richardson's value of the Constant of *Aberration* (20″·5030), deduced from 4119 observations made with the two Greenwich mural circles, has not been used; for, though the author is fully sensible of its importance, he considers that its weight is not so great in comparison of others, as the number of observations would seem to indicate. Without insisting on the fact, that the two circles sometimes give widely different results for the same star, it may be remarked, that the observations were made in pairs, in the same room, on the same days, almost at the same seconds of time. Each pair must, therefore, be similarly affected by those local causes of error (irregular temperature, atmospheric flutter, &c.) which produce the chief uncertainty of circle observations. Two observations so made, cannot be of equal weight with two made on different days; while, on the other hand, the necessary connexion between the two circles for obtaining their index corrections, must introduce a greater possibility of constant errors. These ideas prevented the author from adopting this result; but, he remarks, that had he used it, he should have found the same value of nutation.

For all the stars contained in the *Tabulæ Regiomontanæ*, the reductions were taken from that precious work. For others, the places given for 1830 in the *Nautical Almanac*, 1834, were reduced by the elements given there to the beginning of each year.

6. *Nutation.* The principal term of the nutation, is

$$+n \sin \alpha \cos \Omega - n' \cos \alpha \sin \Omega.$$

which, if we put

$$N' = \tan^{-1}\left(\frac{n}{n'} \times \frac{\sin \alpha}{-\cos \alpha}\right)$$

may be expressed by

$$n \times \frac{\sin \alpha}{\sin N'} \times \sin(N' + \Omega);$$

and as $\frac{n}{n'}$ is constant for a much longer time than the period of these observations, we have the correction of declination produced by a correction of n

$$= dn \times \frac{\sin \alpha}{\sin N'} \times \sin(N' + \Omega).$$

The values of N′ and $\frac{\sin \alpha}{\sin N'}$ were computed for the beginning and end of the period; but, in general, no interpolation was found necessary.

If, now, we put

δ = the declination, computed as above
i = Index error of the circle
dn = correction of Von Lindenau's nutation
N = coefficient of nutation
μ = proper motion, or correction of that given by Bessel
t = time from a given epoch
$d\delta$ = correction of catalogue at that epoch
I = $90° - \delta$ — observed polar distance
I′ = $270° + \delta$ — observed sub-polar distance
I″ = 2 hor. point + $\delta - 90°$ — observed reflected polar distance

we shall have, as the result of each observation, an equation

$$\begin{aligned} \mathrm{I} - i &= d\delta + \mathrm{N}\,dn + t\mu \text{ (above pole)} \\ \mathrm{I}' - i &= -d\delta - \mathrm{N}\,dn - t\mu \text{ (below pole)} \\ \mathrm{I}'' - i &= -d\delta - \mathrm{N}\,dn - t\mu \text{ (by reflection)} \end{aligned}$$

and $$i = \frac{\mathrm{I} + \mathrm{I}'}{2} = \frac{\mathrm{I} + \mathrm{I}''}{2}.$$

By combining the observations at the beginning, those at the middle, and those at the end of a revolution of the Moon's node, three final equations are found, namely,

$$\begin{aligned} \mathrm{E} &= a\,d\delta + \mathrm{N}\,dn - t\mu \\ \mathrm{E}' &= a'd\delta - \mathrm{N}'dn \pm t'\mu \\ \mathrm{E}'' &= a''d\delta + \mathrm{N}''dn + t''\mu, \end{aligned}$$

which give dn, still requiring a correction.

In selecting stars for the investigation, it is necessary to exclude those which pass the meridian so low as to be much affected by irregularities of refraction. The effect does not seem sensible at the lower passages of *Polaris* (alt. 50°), and none of the other stars is lower than 55°. It is also desirable to exclude all observations in which the coefficient of nutation is less than a certain limit. Assuming as a principle that the probable error of dn should never be greater than the probable error of one observation, it is found that N should not be less than 0·6. It is also desirable that the stars should be distributed with tolerable uniformity round the 24 hours of Right Ascension.

On searching the Greenwich Observations, 15 stars only could be found combining the above conditions with the equally important one of being frequently observed. These fifteen, however, present 11,000 observations, of which 3100 belong to *Polaris*. Of the latter, 2000 were used to find the index corrections, and the rest either applied to form the equations of condition, or to ascertain the state of index error between conjugate observations. Unhappily, more than a thousand of the remainder, belonging to the years 1820 and 1821, were rendered useless by an accidental derangement of the mural circle, which had, for some time, escaped detection.

To have applied the method of *minimum squares* to the final

equations, would have caused a very great increase of labour, which did not seem very necessary, considering that the values of N are near the maxima or minima; but it was of considerable importance that the mean of the individual results should be taken according to the theory of probabilities. For example, in *Polaris* but three-fourths of the change of nutation is sensible; in *Aldebaran* nine-tenths, while the observations of the former are about twice as accurate as those of the latter. The observations are accordingly combined as follows:—

In the three final equations formed for each star, a is the number of observations, N the coefficient of nutation, and t the sum of the times from a given epoch. Now it is well known, that if

$$U = pu + q\omega + rv,$$

the probable error of U is given by the equation

$$\epsilon^2 (U) = p^2 \epsilon^2 (u) + q^2 \epsilon^2 (\omega) + r^2 \epsilon^2 (v).$$

If, therefore, according to the common formula of elimination, we express dn in terms of E, E′, and E″, and denote $\frac{N}{a}$ and $\frac{t}{a}$ by ν and τ, and keep in mind that $\epsilon^2 (E) = a \times \epsilon^2$, we shall obtain

$$\epsilon^2(dn) = \frac{\epsilon^2 \times \frac{(\tau' - \tau'')^2}{a} + \frac{(\tau'' - \tau)^2}{a'} + \frac{(\tau - \tau')^2}{a''}}{\left(N \times \frac{\tau - \tau''}{a} + N' \times \frac{\tau'' - \tau}{a'} + N'' \times \frac{\tau - \tau'}{a''}\right)^2}$$

an expression for which the logarithms were all taken out in computing dn. The reciprocal of the second number is, of course, the probable weight.

The value of ϵ is investigated for each star. This is done with sufficient accuracy by forming in each equation of condition of the different groups the quantities

$$\pm \epsilon \mp \frac{E}{a} \mp \pi$$

(where $\pi = (N - \nu)\, dn + (t - \tau)\mu$), by taking the arithmetical mean of these, and multiplying it by the factor 0·8453 (*Fund. Ast.* p. 18). As π is small, and varies slowly, it is sufficient to compute it for every second or third month.

A table of the equations of condition finally obtained is given, and also the three final equations for each star, with the resulting values of dn, the total number of observations being 6023. The discordances are considerable; and it is remarkable, that similar discordances were found by Brinkley in the same stars (with the exception of *Pollux*), *Capella* and *α Cygni* giving less than the mean; *α Lyræ*, *γ Draconis*, *β Tauri*, and *Castor*, more.

The values of μ (proper motion) are, with one exception

(*β Tauri*), negative; a fact which Pond had previously noticed, and endeavoured to explain by the hypothesis of a southern motion. The author thinks there can be little doubt that these proper motions are, in great part, apparent. If, however, the charges which produce these be proportional to the time, they will not affect the accuracy of $d\,n$.

It is necessary to combine the individual values of $d n$ according to the probable weights computed as above. The products of $d n \times W$ (W being the weight) for each of the 15 stars being found, and their sum divided by the sum of the weights, there results

$$d\,n = +\,0''\!\cdot\!26094.$$

In computing the declinations, the values of those terms of nutation which depend on double longitudes, were, of course, derived from Von Lindenau's Constant. This value of $d\,n$, therefore, requires a correction. The terms 2 ☊ and 2 ☽ can make no change, nor is even that of 2 ☉ of much importance; but it seemed right to attend to it. The correction of Bessel's constant of solar nutation was found $= -\,0''\!\cdot\!03377$; from this the values of $d^2 n$ for each equation were computed, and the sum of the products, $d^2 n \times W$, being divided by the sum of the weight, there resulted for this correction

$$d^2 n = +\,0''\!\cdot\!00101.$$

The result of the whole investigation is, therefore,

$$\begin{array}{rcr}
\text{Von Lindenau's Constant} & = & 8''\!\cdot\!97718 \\
d\,n & = & +\,0\cdot26094 \\
d^2 n & = & +\,0\cdot00101 \\
\hline
\text{Constant of nutation} & = & 9\cdot23913
\end{array}$$

which the author offers to the Society as the best that can be obtained from the Greenwich Observations of this period. *How* accurate it is he does not pretend to say; but its precision, so far as errors of observation are concerned, must be considerable.

II. Extract of a Letter from Mr. Charles Piazzi Smyth, dated Observatory, Cape of Good Hope, Dec. 17, 1837.

"The zenith sector came very safely, and is now erected in the outer room, but cannot be used until the opening in the lantern is enlarged, and stones placed under each of the feet, which now rest upon the floor, the bending of which under the weight of a moderate sized person, throws the tube out two or three seconds; but the flexure of a boarded floor, of the best possible construction, was better detected in an operation carried on some time back, when Mr. Maclear wished to examine the divisions of the level of the transit instrument. A deal rod, 14 feet in length, properly

trussed to prevent flexure, was placed in such a manner, that while one end rested in a brass socket, which allowed it only to move in a vertical arc, with that end for its centre, the other was placed upon the end of a screw, by which it could be raised or depressed a small quantity. Thus you will see that the space moved through by the screw was the tangent of the arc which the rod had described; and thus, knowing the length of the rod, and the value of each revolution of the screw, you get at the angle. The screw used was a beautifully constructed micrometer for measuring the thickness of lamina of various substances, and might be read off to a very small fraction of an inch indeed. The level being then placed on the rod, as near the centre end as possible (so that a greater motion was requisite in the screw to produce a small one in the level), the rod was moved through small angles, the level scale and the micrometer being read off every time. A counterpoise weight was applied about the middle of the rod, to take away flexure, and to remove all injurious weight from the screw. Great pains were taken to prevent vibration, but the level was affected when a person sitting on the ground stretched his foot a little further out, or a little nearer to him, than it had been before."

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

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The following communications were read:—

I. An account of some Experiments on two new Invariable Pendulums. By Mr. Baily.

In the summer of the year 1836, Lieut. Wilkes of the United States Navy, visited Europe for the purpose of procuring various astronomical and philosophical instruments, intended for a scientific voyage towards the South Pole, about to be undertaken by the authority of the Congress of the United States of America. Amongst the instruments which he ordered in this country, were the two invariable pendulums which are the subject of the present memoir: and as they were constructed and fitted up under Mr. Baily's direction, and as a number of experiments on them were made by him at his house, and subsequently by Lieut. Wilkes, at New York, he presumed that a short account of the particulars might not be unacceptable to the members of the Society.

Mr. Baily in the first place proceeds to describe the form of these pendulums, which he says are precisely the same as that of the two invariable pendulums (one of copper and the other of iron) belonging to this Society; and a description of which is given in the seventh volume of the *Memoirs:* the only alteration in the construction being in the position of the knife-edges, which, in the present case, are placed at equal distances from the centre of gravity of the pendulum: this being considered by him as the best position for a travelling pendulum, where comparative results only are required. The whole length of the bars (brass and iron) after the final adjustment, was about 67·6 inches; and the knife-edges (which are denoted by the letters A and B) were placed at the distance of 39·4 inches from each other; which gave an interval of about 9 or 10 minutes between the coincidences. Mr. Baily has explained the mode pursued in the adjustments, and the reasons for adopting the principles here alluded to. He then states the several formulæ used in the reductions, and the results of the experiments made by him in London, in November 1836; which, when reduced to the temperature of 62°, in vacuo, and at the mean level of the sea, are as follows: viz.

Pendulum.	Knife-edge A.		Knife-edge B.	
	Hours employed.	Vibrations in a mean Solar Day.	Hours employed.	Vibrations in a mean Solar Day.
Brass	20·25	86091·94	23·34	86082·51
Iron	20·80	86086·15	20·16	86069·07

Immediately on the conclusion of these experiments, the pendulums were carefully packed up, and taken by Lieut. Wilkes to America. On his arrival at New York, he considered it desirable to make some experiments on them prior to their being forwarded to their destination, not only for the purpose of acquiring a greater facility of manipulation and observing, but also with a view of determining the difference in the force of gravity as shewn in that city, compared with that in London. The spot which he chose for the site of his experiments was the same as that selected by Capt. Sabine in the year 1822. A wooden building was erected in the College Green, and the apparatus for the pendulum and astronomical observations being safely mounted, the experiments were made in the months of April and May 1837.

On opening the case containing the iron pendulum, Lieut. Wilkes remarks that he found it speckled with rust. He kept it, however, untouched, till he had made a few experiments with it, and then carefully cleaned it, and concluded his experiments. With the knife-edge A, he found little, or no difference; but with the knife-edge B, there was a discordance amounting to nearly two vibrations in a day. This is inexplicable, unless it arose from the great change of temperature which occasionally occurred during these experiments. For, it should be remarked that the building, above mentioned, does not appear to have been efficient in preserving an uniformity of temperature; since we find, from an examination of the details transmitted by Lieut. Wilkes, that a difference of 5, 6, and even more than 7 degrees in the height of the thermometer sometimes occurred during the two or three hours that a set of the experiments has occupied. This also has probably been the chief cause of some other anomalies that may be discovered on comparing the individual results of the several sets. The following is the general result obtained from a mean of all the experiments, and reduced as before mentioned: viz.

Pendulum.	Knife-edge A.		Knife-edge B.	
	Hours employed.	Vibrations in a mean Solar Day.	Hours employed.	Vibrations in a mean Solar Day.
Brass	21·04	86046·26	25·39	86037·62
Iron	27·91	86046·02	28·43	86028·04

If we compare these values with those previously obtained in London, we shall find that the two pendulums give different results for the effect of gravity; and, in which, both knife-edges agree. For, whilst the brass pendulum gives 45·28 vibrations in a mean solar day, as the difference between London and New York, the iron pendulum gives only 40·58 vibrations: the mean (= 42·93) is somewhat greater than that obtained by Capt. Sabine. On the whole, however (as the Author remarks), the general result, and the great care and attention bestowed on the experiments by Lieut. Wilkes, do great credit to his zeal and perseverance. And it is to be hoped that he will follow up his original intention of swinging these pendulums in as many parts of the United States as he may find it practicable and convenient, without suffering the present trifling discordances to divert him from so praiseworthy an object.

It appears, from Lieut. Wilkes's communication, that the American Expedition above alluded to, which was supposed to have been abandoned, is again revived, and that it will probably sail in the course of the ensuing autumn, under more favourable auspices than were originally contemplated.

II. Right Ascensions of 125 Stars, chiefly of the 7th Magnitude, observed at Ashurst. By R. Snow, Esq.

The observations were made with a transit instrument, by Simms, of 3½ feet focal length, mounted substantially on stone in a convenient observatory, and furnished with the usual adjustments. The detail of the method of using the instrument is entered into; and annexed is given the longitude of the place of observation, deduced from comparisons of four chronometers on three different days with the transit clocks at Ashurst and Mr. Wrottesley's Observatory at Blackheath. In these comparisons, the greatest difference from the mean result was, on the first day, $-1^s,05$; on the second, $-0^s,16$; and on the third, $+0^s,38$. The final result makes Ashurst $1^m\ 12^s,8$ west of Mr. Wrottesley's Observatory at Blackheath. The latitude has been before stated to be 51° 15′ 58″ north, as determined from a mean of twenty observations, made with a portable transit instrument on the Prime Vertical.

The Notes contain some remarks on the fainter stars, together with suspected proper notions in R.A. in the following,

743 A. S. C.
779 A. S. C.
908 A. S. C.
1336 A. S. C.
1651 A. S. C.

III. On the Total Eclipse of the Sun which will happen July 7, 1842; with the Elements for Calculation from the Solar Tables of Carlini, and the Lunar Tables of Burckhardt. Also the results of Calculations for the Observatories of Greenwich and Marseilles. By Mr. George Innes.

The author remarks that the great Solar Eclipse of 1842 will

excite much interest, as it will be *total* for the southern parts o France, and very large throughout Great Britain, though not tota for any place in the island. The elements for calculating th eclipse are given, and the results of his own calculations for Greenwich and Marseilles; which latter place was selected as being th nearest to Greenwich of the properly fixed points at which accurat observations of the *total eclipse* are likely to be made. The fina results are the following:

For the Observatory of Greenwich,

		Mean Time. d h m s
Eclipse begins	July	7 16 53 19,88
Apparent Ecliptic conjunction	—	17 46 11,92
Greatest Obscuration	—	17 46 21,33
End of the Eclipse	—	18 42 54,16

Digits eclipsed at greatest Obscuration, 9 dig. 42′ 30″·43 o the south part of the Sun's disc. The Moon will enter the Sun' disc at 60° 1′ 32″·6 to the right of his vertex, as seen by direc vision.

For the Observatory of Marseilles,

		Mean Time. d h m s
Eclipse begins	July	7 17 2 46,77
Beginning of Total Darkness	—	17 56 40,30
Least distance of centres (18″·34)	—	17 57 41,86
End of Total Darkness	—	17 58 43,42
End of the Eclipse	—	18 57 1,07

The Moon will enter the Sun's disc at 50° 47′ 17″·8 to the righ of his vertex, as seen by direct vision.

IV. Observations of the Occultation of Mercury on the 25tl of April, 1838, made at Aberdeen, by Dr. Cruickshank and M Innes, in a Letter to the President.

The immersion was observed by Dr. Cruickshank at the observ atory of Marischal College, with a 3½-feet achromatic by Dollond and a power of about 125. The state of the weather was favour able, the planet's crescent well defined, and the moon's dark limb near the planet, not visible.

	h m s
First perceptible impression on the planet at	8 8 35,1
Simultaneous disappearance of both horns	8 8 43,6
Distinct reappearance of southern horn	8 8 45,0
Final disappearance of southern horn	8 8 45,2

The emersion was observed by Mr. Innes with a 30-inch achro matic by Ramsden. The moon and planet finally separated a $8^h\ 53^m\ 10^s,7$. Both observations are for mean time.

V. Extract of a Letter from Professor Encke, of Berlin, t Mr. Stratford.

M. Encke announces that an ephemeris of the comet for it appearance in the present year, will be inserted in the *Berli*

VII. Moon-culminating Stars observed at Greenwich, Cambridge, and Edinburgh, in the months of January, February, March, April, and May, 1838.

Date.	Object.	Apparent Right Ascension. Edinburgh.	Apparent Right Ascension. Cambridge.	Greenwich. Obs. Trans. uncorrected for Inst. Errors.	Greenwich. Approx. Clock Rate.
		h m s	h m s	h m s	s
Jan. 2	n Piscium......	...	23 39 36,20	...	−0,85
	q Piscium	...	23 53 30,76	...	
	Moon 1 L. .	...	0 18 43,57	0 18 14,63	
	Piscium	...	0 39 53,09	0 39 22,98	
	ι Piscium	...	0 54 32,31	0 54 2,18	
3	Piscium	...	0 39 53,20	0 39 22,75	−0,90
	ι Piscium	...	0 54 32,23	0 54 1,68	
	Moon 1 L. .	...	1 9 31,06	1 9 1,33	
	π Piscium......	...	1 22 49,23	...	
	o Piscium	...	1 36 50,83	...	
4	π Piscium......	1 22 49,09	1 22 49,32	...	
	o Piscium	1 36 50,67	1 36 50,71	...	
	Moon 1 L.	2 1 15,40	2 0 47,02	...	
	ψ Arietis	2 21 55,98	2 21 55,99	...	
	σ Arietis	2 40 15,73	2 40 15,97	...	
5	ψ Arietis	2 21 56,08	2 21 56,00	...	
	σ Arietis	2 40 15,93	2 40 15,96	...	
	Moon 1 L. .	2 54 9,04	2 53 39,41	...	
	g Arietis	3 14 45,59	3 14 45,63	...	
	η Tauri	3 37 52,51	3 37 52,43	...	
6	g Arietis	3 14 45,38	...	...	
	η Tauri	3 37 52,24	...	...	
	Moon 1 L.	3 49 19,53	...	...	
	υ^1 Tauri	4 16 37,79	...	...	
	τ Tauri	4 32 32,12	...	...	
7	υ^1 Tauri	..	...	4 16 4,66	−0,70
	τ Tauri	...	...	4 31 58,83	
	Moon 1 L.	...	...	4 45 41,63	
8	β Tauri	5 16 4,39	...	...	
	Moon 1 L. .	5 45 37,89	...	...	
	α Aurigæ	6 5 4,44	...	...	
	ϵ Geminorum	6 33 58,97	...	...	
14	Moon 2 L. .	...	...	10 59 12,23	−0,20
	ι Leonis	...	...	11 14 52,21	
18	a Virginis ...	13 16 39,98	...	...	
	m Virginis ...	13 33 6,84	...	...	
	Moon 2 L. .	13 56 29,43	...	...	
19	λ Virginis	14 11 21,19	...	...	
	Moon 2 L.	14 45 57,72	...	...	
	ι^1 Libræ	15 2 59,11	...	...	
	χ Libræ	15 30 42,28	...	...	
Feb. 2	Moon 1 L.	...	...	3 31 4,66	−0,42
	A^1 Tauri	...	...	3 54 35,39	
	υ^1 Tauri........	...	...	4 16 5,23	

Jahrbuch for 1840; but, lest that work may not be received in England in time to assist observers to find the comet in the month of August, he transmits the Ephemeris in manuscript, and requests Mr. Stratford to communicate some places from it to the astronomers of Greenwich, Cambridge, and Edinburgh.*

M. Encke remarks that the elements of the comet appear to be sufficiently exact. For 1832 the errors are only about 2′ 28″ in R.A., and 1′ 15″ in declination. In 1835, the differences from observations amounted only to 1′ 5″ in R.A., and 19″ in declination. For 1838, the perturbations of *Mercury* are so great as to render it possible that the uncertainty which still exists respecting this element of our system will occasion much greater differences than the errors of the elements would produce. The perturbations of *Mercury* will alone produce, in the present revolution, the following effects on the geocentric position of the comet:

		R.A.	Declination.
October	13	+13′ 8″	+ 7′ 26″
	23	+13 22	+17 12
November	2	−57 54	+16 50
	12	−41 48	− 8 16
	23	−24 33	− 8 22

so that this appearance will be one of great interest. The comet will approach as near to the earth as the elements of the two bodies will in general permit; and it may be hoped that it will be visible from the beginning of September till after the beginning of December. Observers may begin to look out for it towards the end of July.

VI. Observations of the Moon and Moon-culminating Stars, made in the Observatory of S. Fernando, in the year 1837. By M. Cerquero.

* Instead of contenting himself with complying with the request of Professor Encke, the Superintendent, with his usual zeal, has published the whole Ephemeris for the months of August, September, October, November, and December, adapted to the meridian of Greenwich; and copies of it may be had on application to Mr. Hartnup, the Assistant-Secretary of this Society.

Date.	Object.	Apparent Right Ascension.		Greenwich.	
		Edinburgh.	Cambridge.	Obs. Trans. uncorrected for Inst. Errors.	Approx. Clock Rate.
		h m s	h m s	h m s	s
Feb. 3	A^1 Tauri	3 55 7,62	...	3 54 34,44	−0,42
	ν^1 Tauri	4 16 37,54	...		
	Moon 1 L. .	4 28 58,17	4 28 26,16	4 27 53,71	
	ι Tauri	4 53 25,85	4 53 25,68	...	
	β Tauri	5 16 4,29	5 16 4,29	5 15 30,65	
4	ι Tauri	...	4 53 25,67	...	
	β Tauri	...	5 16 4,12	...	
	Moon 1 L. .	...	5 26 39,77	...	
	C Tauri	...	5 43 9,85	...	
	κ Aurigæ......	...	6 5 4,37	...	
8	Moon 1 L...	...	9 8 12,80	...	
	λ Leonis	...	9 22 29,60	...	
	ψ Leonis	...	9 34 55,46	...	
9	λ Leonis	9 22 29,67	9 22 29.70	9 21 52,93	−0,62
	ψ Leonis	9 34 55,63	9 34 55,70	9 34 18,86	
	Moon 2 L. .	9 59 1,22	9 58 35,90	9 57 59,95	
	γ Leonis	10 11 3,37	10 11 3,51	10 10 26,73	
	ξ Leonis	10 24 17,91	10 24 18,00	...	
10	γ Leonis	...	10 11 3,40	...	
	ξ Leonis	10 24 18,01	10 24 18,03	...	
	Moon 2 L...	10 44 34,73	10 44 10,73	...	
	χ Leonis	10 56 40,87	10 56 40,72	...	
	ι Leonis	11 15 29,99	11 15 29,94	...	
11	χ Leonis	10 56 40,85	10 56 40,85	...	
	ι Leonis	11 15 29,97	11 15 29,98	...	
	Moon 2 L.	11 28 20,67	11 27 57,34	...	
	β Virginis ...	...	11 42 16,73	...	
	b Virginis	...	11 51 40,35	...	
12	β Virginis	...	...	11 41 39,06	−0,48
	b Virginis	...	11 51 40,40	11 51 2,69	
	Moon 2 L.	...	12 10 54,51	12 11 17,63	
Mar. 3.	ι Tauri	..	4 53 25,39	...	
	Moon 1 L..	...	5 7 28,43	...	
	l Aurigæ	...	5 28 14,76	...	
	C Tauri	...	5 43 9,43	...	
5	ι Geminorum	6 33 58,76	...	...	
	Moon 1 L.	7 5 0,41	...	...	
6	α^2 Geminorum	...	7 24 16,64	7 23 29,19	−0,64
	κ Geminorum	...	7 34 40,94	7 33 53,40	
	Moon 1 L. .	8 0 20,55	7 59 51,08	7 59 4,60	
	ϕ^2 Cancri prec.	8 17 0,08	8 17 0,45	8 16 13,03	
	ϕ^2 Cancri fol...	8 17 0,28	...	...	
	γ Cancri	...	8 33 55,65	...	
7	ϕ^2 Cancri prec.	8 17 0,16	...	8 16 12,77	−0,41
	ϕ^2 Cancri fol...	8 17 0,34	...	...	
	γ Cancri	8 33 55,74	8 33 55,67	8 33 8,05	
	Moon 1 L.	8 52 30,09	8 52 2,35	8 51 15,74	
	q Cancri	9 9 57,34	9 9 57,51	...	
	λ Leonis	9 22 29,71	9 22 29,70	...	

Date.	Object.	Apparent Right Ascension.		Greenwich.	
		Edinburgh.	Cambridge.	Obs. Trans. uncorrected for Inst. Errors.	Approx. Clock Rate.
		h m s	h m s	h m s	s
Mar. 8	φ Cancri	9 9 57,45	...	...	
	λ Leonis	9 22 29,71	9 22 29,70	...	
	Moon 1 L.	9 41 27,51	9 41 1,50	...	
	α Leonis	9 59 45,92	9 59 45,92	...	
	γ Leonis prec.	10 11 3,69	10 11 3,74	...	
	γ Leonis fol...	10 11 3,89	...	...	
9	α Leonis	9 59 46,10	9 59 46,01	9 58 56,92	−0,70
	γ Leonis prec.	10 11 3,81	10 11 3,70	10 10 14,74	
	γ Leonis fol. ..	10 11 4,08	...	...	
	Moon 1 L. .	10 27 40,79	10 27 16,23	10 26 27,82	
	l Leonis	10 40 46,05	10 40 46,06	...	
	χ Leonis	10 56 41,26	10 56 41,18	...	
10	l Leonis	...	10 40 45,92	...	
	χ Leonis	...	10 56 41,14	10 55 51,03	−0,60
	Moon 1 L...	...	11 11 31,78	11 10 42,45	
	Moon 2 L...	...	11 13 34,62*	11 12 45,17	
	ν Virginis	...	11 37 33,64	...	
	β Virginis	...	11 42 17,11	...	
11	ν Virginis	...	11 37 33,63	11 26 43,46	−0,60
	β Virginis	...	...	11 41 26,82	
	Moon 2 L...	...	11 56 45,02	11 55 55,39	
	η Virginis	...	12 11 38,84	...	
	γ^1 Virginis	...	12 33 28,81	...	
12	η Virginis	...	12 11 38,79	12 10 47,26	−0,58
	γ^1 Virginis	...	12 33 28,90	12 32 37,36	
	Moon 2 L...	...	12 39 52,40	12 39 1,58	
	θ Virginis	...	13 1 35,59	...	
	α Virginis	...	13 16 41,51	...	
15	Moon 2 L..	...	...	14 58 33,67	−0,51
	b Scorpii	...	...	15 40 22,64	
17	Moon 2 L.	...	...	16 49 31,95	−0,41
	θ Ophiuchi ...	...	...	17 11 10,61	
30	Moon 1 L.	4 43 24,92	4 42 51,49	...	
	β Tauri	5 16 3,36	5 16 3,33	...	
	ζ Tauri	5 27 58,02	5 27 58,12	...	
April 1	κ Aurigæ	...	6 5 3,62	...	
	ε Geminorum	...	6 33 58,29	...	
	Moon 1 L...	6 44 16,67	6 43 44,37	...	
	ι Geminorum	7 15 40,30	7 15 40,23	...	
	α^2 Gemin. prec.	7 24 15,79	...	...	
	α^2 Gemin. fol.	7 24 16,26	7 24 16,25	...	
2	ι Geminorum	7 15 40,45	...	...	
	α^2 Gemin. prec.	7 24 15,78	...	...	
	α^2 Gemin. fol..	7 24 16,01	...	...	
	Moon 1 L..	7 41 27,08	...	...	
	λ Cancri	8 10 54,83	...	...	
	φ^2 Cancri prec.	8 16 59,71	...	...	
	φ^2 Cancri fol. .	8 16 59,86	...	...	

* The correction, 0s,06, is added for defect of illumination.

Date.	Object.	Apparent Right Ascension.		Greenwich.	
		Edinburgh.	Cambridge.	Obs. Trans. uncorrected for Inst. Errors.	Approx. Clock Rate.
		h m s	h m s	h m s	s
April 3	φ^1 Cancri prec.	8 16 59,62	...	...	
	φ^2 Cancri fol. .	8 16 59,79	...	...	
	Moon 1 L. .	8 35 8,13	...	...	
	ξ Cancri	9 0 3,25	...	...	
	q Cancri	9 9 57,11	...	...	
4	Moon 1 L. .	...	...	9 23 46,03	−0,68
	o Leonis	...	...	9 48 28,43	
	π Leonis	...	...	9 57 28,10	
5	π Leonis	9 58 31,00	...	...	
	Moon 1 L. .	10 12 16,47	...	...	
6	ρ Leonis	10 24 18,14	...	...	
	l Leonis	10 40 45,82	...	...	
	Moon 1 L. .	10 57 1,11	...	...	
	ι Leonis	11 15 30,18	...	...	
	υ Leonis	11 28 40,76	...	...	
7	ι Leonis	...	11 15 30,66	...	
	υ Leonis	...	11 28 41,19	11 27 37,06	−0,23
	Moon 1 L. .	...	11 40 3,34	11 39 0,10	
	b Virginis ...	...	11 51 40,86	11 50 36,70	
	π Virginis ...	...	12 11 39,28	...	
8	b Virginis ...	11 51 40,66	11 51 40,76	...	
	π Virginis ...	12 11 38,93	12 11 38,99	...	
	Moon 1 L. .	12 23 35,24	12 23 11,30	...	
	γ Virginis . .	12 33 29,00	12 33 29,04	...	
	ψ Virginis ...	12 45 57,61	12 45 57,84	...	
9	γ Virginis ...	...	12 33 29,10	...	
	ψ Virginis ...	...	12 45 57,90	...	
	Moon 1 L. .	...	13 7 4,33	13 6 0,75	−0,13
	Moon 2 L. .	...	13 9 8,66*	13 8 4,89	
	α Virginis ...	...	13 16 41,71	13 15 37,76	
11	λ Virginis ...	14 10 22,90	14 10 23,02	...	
	Moon 2 L. .	14 43 57,19	14 43 29,90	...	
	ι^1 Libræ	15 3 1,68	15 3 1,85	...	
12	ι^1 Libræ	...	...	15 2 55,59	−0,66
	Moon 2 L. .	...	...	15 35 43,51	
May 2	Moon 1 L. .	...	...	9 54 20,69	−0,51
	γ Leonis	...	...	10 10 52,37	
3	γ Leonis	...	10 11 3,06	...	−0,30
	ρ Leonis	...	10 24 17,79	10 24 6,93	
	Moon 1 L. .	...	10 40 8,11	10 39 57,95	
	χ Leonis	...	10 56 40,69	10 56 30,15	
	σ Leonis	...	11 12 48,28	11 12 37,63	
4	χ Leonis	...	10 56 41,10	10 55 29,45	−0,27
	σ Leonis	11 12 48,40	11 12 48,49	11 12 36,93	
	Moon 1 L. .	11 24 20,04	11 23 56,55	11 23 45,81	
	ν Virginis ...	...	11 37 33,59	11 37 22,04	
	β Virginis ...	...	11 42 17,10	11 42 5,54	

* Correction for defect of illumination, $+0^s{,}01$.

Date.	Object.	Apparent Right Ascension.		Greenwich.	
		Edinburgh.	Cambridge.	Obs. Trans. uncorrected for Inst. Errors.	Approx. Clock Rate.
		h m s	h m s	h m s	s
May 5	ν Virginis ...	11 37 33,43	11 37 33,63	11 37 22,25	−0,28
	β Virginis ...	11 42 17,04	11 42 17,35	11 42 5,74	
	Moon 1 L..	12 7 26,41	12 7 3,05	12 6 52,14	
	γ Virginis ...	...	12 33 29,24	...	
6	γ^1 Virginis ...	12 33 29,05	...	12 33 17,10	−0,28
	Moon 1 L..	12 50 59,29	...	12 50 23,83	
	θ Virginis ...	13 1 36,01	...	13 1 24,04	
	α Virginis ...	13 16 41,73	...	13 16 30,02	
7	θ Virginis ...	13 1 35,90	13 1 35,89	13 1 23,83	−0,38
	α Virginis ...	13 16 41,88	...	13 16 29,69	
	Moon 1 L..	13 36 7,19	13 35 41,70	13 35 30,41	
	λ Virginis ...	14 10 23,33	14 10 23,27	...	
8	λ Virginis ...	14 10 23,34	14 10 23,28	14 10 10,38	−0,60
	Moon 1 L..	14 23 57,36	14 23 30,27	14 24 18,22	
	α^2 Libræ	14 41 57,78	14 41 57,68	14 41 44,92	
	20 Libræ	14 54 38,36	14 54 38,40	14 54 25,46	
9	α^2 Libræ	...	14 41 57,94	...	
	Moon 2 L..	...	15 17 16,16	...	
10	χ Libræ	15 30 45,51	15 30 45,31	...	
	δ Scorpii	15 50 47,94	15 50 48,20	...	
	Moon 2 L..	16 13 38,77	16 13 7,59	...	
	τ Scorpii	16 25 50,79	16 25 51,04	...	
	25 Scorpii	16 36 59,18	16 36 59,00	...	
11	τ Scorpii	...	16 25 50,82	16 25 37,53	−0,33
	25 Scorpii	...	16 36 59,10	16 36 45,73	
	Moon 2 L..	...	17 13 5,41	17 12 52,91	
	3 Sagittarii ...	..	17 37 24,47	...	
	γ^2 Sagittarii ...	...	17 55 26,50	...	
27	β Geminorum	7 35 23,80	7 35 23,86	7 35 6,19	−0,26
	Moon 1 L..	7 50 3,24	7 49 32,20	7 49 15,42	
29	Moon 1 L..	...	...	9 34 29,92	−0,68
	α Leonis	...	...	9 59 26,95	
30	α Leonis	9 59 45,17	9 59 45,15	...	
	γ Leonis prec.	10 11 2,89	10 11 2,84	...	
	γ Leonis fol..	10 11 3,07	...	...	
	Moon 1 L..	10 22 13,95	10 21 49,11	...	

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The following communications were read :—

I. Astronomical Observations made at the Imperial Observatory at Wilna, in the year 1835. By M. Slavinski.

These observations are of a similar nature to those made in former years, and communicated to the Society from time to time. The present collection consists of observations of *Jupiter*, *Saturn*, *Mars*, and *Uranus*, as well as of moon-culminating stars, occultations of stars by the moon, and of eclipses of *Jupiter's* satellites. The geocentric right ascension and declination of each planet, and for each day of observation, are compared with the positions deduced from Encke's *Berlin Ephemeris*, and the differences noted. In the case of *Jupiter* these differences in right ascension are all positive, and the maximum difference is $1^s,36$; in declination the maximum differences are $-4''\cdot1$ and $+6''\cdot9$. In the case of *Saturn*, the differences are likewise (with one slight exception) all positive, the maximum being $0^s,80$; the same remark applies to the declinations, the whole of the differences being positive, and varying from $15''\cdot6$ to $28''\cdot1$. In the case of *Mars*, the differences in right ascension vary from $-0^s,25$ to $+0^s,52$; and in declination from $-1''\cdot9$ to $+17''\cdot6$. In the case of *Uranus*, the differences in right ascension are very considerable, and all positive, varying from $3^s,07$ to $4^s,02$; but in declination the differences are not so great, being confined within $-4''\cdot9$ and $+3''\cdot9$. These large errors in the tables of some of the planets are confirmed by observations made at other observatories; and will doubtless, in time, lead to their correction.

The observations of the moon-culminating stars were made on thirty-two days at various parts of the year, each observation being made with the five wires of the transit. Only six of these observations were made of the second limb of the moon.

The occultations are six in number: one being of *Saturn* on

the 27th of August; and the others of various stars. There are ten eclipses of *Jupiter's* satellites; viz. five of the first, and five of the second. To the whole is subjoined the monthly mean of the barometer and thermometer during the year, with a statement of the prevailing wind, which appears to be north-west and south.

II. A letter from Professor Bessel to Sir J. Herschel, Bart., dated Konigsberg, Oct. 23, 1838.

Esteemed Sir,—Having succeeded in obtaining a long-looked-for result, and presuming that it will interest so great and zealous an explorer of the heavens as yourself, I take the liberty of making a communication to you thereupon. Should you consider this communication of sufficient importance to lay before other friends of Astronomy, I not only have no objection, but request you to do so. With this view, I might have sent it to you through Mr. Baily; and I should have preferred this course, as it would have interfered less with the important affairs claiming your immediate attention on your return to England. But, to you, I can write in my own language, and thus secure my meaning from indistinctness.

After so many unsuccessful attempts to determine the parallax of a fixed star, I thought it worth while to try what might be accomplished by means of the accuracy which my great Fraunhofer Heliometer gives to the observations. I undertook to make this investigation upon the star 61 *Cygni*, which, by reason of its great proper motion, is perhaps the best of all; which affords the advantage of being a double star, and on that account may be observed with greater accuracy; and which is so near the pole that, with the exception of a small part of the year, it can always be observed at night at a sufficient distance from the horizon. I began the comparisons of this star in September 1834, by measuring its distance from two small stars of the 11th magnitude, of which one precedes, and the other is to the northward. But I soon perceived that the atmosphere was seldom sufficiently favourable to allow of the observation of stars so small; and, therefore, I resolved to select brighter ones, although somewhat more distant. In the year 1835, researches on the length of the pendulum at Berlin took me away for three months from the observatory; and when I returned, Halley's Comet had made its appearance, and claimed all the clear nights. In 1836, I was too much occupied with the calculations of the measurement of a degree in this country, and with editing my work on the subject, to be able to prosecute the observations of α *Cygni* so uninterruptedly as was necessary, in my opinion, in order that they might afford an unequivocal result. But, in 1837 these obstacles were removed, and I thereupon resumed the hope that I should be led to the same result which Struve grounded upon his observations of α *Lyræ*, by similar observations of 61 *Cygni*.

I selected among the small stars which surround that double

star, two between the 9th and 10th magnitudes; of which one (a) is nearly perpendicular to the line of direction of the double star; the other (b) nearly in this direction. I have measured with the heliometer the distances of these stars from the point which bisects the distance between the two stars of 61 *Cygni*; as I considered this kind of observation the most correct that could be obtained, I have commonly repeated the observations sixteen times every night. When the atmosphere has been unusually unsteady, I have, however, made more numerous repetitions; although, by this, I fear the result has not attained that precision which it would have possessed by fewer observations on more favourable nights. This unsteadiness of the atmosphere is the great obstacle which attaches to all the more delicate astronomical observations. In an unfavourable climate we cannot avoid its prejudicial influence, unless by observing only on the finest nights; by which, however, it would become still more difficult to collect the number of observations necessary for an investigation. The places of both stars, referred to the middle point of the double star, are for the beginning of 1838,

	Distance.	Angle of Pos.
a	461″·617	201° 29′ 24″
b	706 ·279	109 22 10

As the instrument gives, at the same time, the distance and angle of position, I have always observed both. But the position circle is divided only into whole minutes; which, in the distance of the first star, have the value of 0″·134; in that of the second, 0″·205. Moreover, other causes exist which may render the observation of the angle of position less certain than that of the distances. I have, accordingly, considered the first of these as of less consequence in so delicate an investigation, and concentrated my attention, as far as I could, upon the latter.

The following tables contain all my measures of distance, freed from the effects of refraction and aberration, and reduced to the beginning of 1838. In these reductions, the annual variations employed of both distances are $= +4''\cdot3915$ and $-2''\cdot825$; which I have deduced (on the supposition that the stars a and b have no proper motions) from the mean motions of both stars of 61 *Cygni*, which M. Argelander had lately found by comparison of my determination (from Bradley's observations) for 1755, with his own for 1830. In the meantime, we cannot regard these variations of distance as the *true variations*; because the stars compared may have proper motions, and, also, because it is not known whether the mean of the motions of both stars of 61 *Cygni* appertains to its centre, and whether *this* (motion) is proportional to the time. In what follows, let us denote the true variations of the distances by $+4''\cdot3915+\alpha'$ and $-2''\cdot825+\beta'$, the mean distances for the beginning of 1835 by α and β; the time, reckoned from this beginning, by t; the difference of the constants of the annual parallax of 61 *Cygni*, and of the comparison-stars a and b, by α'' and β''; and, lastly, the coefficients of the parallax depending on

the place of the earth by a. Then the expressions of the distances at the beginniug of 1838 are—

$$\text{For the star } a = \alpha + t\alpha' + a\alpha''$$
$$\text{For the star } b = \beta + t\beta' + a\beta''$$

These expressions, as they were at the time of each observation, I have written against the observations; we can, therefore, by inspection, perceive how the observations agree with the theory.

OBSERVATIONS OF THE STAR a.

1	1837. Aug. 18	462·050″	$\alpha - 0·369\,\alpha'$	$+0·635\,\alpha''$	34	1838. May 16	461·915″	$\alpha + 0·372\,\alpha'$	$+0·661\,\alpha''$
2	19	1·619	−0·367	+0·624	35	17	2·015	+0·376	+0·680
3	20	1·693	−0·364	+0·611	36	19	1·813	+0·380	+0·701
4	28	1·726	−0·342	+0·513	37	21	1·902	+0·386	+0·721
5	30	1·940	−0·337	+0·487	38	22	1·840	+0·389	+0·730
6	Sept. 4	1·912	−0·323	+0·414	39	23	1·978	+0·392	+0·740
7	8	1·841	−0·312	+0·363	40	June 1	1·879	+0·416	+0·817
8	9	1·597	−0·309	+0·349	41	2	2·100	+0·419	+0·825
9	11	1·633	−0·304	+0·321	42	12	1·867	+0·446	+0·885
10	14	1·779	−0·296	+0·270	43	13	1·951	+0·449	+0·889
11	20	1·502	−0·279	+0·184	44	22	1·658	+0·474	+0·919
12	23	1·814	−0·271	+0·138	45	26	1·886	+0·485	+0·926
13	24	1·591	−0·268	+0·123	46	27	1·940	+0·488	+0·928
14	Oct. 1	1·614	−0·249	+0·012	47	28	2·111	+0·490	+0·928
15	2	1·760	−0·246	−0·003	48	29	2·132	+0·493	+0·928
16	16	1·708	−0·208	−0·222	49	30	2·168	+0·496	+0·929
17	28	1·512	−0·175	−0·398	50	July 1	1·790	+0·499	+0·928
18	Nov. 22	1·395	−0·107	−0·699	51	8	1·778	+0·518	+0·921
19	Dec. 1	1·321	−0·083	−0·779	52	10	1·927	+0·524	+0·917
20	30	1·233	−0·003	−0·897	53	14	1·631	+0·534	+0·910
21	31	1·306	−0·001	−0·897	54	17	1·851	+0·543	+0·892
22	1838. Jan. 8	1·168	+0·023	−0·886	55	29	1·973	+0·575	+0·825
23	10	1·226	+0·028	−0·881	56	Aug. 4	1·817	+0·592	+0·778
24	14	1·175	+0·044	−0·855	57	11	1·803	+0·611	+0·713
25	17	1·485	+0·047	−0·852	58	20	1·579	+0·636	+0·615
26	20	1·112	+0·056	−0·837	59	21	1·833	+0·638	+0·604
27	Feb. 1	1·491	+0·088	−0·751	60	25	1·707	+0·649	+0·556
28	5	1·620	+0·099	−0·715	61	26	1·770	+0·652	+0·543
29	10	1·048	+0·113	−0·665	62	29	1·812	+0·660	+0·500
30	May 3	1·675	+0·337	+0·514	63	Sept. 3	1·822	+0·674	+0·432
31	4	1·880	+0·340	+0·529	64	5	1·691	+0·679	+0·405
32	6	1·811	+0·345	+0·553	65	7	1·911	+0·685	+0·377
33	12	1·686	+0·361	+0·623	66	8	1·774	+0·687	+0·363

OBSERVATIONS OF THE STAR a (continued).

67	1838. Sept. 12	461″·832	α+0·698 α'	+0·304α''	77	1838. Sept. 23	461″·638	α+0·728 α'	+0·138α''
68	13	1·599	+0·701	+0·289	78	24	1·505	+0·731	+0·122
69	14	1·579	+0·704	+0·273	79	25	1·778	+0·734	+0·106
70	15	1·620	+0·707	+0·259	80	26	1·631	+0·737	+0·090
71	16	1·748	+0·709	+0·244	81	27	1·540	+0·739	+0·075
72	17	1·552	+0·712	+0·229	82	28	1·515	+0·742	+0·059
73	18	1·443	+0·715	+0·214	83	29	1·675	+0·745	+0·043
74	20	1·519	+0·720	+0·183	84	30	1·684	+0·748	+0·027
75	21	1·695	+0·723	+0·168	85	Oct. 1	1·436	+0·750	+0·016
76	22	1·744	+0·726	+0·153					

OBSERVATIONS OF THE STAR b.

1	1838. Aug. 16	706″·572	β−0·375 β'	+0·436β''	30	1838. Feb. 1	706″·199	β+0·088 β'	−0·267β''
2	18	6·434	−0·369	+0·462	31	5	6·123	+0·099	−0·326
3	19	6·783	−0·367	+0·474	32	10	6·127	+0·113	−0·398
4	20	6·684	−0·364	+0·487	33	19	5·887	+0·138	−0·519
5	28	6·147	−0·342	+0·585	34	Mar. 12	6·167	+0·195	−0·749
6	30	6·404	−0·337	+0·609	35	13	5·633	+0·198	−0·758
7	Sept. 4	6·373	−0·323	+0·653	36	May 2	6·083	+0·334	−0·861
8	9	6·650	−0·309	+0·711	37	3	6·075	+0·337	−0·857
9	11	6·296	−0·304	+0·725	38	4	6·214	+0·340	−0·852
10	14	6·567	−0·296	+0·752	39	6	6·303	+0·345	−0·842
11	20	6·594	−0·279	+0·795	40	12	6·301	+0·361	−0·806
12	23	6·517	−0·271	+0·815	41	16	6·270	+0·372	−0·778
13	24	6·354	−0·268	+0·823	42	17	6·094	+0·375	−0·771
14	Oct. 1	6·547	−0·249	+0·855	43	19	6·294	+0·380	−0·754
15	2	6·442	−0·246	+0·859	44	21	6·144	+0·386	−0·737
16	16	6·467	−0·208	+0·891	45	22	6·152	+0·389	−0·728
17	28	6·210	−0·175	+0·876	46	23	6·338	+0·392	−0·719
18	Nov. 22	6·186	−0·107	+0·718	47	June 1	6·299	+0·416	−0·625
19	Dec. 1	6·367	−0·083	+0·625	48	2	6·368	+0·419	−0·618
20	17	6·176	−0·041	+0·430	49	12	6·337	+0·446	−0·496
21	30	6·400	−0·003	+0·241	50	13	6·376	+0·449	−0·486
22	31	6·188	−0·001	+0·236	51	22	6·639	+0·474	−0·366
23	1838. Jan. 5	6·272	+0·015	+0·150	52	26	6·331	+0·485	−0·310
24	6	6·116	+0·018	+0·134	53	27	6·267	+0·488	−0·296
25	8	6·238	+0·023	+0·104	54	28	6·460	+0·490	−0·282
26	10	6·126	+0·028	+0·072	55	29	6·440	+0·493	−0·268
27	14	5·944	+0·039	+0·011	56	30	6·430	+0·496	−0·253
28	17	6·181	+0·047	−0·035	57	July 1	6·603	+0·499	−0·238
29	20	6·312	+0·056	−0·083	58	8	6·568	+0·518	−0·135

OBSERVATIONS OF THE STAR b (continued).

59	1838. July 10	706″·241	$\beta + 0{\cdot}524\,\beta' - 0{\cdot}106\,\beta''$	79	1838. Sept. 13	706″·831	$\beta + 0{\cdot}701\,\beta' + 0{\cdot}744\,\beta''$
60	14	6·437	+0·534 −0·046	80	14	6·696	+0·704 +0·752
61	17	6·391	+0·543 −0·000	81	15	6·899	+0·707 +0·760
62	29	6·610	+0·575 +0·179	82	16	6·743	+0·709 +0·767
63	Aug. 2	6·430	+0·586 +0·230	83	17	6·784	+0·712 +0·775
64	4	6·444	+0·592 +0·268	84	18	6·795	+0·715 +0·782
65	11	6·493	+0·611 +0·365	85	19	6·814	+0·718 +0·789
66	20	6·580	+0·636 +0·485	86	20	6·783	+0·720 +0·796
67	21	6·671	+0·638 +0·496	87	21	6·463	+0·723 +0·803
68	25	6·661	+0·649 +0·549	88	22	6·551	+0·726 +0·810
69	26	6·587	+0·652 +0·560	89	23	6·679	+0·728 +0·813
70	29	6·536	+0·660 +0·598	90	24	6·682	+0·731 +0·822
71	Sept. 3	6·299	+0·674 +0·650	91	25	6·611	+0·734 +0·827
72	4	6·391	+0·676 +0·660	92	26	6·672	+0·737 +0·833
73	5	6·394	+0·679 +0·671	93	27	6·849	+0·739 +0·839
74	6	6·645	+0·682 +0·681	94	28	6·762	+0·742 +0·844
75	7	6·741	+0·685 +0·690	95	29	6·696	+0·745 +0·848
76	8	6·517	+0·687 +0·700	96	30	6·713	+0·748 +0·852
77	12	6·475	+0·698 +0·735	97	Oct. 1	6·717	+0·750 +0·857
78	—	6·500	+0·698 +0·735	98	2	6·721	+0·753 +0·861

If we compare both divisions of these tables, we shall perceive that the agreement of the observations with each other is considerably augmented by giving to α'' and β'' positive values; or, in other words, by admitting a sensible parallax. If we consider this parallax as vanishing, the sum of the squares of the remaining differences of the eighty-five observations of the star a can be diminished only to 4·4487; that of the ninety-eight observations of the star b to 4·7108. If, however, we determine α'' and β'', so that the observations may be represented as exactly as possible, we can reduce these sums to 1·4448 and 2·4469. By this means we obtain the mean error of an observation of the star $a = \pm 0''{\cdot}1327$, of the star $b = \pm 0''{\cdot}1605$. That the observations of the second star are less accurate than those of the first, I consider to be owing to the *difference* of the directions of the two stars with respect to the direction of the double star. The way in which I conceive this difference to effect the result I shall here leave unexplained; but refer to the complete discussion, which I shall enter into at some future time, of the parallax of 61 *Cygni*.

I have employed the preceding list of the observations of the distances of the star 61 *Cygni* from a and b, in two different ways, in order to deduce from it results for the annual parallax of α *Cygni*. I have first assumed α'' and β'' as independent of each other; or, in other words, considered it as not improbable that a and b themselves may possess sensible parallax. In this way I have found,

For the Star a.

		Mean Error.
Mean distance for the beginning of 1838	461″·6094	
Annual variation = + 4″·3915 − 0″·0543	+ 4 ·3372	±0″·0398
Difference of annual parallax of 61 and a ... α'' =	+ 0 ·3690	±0 ·0283

For the Star b.

Mean distance for the beginning of 1838	706 ·2909	
Annual variation = − 2″·825 + 0″·2426	− 2 ·5824	±0 ·0434
Difference of annual parallax of 61 and b ... β'' =	+ 0 ·2605	±0 0278

The observations seem also to indicate, that the difference of the parallaxes of 61 and b is smaller than that of 61 and a; which must be the case, indeed, if b itself have a sensible parallax greater than a. The difference of the computed values of α'' and β'', in fact, exceeds the limits of the probable uncertainty of the observations; but it is to be observed that the probability of *equal* values of α'' and β'' is not so small that we should be inclined to consider the difference of the two as *proved* by the observations. Further observations will increase the weight of both results, and, at the same time, give more accurate values of the annual variations.

I have, therefore, deduced a second result from the observations, which rests on the supposition that the parallaxes of a and b are *insensible;* or that α'' and β'' are equal. For this purpose, since both series must now be brought into connexion with one another, it was necessary to deduce the *weight* of the observations contained in the second series, the weight of those in the first series being taken as unit. I have found it =0·6889; and hence the most probable value of the annual parallax of 61 *Cygni* =0″·3136. On this hypothesis, I find the mean distances of both stars for the beginning of 1838, to be 461″·6171 and 706″·2791; and the corrections of the assumed values of the annual variations, = −0″·0293 and +0″·2395. The mean error of an observation of the kind of which I have assumed the weight as unit, is ± 0″·1354, and the mean error of the annual parallax of 61 *Cygni*, = ± 0″·0202.

This hypothesis manifestly represents the observations somewhat less correctly than the first calculation which was instituted; but what we lose in this respect is not sufficient to outweigh the decided preference due to this last calculation. We can form a judgment upon this point by the following lists of errors of the observations, which contain their comparisons with two formulæ; namely, that of the first calculation and the present hypothesis. I have also added a third column, which contains the errors that arise when we assume the parallaxes α'' and β'' in the first formula as vanishing. This column also shews immediately what differences were still to be explained by the annual parallax. It shews, in fact, that these differences are commonly positive or negative, according as the coefficient of the annual parallax, which the foregoing tables give, is positive or negative.

OBSERVATIONS OF THE STAR *a*.

	I.	II.	III.		I.	II.	III.
1	+0″·19	+0″·22	+0″·42	35	+0″·17	+0″·20	+0″·43
2	−0·24	−0·21	−0·01	36	−0·03	−0·01	+0·23
3	−0·16	−0·13	+0·06	37	+0·05	+0·07	+0·31
4	−0·09	−0·06	+0·10	38	−0·02	0·00	+0·25
5	+0·13	+0·16	+0·31	39	+0·12	+0·14	+0·39
6	+0·13	+0·16	+0·28	40	−0·01	+0·02	+0·29
7	+0·09	+0·11	+0·22	41	+0·21	+0·24	+0·51
8	−0·16	−0·14	−0·03	42	−0·04	−0·02	+0·28
9	−0·11	−0·09	+0·01	43	+0·04	+0·07	+0·37
10	+0·05	+0·07	+0·15	44	−0·26	−0·23	+0·07
11	−0·19	−0·18	−0·12	45	−0·04	−0·01	+0·30
12	+0·14	+0·15	+0·19	46	+0·01	+0·05	+0·36
13	−0·08	−0·07	−0·03	47	+0·19	+0·22	+0·53
14	−0·01	−0·01	−0·01	48	+0·21	+0·24	+0·55
15	+0·14	+0·14	+0·14	49	+0·24	+0·27	+0·59
16	+0·17	+0·16	+0·09	50	−0·14	−0·10	+0·21
17	+0·04	+0·03	−0·11	51	−0·14	−0·11	+0·20
18	+0·04	−0·01	−0·22	52	+0·01	+0·04	+0·35
19	0·00	−0·06	−0·29	53	−0·28	−0·26	+0·05
20	−0·05	−0·10	−0·38	54	−0·06	−0·03	+0·27
21	+0·03	−0·03	−0·30	55	+0·09	+0·11	+0·39
22	−0·11	−0·17	−0·44	56	−0·05	−0·03	+0·24
23	−0·06	−0·11	−0·38	57	−0·04	−0·02	+0·23
24	−0·12	−0·17	−0·43	58	−0·22	−0·21	+0·01
25	+0·19	+0·14	−0·12	59	+0·04	+0·05	+0·25
26	−0·19	−0·24	−0·49	60	−0·07	−0·06	+0·13
27	+0·16	+0·11	−0·11	61	0·00	0·00	+0·20
28	+0·28	+0·23	+0·02	62	+0·05	+0·06	+0·24
29	−0·31	−0·36	−0·55	63	+0·09	+0·09	+0·25
30	−0·10	−0·09	+0·08	64	−0·03	−0·03	+0·12
31	+0·10	+0·11	+0·30	65	+0·20	+0·20	+0·34
32	+0·02	+0·03	+0·22	66	+0·07	+0·06	+0·20
33	−0·13	−0·12	+0·10	67	+0·15	+0·14	+0·26
34	+0·08	+0·10	+0·33	68	−0·08	−0·09	+0·03

OBSERVATIONS OF THE STAR *a* (continued).

	I.	II.	III.		I.	II.	III.
69	−0″·09	−0″·10	+0″·01	78	−0″·11	−0″·13	−0″·06
70	−0·05	−0·06	+0·05	79	+0·17	+0·15	+0·21
71	+0·09	+0·08	+0·18	80	+0·03	+0·01	+0·06
72	−0·10	−0·12	−0·02	81	−0·06	−0·08	−0·03
73	−0·21	−0·22	−0·13	82	−0·08	−0·10	−0·05
74	−0·12	−0·13	−0·05	83	+0·09	+0·07	+0·11
75	+0·06	+0·05	+0·12	84	+0·11	+0·08	+0·11
76	+0·12	+0·10	+0·17	85	−0·14	−0·16	−0·13
77	+0·02	0·00	+0·07				

OBSERVATIONS OF THE STAR *b*.

	I.	II.	III.		I.	II.	III.
1	+0·26	+0·24	+0·37	27	−0·36	−0·35	−0·36
2	+0·11	+0·10	+0·23	28	−0·11	−0·10	−0·12
3	+0·46	+0·44	+0·58	29	+0·03	+0·05	+0·01
4	+0·35	+0·34	+0·48	30	−0·04	−0·02	−0·11
5	−0·21	−0·23	−0·06	31	−0·11	−0·08	−0·19
6	+0·04	+0·02	+0·20	32	−0·09	−0·05	−0·19
7	−0·01	−0·03	+0·16	33	−0·30	−0·26	−0·44
8	+0·25	+0·22	+0·43	34	+0·02	+0·08	−0·17
9	−0·11	−0·14	+0·08	35	−0·51	−0·45	−0·71
10	+0·15	+0·12	+0·35	36	−0·06	−0·01	−0·29
11	+0·16	+0·13	+0·37	37	−0·08	−0·02	−0·30
12	+0·08	+0·05	+0·29	38	+0·06	+0·12	−0·16
13	−0·09	−0·12	+0·13	39	+0·15	+0·21	−0·07
14	+0·09	+0·06	+0·32	40	+0·13	+0·19	−0·08
15	−0·01	−0·05	+0·21	41	+0·10	+0·15	−0·11
16	−0·01	−0·04	+0·23	42	−0·09	−0·03	−0·29
17	−0·27	−0·30	−0·04	43	+0·11	+0·16	−0·09
18	−0·27	−0·29	−0·08	44	−0·05	0·00	−0·24
19	−0·07	−0·09	+0·10	45	−0·04	+0·01	−0·23
20	−0·22	−0·23	−0·11	46	+0·14	+0·19	−0·05
21	+0·05	+0·05	+0·11	47	+0·07	+0·12	−0·09
22	−0·16	−0·17	−0·10	48	+0·14	+0·18	−0·03
23	−0·06	−0·06	−0·02	49	+0·07	+0·11	−0·05
24	−0·21	−0·21	−0·18	50	+0·10	+0·14	−0·02
25	−0·09	−0·08	−0·06	51	+0·33	+0·36	+0·23
26	−0·19	−0·18	−0·17	52	0·00	−0·03	+0·08

OBSERVATIONS OF THE STAR *b* (continued).

	I.	II.	III.		I.	II.	III.
53	−0″·06	−0″·04	−0″·14	76	−0″·12	−0″·15	+0″·06
54	+0·12	+0·15	+0·05	77	−0·18	−0·20	+0·02
55	+0·10	+0·13	+0·03	78	−0·15	−0·18	+0·04
56	+0·08	+0·11	+0·02	79	+0·18	+0·15	+0·37
57	+0·25	+0·28	+0·19	80	+0·04	+0·01	+0·23
58	+0·19	+0·21	+0·15	81	+0·24	+0·21	+0·43
59	−0·15	−0·13	−0·18	82	+0·08	+0·05	+0·28
60	+0·03	+0·04	+0·02	83	+0·12	+0·09	+0·32
61	−0·03	−0·02	−0·03	84	+0·13	+0·10	+0·33
62	+0·13	+0·14	+0·18	85	+0·14	+0·11	+0·35
63	−0·06	−0·06	0·00	86	+0·11	+0·08	+0·32
64	+0·06	−0·06	+0·01	87	−0·21	−0·24	0·00
65	−0·04	−0·05	+0·05	88	−0·13	−0·16	+0·08
66	+0·01	0·00	+0·13	89	0·00	−0·03	+0·21
67	+0·09	+0·08	+0·23	90	0·00	−0·03	+0·21
68	+0·07	+0·06	+0·21	91	−0·07	−0·10	+0·14
69	−0·01	−0·02	+0·14	92	−0·02	−0·05	+0·20
70	−0·07	−0·09	+0·09	93	+0·16	+0·13	+0·38
71	−0·32	−0·34	−0·16	94	+0·07	+0·04	+0·29
72	−0·24	−0·26	−0·06	95	0·00	−0·03	+0·22
73	−0·24	−0·26	−0·06	96	+0·02	−0·01	+0·24
74	+0·01	−0·01	+0·19	97	+0·02	−0·01	+0·24
75	+0·10	+0·08	+0·28	98	+0·02	−0·01	+0·25

As the mean error of the annual parallax of 61 *Cygni* (=0″·3136) is only ± 0″·0202, and consequently not $\frac{1}{15}$ of its value computed; and as these comparisons shew that the progress of the influence of the parallax, which the observations indicate, follows the theory as nearly as can be expected considering its smallness, we can no longer doubt that this parallax is sensible. Assuming it 0″·3136, we find the distance of the star 61 *Cygni* from the sun 657700 mean distances of the earth from the sun: light employs 10·3 years to traverse this distance. As the annual proper motion of α *Cygni* amounts to 5″·123 of a great circle, the *relative* motion of this star and the sun must be considerably more than sixteen semidiameters of the earth's orbit, and the star must have a constant aberration of more than 52″. When we shall have succeeded in determining the elements of the motion of both the stars forming the double star, round their common centre of gravity, we shall be able also to determine the sum of their masses. I have

attentively considered the preceding observations of the relative positions; but I consider them as yet very inadequate to afford the elements of the orbit. I consider them sufficient only to shew that the annual angular motion is somewhere about $\frac{2}{3}$ of a degree; and that the distance, at the beginning of this century, had a minimum of about 15″. We are enabled hence to conclude that the time of a revolution is more than 540 years, and that the semi-major axis of the orbit is seen under an angle of more than 15″. If, however, we proceed from these numbers, which are merely *limits*, we find the sum of the masses of both stars less than half the sun's mass. But this point, which is deserving of attention, cannot be established until the observations shall be sufficient to determine the elements accurately. When long-continued observations of the places which the double star occupies amongst the small stars which surround it, shall have led to the knowledge of its centre of gravity, we shall be enabled to determine the two masses separately. But we cannot anticipate the time of these further researches.

I have here troubled you with many particulars; but I trust it is not necessary to offer any excuse for this, since a correct opinion as to whether the investigation of the parallax of 61 *Cygni* has already led to an approximate result, or must still be carried further before this can be affirmed of them, can only be formed from the knowledge of those particulars. Had I merely communicated to you the result, I could not have expected that you would attribute to it that certainty which, according to my own judgment, it possesses. I have the honour to be, esteemed Sir, yours,

F. W. Bessel.

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *December* 14, 1838. No. 18.

The following communications were read :—

I. Extract of a letter from Professor Bessel to Sir J. Herschel, Bart., dated Konigsberg, Nov. 4, 1838.

The means which I have employed to ascertain the effect of temperature upon the measures by the heliometer, consist in observing such of the stars of the *Pleiades* as are visible in the coldest winter, by night, and in the warmest summer, by day. Soon after the instrument was set up (in November and December 1829) I made a series of observations of this kind, and repeated them in the summer of 1830. From these I found the value of one revolution of the screw, in the temperature f of Fahrenheit,

$$= 52''\cdot 91788 - (f - 49\cdot 2)0''\cdot 0004493$$ (*Astro. Nach.* No. 189, p. 418).

Further observations, however, have reduced the value of the last term of the formula to $0''\cdot 0003912$: this latter value is that which I have employed in the reduction of the observations of 61 *Cygni*. If this correction of the measures had been altogether neglected, the result, which the star a affords, would have been in error about $0''\cdot 06$; but, in the case of the star b, the effect would be altogether inappreciable, since the maximum of the influence in question takes place at that time of the year in which the parallax disappears. I owe this explanation to you, since you have enquired expressly as to this point; and, moreover, it could not be indifferent to me that an astronomer to whose opinion I attach so much importance, should not only be partially, but also thoroughly, satisfied as to the parallax of 61 *Cygni*.

After nine years' service, I resolved to take the heliometer to pieces, in order to examine anew all the parts of the mechanism of this very ponderous instrument; and to provide, in time, against any damage it may have sustained. The whole, however, is so solidly and durably constructed, that it has been found to need scarcely any repair. I have taken the opportunity of making some alterations for the greater convenience of the observer. The instrument, on account of these circumstances, has been nearly three weeks out of use: it is now, however, again in a fit state for observation.

I am particularly anxious to obtain your physical observations

of the comet. Struve has lately communicated to me his own, which differ considerably from mine, as they shew the tail defined, whereas it appeared to me undefined;  Struve,  Bessel. In other respects these observations are similar to mine, except that they go more into detail. It appears that I am the only one who has had the good fortune to be able to follow the comet during an entire night, in which the motion of the tail fell in its own direction. According to the letter which I had the pleasure to receive from you, the comet seems latterly to have lost its tail altogether; at least you mention only the complete definition of the disc, which also I consider a very important observation.

At the approaching disappearance of *Saturn's* ring, sufficiently powerful telescopes will probably be employed to shew *all* the satellites of the planet. I believe that large reflecting telescopes will begin to supersede achromatic ones; at least, I have no doubt they are capable of greater perfection. They *can* be made with mathematical precision, which is not the case with achromatic telescopes. I think, also, that opticians would have devoted their attention to them in preference, if they had not been discouraged by their more rapid destructibility. If the method of making an indestructible *metallic* surface could be discovered, I should no longer doubt of a still further perfection of the reflecting telescope. Could not *hard* steel be made available? and would it not, if proper care was taken of it, be less destructible than the common metallic reflector?

II. Errors of Heliocentric Longitude and Ecliptic Polar Distance of the planet *Venus*, computed from the Tabular Errors of R. A. and N. P. D., given in the Cambridge Observations of 1836. By the Rev. R. Main.

Having been engaged in correcting the Elements of the Orbit of *Venus*, it occurred to Mr. Main that the reduction of the Tabular Errors of R. A. and N. P. D., derived from the Cambridge Observations of 1836, to errors of Heliocentric Longitudes and Ec. P. D., would form a desirable supplement to his papers.

He has added the equations which arise for the corrections of the Node and Inclination of the Orbit; and it is his intention to form those for the corrections of the four remaining elements.

He has used Mr. Airy's formulæ contained in the tenth volume of the Society's *Memoirs*, and divided the observations into groups of about the same length; applying the same corrections to the Right Ascensions for the Error of the Equinoctial Point.

The general agreement between the errors, and those given in the *Greenwich Observations for* 1836, shews the goodness of the observations, and gives additional confidence in the results to be derived from them.

The errors of the tables are then given, and equations for correcting the elements.

III. A Catalogue of 726 Stars, reduced to the year 1830, and deduced from the Observations made at Cambridge in the years 1828–1835. By G. B. Airy, Esq., Astronomer Royal.

The state of reduction in which the places of the stars had been published in each of the annual volumes of the *Cambridge Observations*, left little to be done for the formation of this catalogue, except the combination of the results of the different years. This was done by applying to the mean of each year's results the annual variation in the Catalogue of this Society (except for stars near the pole), so as to bring the places up to Jan. 1, 1830; and then taking the mean of the different results for 1830, giving to each year a weight proportioned to the number of observations. Special methods of reducing some of the observations are fully explained in the preface, to which it is not requisite here to allude further; and a list of the principal discordances are subjoined, as well as of some of the observations that have been omitted in the reductions.

IV. Extract of a Letter from Mr. Henderson to Mr. Baily, relative to the late Annual Eclipse of the Sun on May 15, 1836.

After correcting the places of the sun and moon, and their semidiameters, by the quantities mentioned in my last letter, I computed the beginning and end of the eclipse, and of the annulus; and I annex the *computed* times of these phenomena, and also the *observed*.

	Computed sid. Time.	Observed Time.
	h m s	h m s
Beginning of eclipse	5 6 9,2	not observed.
At formation of annulus,		
First appearance of detached luminous portions of sun's limb		6 30 41,1
Internal contact of sun and moon	6 30 46,2	
Annulus completely formed by disappearance of black spots		6 30 50,1
At dissolution of annulus,		
Annulus broken by appearance of black spots		6 34 33,0
Internal contact of sun and moon	6 34 40,3	
Disappearance of detached luminous portions of sun's limb		6 34 44,0
End of eclipse	7 53 7,5	7 53 {0,8 / 7,8}

It thus appears, that the true internal contacts happened between the moments when the beads of light were observed to appear and disappear, and when the black spots disappeared and appeared. At the formation of the annulus, when the beads appeared, 1"·6 of the moon (a segment of that *maximum* breadth) seems to have been beyond the sun's disc; and when the black spots disappeared, the annulus was 1"·1 in breadth, where least. This agrees with the appearance observed, " that the annulus was

seen completely formed of sensible breadth at the narrowest part." Again, at the dissolution of the annulus, when the black spots appeared, it is found that the least breadth of the ring was $2''\cdot3$, also agreeing with the actual observation, that "the annulus, being of sensible breadth, was suddenly broken;" and when the beads disappeared, $1''\cdot2$ of the moon was beyond the sun's disc. The same telescope was used for observing the formation and dissolution of the annulus, and for making the observations from which the corrections of the elements have been determined.

On applying the same corrections, I find that in latitude 55° 27′ 30″ N. and longitude $10^m\ 12^s,0$ W. of Greenwich, the true

internal contacts were at	Mean Time. h m s	Observed by you at	Mean Time. h m s
	3 0 33		3 0 57
	3 5 7		3 5 23
Duration =	0 4 34	Duration =	0 4 26

I suspect that the longitude of your station is not well determined; but, as a small error in the longitude will not sensibly affect the duration of the annulus, I am inclined to believe that the times you noted are those when the black spots disappeared and appeared. You seem to have been in very nearly the line of the central eclipse, as the least distance of the centres was only 1″.

V. A letter from Mr. Lassell to the Rev. R. Sheepshanks, relative to Observations with a small Sextant.

The sextant, here alluded to, was made by Dollond. It is only 3 inches radius, divided to 20′, and by vernier reading to 30″; but, by means of the reading microscope, subdivisions may be estimated to 10″. The telescope magnifies 6 and 11 times: but the higher power is generally used. The whole packs in a box 4·3 inches square, and 2·7 inches deep. With this instrument Mr. Lassell made a number of observations on various stars, both for the time and latitude, for the express purpose of determining how near to the truth he might be able to approximate by its means. The observations are given in detail, and the result at which Mr. Lassell arrives is, that under ordinary circumstances the mean of one set of altitudes east, and another west, would give the time truly within about one second: and that a set of each, north and south, at something like equal altitudes, would give the latitude within eight or ten seconds.

⁎⁎ The Tenth Volume of the Memoirs of the Society (containing an Index to the whole of the ten volumes) is now finished, and may be had as usual of the publishers.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *January* 11, 1839. No. 19.

The following communications were read:—

I. On the Obliquity of the Ecliptic. By the Rev. Dr. Pearson.

The object of this paper is to give an account of a determination of the annual diminution of the obliquity, by a comparison of observations of the solstices made by Dr. Pearson at South Kilworth, with similar observations of Bradley at the Royal Observatory. Dr. Pearson remarks, that the most remote determination of the obliquity which has any claim to precision, is Dr. Bradley's; and, accordingly, some one of his obliquities has been chosen by almost all practical astronomers for the purpose of comparison with their own, in order to determine the annual diminution. But, notwithstanding the apparent facility with which the diminution may be obtained, by a comparison of determinations separated by a considerable interval of time, no two astronomers agree in their results. Dr. Maskelyne considered it to be $-0''\cdot52$; Delambre adopted $-0''\cdot48$; Dr. Brinkley, $-0''\cdot43$; and Bessel, $-0''\cdot457$. These discrepancies, which in the course of years amount to considerable quantities, demand that the question of preference should be settled; and this can only be effected by practical methods.

Bradley determined his summer and winter obliquities by separate deductions,—a method which rendered the result dependent on the assumed latitude of the place. He assumed the latitude of the observatory to be $51^\circ\ 28'\ 40''$, which has since been shewn to be too great by at least one, if not two, seconds; and, accordingly, all his obliquities are affected with a corresponding error, which may explain the reason why the Greenwich winter obliquities are smaller by some seconds than the summer ones. But it is easy to see that, by combining the observations of two successive solstices, the latitude may be eliminated; for, half the difference of the sun's extreme meridian altitudes gives the obliquity due to the middle of the interval, which, if the winter solstice be taken first, will be the vernal equinox. In like manner, if we take three, or any *odd* number of successive obliquities, separately deduced by means of an assumed colatitude, the sum of the whole, divided by their number, will give the mean obliquity belonging to the middle epoch, independent of the assumed colatitude.

Bradley's first recorded determination of an obliquity was in the winter of 1753; and he observed seven winter solstices, and as many summer ones, without interruption. Omitting the first determination, in order to have an odd number, and taking the thirteenth part of the sum of the remaining thirteen half-yearly determinations, we have an average obliquity corresponding to June 1757, viz. 23° 28′ 13″·4446.

Dr. Pearson commenced his solstitial observations in June 1828, and continued them till June 1838, thereby obtaining twenty-one successive half-yearly obliquities, the sum of which gives an average obliquity corresponding to June 1833. His result is 23° 27′ 39″·2409, and is therefore less than the average resulting from Bradley's determinations by 34″·2037. Dividing this difference by 76, the number of years between the two epochs (1757—1833), the annual diminution is found = —0″·4500. This accords very nearly with the annual diminution adopted by Bessel in the *Tabulæ Regiomontanæ*.

The instrument with which the observations were made, is an altitude and azimuth instrument, described circumstantially in Vol. II. Part I. of the *Memoirs*. Dr. Pearson describes the mode in which the instrument was used and its errors corrected, together with the methods followed in reducing the observations, and the elements employed in computing the corrections for parallax, refraction, nutation, and the sun's latitude; and concludes with a synopsis of the reduced observations, which were in number 1648, and a table of the mean obliquity on the 1st of January in each year, from 1750 to 1900, both inclusive, deduced from the above determination of the annual diminution.

II. On the Parallax of *α Centauri*. By Professor Henderson.

The two stars designated α^1 and α^2 *Centauri*, are situated within 19″ of space of each other. On comparing the observations of Lacaille with those of the present time, it has been found that, although the two stars have not sensibly changed their relative positions, each has an annual proper motion of 3·6 seconds of space. It thus appears that they form a binary system, having one of the greatest proper motions that have been observed; and from this circumstance, and the brightness of the stars, it is reasonable to suppose that their parallax may be sufficiently sensible to powerful instruments.

On reducing the declinations from his observations at the Cape of Good Hope, Mr. Henderson remarks, that a sensible parallax appeared, but he delayed communicating the result until it should be seen whether it was confirmed by the observations of Right Ascension made by Lieutenant Meadows, with the transit instrument. He now finds that these observations also indicate a sensible parallax.

It is to be observed, that the observations both of right ascension and of declination were not made for the purpose of ascertaining the parallax, but of determining the mean places of

the stars with a proper degree of accuracy. Had the author been aware of the proper motion at an earlier period, a much greater number of observations, and of such as would have been better adapted for ascertaining the parallax, would have been made, and the result thereby rendered more secure.

The right ascensions and declinations of the two stars (which are always above the horizon of the Cape, and favourably placed for observation at all seasons), have been determined by comparisons with such of the principal, or standard stars, as were observed on the same day. It is consequently assumed that the latter have no sensible parallax. The mean places of the standard stars, or rather their relative positions, are also assumed to be known; and, in reducing the observations to the beginning of 1833, the coefficient of aberration has been assumed $= 20''\cdot5$, and that of lunar nutation $= 9''\cdot25$. Recent observations make the coefficient of aberration less; but a term is introduced into the equations of condition, by which the effect of a change in the aberration is immediately obtained.

For the determination of the parallaxes, three systems of equations of condition are formed for each star, namely, from the observations of right ascension, the direct observations of declination, and the reflected observations of declination. On resolving the equations by the method of least squares, and assuming the coefficient of aberration to be $20''\cdot36$, Mr. Henderson finds the following results:—

Parallax of α^1 *Centauri* =

$+0''\cdot92$; probable error $0''\cdot35$; from observations of right ascension.
$+1\ \cdot42$; probable error $0\ \cdot19$; from direct observations of declination.
$+1\ \cdot96$; probable error $0\ \cdot47$; from reflected observations of declination.

And $= +1''\cdot38$, with a probable error of $0''\cdot16$, by taking a mean of the three determinations according to their weights.

Parallax of α^2 *Centauri* =

$+0''\cdot48$; probable error $0''\cdot34$; from observations of right ascension.
$+1\ \cdot05$; probable error $0\ \cdot18$; from direct observations of declination.
$+1\ \cdot21$; probable error $0\ \cdot64$; from reflected observations of declination.

And $= +0''\cdot94$, with a probable error of $0''\cdot16$, by taking the mean according to their weights.

If we suppose that the two stars are at the same distance, then the parallax $= +1''\cdot16$, with a problem error of $0''\cdot11$. It therefore appears probable, that these stars have a sensible parallax of about one second of space.

Mr. Baily, the president, called the attention of the meeting to the recent accounts which had been received from America, relative to the late eclipse of the sun, which was annular in several parts of the United States. He alluded more especially to that remarkable phenomenon which he himself had observed in the annular eclipse

of May 15, 1836, as published in the tenth volume of the *Memoirs*. The present observations were laid before the American Philosophical Society; an abstract of which has recently been received in this country. These accounts include the observations of fifteen observers in the city of Philadelphia, and of ten in other parts of the Union; the whole of whom were on the watch for the phenomenon alluded to by Mr. Baily. Although the statement is obscurely detailed, it appears that, as in the case of former annular eclipses, some of the observers did, and some did not, notice the luminous beads and dark lines immediately preceding the formation and dissolution of the annulus. The major part of them, however, observed the luminous beads, and several of them noticed the dark lines: and one of these observers says, "the lucid points and dark intervening spaces corresponded closely to Baily's description."

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *February* 8, 1839. No. 20.

Report of the Council of the Society to the Nineteenth Annual General Meeting, held this Day.

Agreeably to the custom hitherto pursued at the Anniversary Meetings of this Society, the Council now proceed to notice such subjects as have occupied their attention during the preceding year, or relate to new improvements or discoveries which have been made in the several branches of astronomy. Amongst the latter, the Council have great pleasure in calling the attention of the Fellows to the labours of M. Bessel and Mr. Henderson, in regard to the parallax of the fixed stars, a subject which has hitherto given rise to much discussion and controversy; some astronomers maintaining that it *does*, and others that it does *not*, exist, even when the same star has been the object under investigation. M. Bessel has stated that his observations indicate a parallax of about one-third of a second of space as belonging to 61 *Cygni;* and Mr. Henderson has deduced a probable parallax of about one second from his observations of α *Centauri.* The Council trust that this subject will not be lost sight of by other astronomers; and that the apparent success which has attended the researches of these two observers will induce others to pursue this inquiry in different parts of the heavens; so that the delicate and important question of the existence or non-existence of a measurable parallax may at length be set at rest.

As connected with the advancement of Astronomy, the Council have likewise to notice the appearance of the *first* volume of the Observations made at the Royal Observatory at Edinburgh, by Mr. Henderson; whereby a new extension has been given to the sources of astronomical information which are annually opened up to the public. Mr. Henderson's talents and skill are well known to, and duly appreciated by, the Fellows of this Society. In arranging the materials of the present work, he has adopted the plan originally introduced by Mr. Airy at Cambridge, and since pursued at Greenwich, of reducing every observation, so that it may be at once available for immediate use: a plan which presents so many advantages to the astronomer, that, sooner or later, it must be adopted in every observatory. This volume has, at the recommendation of the Council, been printed at the public expense; and a certain number of copies have been placed by the Government at the disposal of the Council, for distribution amongst the principal astronomers and public institutions.

In alluding to this latter subject, the Council take this opportunity of mentioning that, by Her Majesty's warrant, they are

authorised to distribute also a certain number of the volumes of the *Greenwich Observations*; and a list of the persons and institutions entitled thereto is, from time to time, published in the Memoirs of this Society. Many of the parties, however, have not availed themselves of their privilege; and these neglected volumes, which are fast accumulating, encumber the shelves of the Society. Under these circumstances, the Council have ordered that, hereafter, all copies not claimed within six months, shall become forfeited; and the names of the respective parties will then be struck out of the list.

The Council have great pleasure in noticing the flattering state of their finances, as exhibited in the following abstract from the report of the Auditors, which shews that all the claims on the Society have been liquidated, and that a sufficient balance is left in the hands of the Treasurer to meet all the estimates of the ensuing year.

RECEIPTS.

	£.	s.	d.
Balance of last year's Account	285	1	0
1 year's dividend on £500 Consols	15	0	0
1 year's ditto on £1536 14s. 2d. New 3½ per Cents	53	15	8
Sale of Memoirs	148	16	11
On account of arrears	75	12	0
70 annual contributions, (1838–1839)	147	0	0
4 compositions	84	0	0
20 admission fees	42	0	0
16 first year's contributions	29	8	0
1 non-resident removed	4	4	0
	£884	17	7

PAYMENTS.

				£.	s.	d.
J. Weale, for paper and printing Volume X.				253	16	9
J. Basire, for engraving and printing Plate VI.				15	3	6
J. Moyes, for paper and printing Catalogue of Library				35	12	3
Ditto, ditto, Monthly Notices (two years)				64	13	6
Ditto, ditto, sundries				6	12	9
H. Hoppe, for charges relative to Hartwell advowson				17	14	10
Purchase of £62 14s. 10d. New 3½ per Cents				63	0	0
Quarter of a year's salary to Mr. Epps				25	0	0
Three quarters, ditto, to Mr. Hartnup				60	0	0
J. Epps, for book-cases and shelves				5	9	4
Stationery				7	2	5
Coals, candles, &c.				13	5	8
Postage, porterage, and porter's work at the apartments				9	14	5
Charwoman, and cleaning apartments				5	0	0
Sundry other disbursements by the treasurer				2	14	9
Commission for collecting, and on the sale of Memoirs				15	14	0
Tea, sugar, cakes, &c. for evening meetings				11	14	0
Taxes: Poor's rate	6	16	0			
Land tax	3	2	6			
Church and rector's rate	2	3	9			
				12	2	3
Balance in the Treasurer's hands (January 31, 1839)				260	7	2
				£884	17	7

The assets and present property of the Society may be estimated as under: viz.

		£.	s.	d.
Balance in the hands of the Treasurer		260	7	2
Arrears, January 31, 1839.				
3 contributions of five years' standing	£31 10 0			
2 ——— of four ditto	16 16 0			
2 ——— of three ditto	12 12 0			
7 ——— of two ditto	29 8 0			
49 ——— of one ditto	102 18 0			
4 admission fees	8 8 0			
2 first year's contributions	4 4 0			
1 ditto ditto	2 2 0			
1 ditto, and admission fee	3 3 0			
		211	1	0
£1578 14s. 2d. 3½ per Cent Annuities, £500 3 per Cent Consols } valued at		2000	0	0

Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

Four of the instruments above alluded to, are now in the Apartments of the Society: viz.

The *Harrison* clock.
The *Owen* double portable circle.
The *Owen* quadruple portable sextant.
The *Fuller* Theodolite.

Two others are, for the purpose of safe custody, placed as under: viz.

A brass quadrant, said to be Lacaille's, at the Royal Society.
The Standard scale, at Mr. Baily's.

The remainder are lent, during pleasure, to the several parties under mentioned: viz.

The *Beaufoy* circle, } to the Rev. M. Ward.
The *Beaufoy* clock, }
The *Beaufoy* transit, to Mr. T. Jones.
The other *Beaufoy* clock, to Col. Pasley, R.E.
The *Lee* circle, to Capt. Smyth, R.N.
The *Wollaston* telescope, to Professor Schumacher.
The two invariable pendulums to Col. Chesney.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year: viz.

	Compounded.	Annual Contributors.	Non-resident.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1838	71	136	94	6	307	39	346
Since elected	2	12	...	...	14	...	14
Deceased, &c.	−1	−2	−3	...	−6	−2	−8
Resigned	...	−7	...	...	−7	...	−7
Removals	+2	−2	...	...	...	...	...
February 1839	74	137	91	6	308	37	345

Since the last Anniversary, the Council have to regret the loss of several of its Members by death. Of these, the name of one only appears on the foreign list, which is that of Dr. Bowditch.

Dr. Nathaniel Bowditch was the son of a cooper at Salem, in Massachusetts, and was born at that place in 1773. His education was of the most limited kind; and the acquirements by which he has made his name celebrated were entirely the result of his own unaided studies. He was successively a merchant's clerk, the supercargo of a vessel, and the actuary of an insurance office at Boston. He died March 1838.

Dr. Bowditch was the author of a work on navigation, much used in his own country. He was also, as is well known, the translator and commentator of the *Mécanique Céleste*. This laborious undertaking was performed, as appears by the account given in the Preface, between 1815 and 1817, the notes having previously been written, in reading the several volumes of the original, as they appeared. A proposal made at that time by the American Academy of Arts and Sciences, to defray the expense of the publication, was not accepted; one reason being, that Dr. Bowditch wished to defer the completion of his translation, in the expectation that Laplace himself would publish another edition. On the death of Laplace, in 1827, Dr. Bowditch began to prepare for printing at his own expense: and the first volume of the translation accordingly appeared in 1829. The second and third volumes, with an appendix to the latter, have been since published; and the fourth volume was going through the press when the translator was removed by death. It is understood, however, that this remaining volume may be expected in a very short time.

The commentary on the *Mécanique Céleste* is composed of developements of all those parts of the processes which the author has not filled up. It bears the mark of being what the translator describes, namely, a collection of notes made in acquiring the first comprehension of the author. An expert mathematician would find most of them needless; but to the student who has sufficient knowledge to understand, without the habit of previous investigation which the reading of Laplace always requires, the work of Dr. Bowditch is invaluable. We see in it, not the performance of a practised analyst, but the record of the steps by which the translator became one; and a person more familiar with the most modern form of analysis than he appears to have been at the time when he wrote, would probably have filled his commentary with difficulties of the same order as those of his author. This, however, was not done; and the work, as it stands, is most unquestionably fitted to bring the *Mécanique Céleste* within the grasp of a number of students, exceeding five times, at least, that of those who could master Laplace by themselves.

The appendix to the third volume, published in 1834, contains a digested account of the methods for the computation of the orbit of a comet or planet, with supplementary tables, which include all those published by Gauss in his *Theoria*.

The name of Dr. Bowditch must be long remembered in the

United States by the impulse which such a work as his commentary cannot fail to give to the study of analysis in that country. The undertaking required sound knowledge, power of combining, brevity and clearness, an accurate remembrance of the nature of a beginner's difficulties, and determined perseverance. The congratulations which this Society would so cordially have offered to the performer of this strikingly useful task, can now only be forwarded, with expressions of sympathy and condolence, to his surviving relatives and friends: with the hope that astronomy, theoretical and practical, will flourish in the country which has produced so remarkable a facilitation of the study of the former, and so sound an example of the union of both.

It is understood that Dr. Bowditch has bequeathed his books to the state of Massachusetts, and that a public library is to be formed, of which they will compose a nucleus.

Amongst the losses on the home list, the Council have to notice, with much regret, that of Mr. Timothy Bramah, one of the members of the Council, who, although a great admirer of the science of astronomy, was prevented, by his numerous avocations and his extensive business, from entering so much into its pursuits as he could have otherwise wished. In early life he applied himself very assiduously and successfully to the acquirement of the knowledge of those sciences more immediately connected with the profession of an Engineer. His attainments in this branch of knowledge are well known to the country; and many of his inventions exhibit instances of great ingenuity and skill. It was to him that application was made to form the two large leaden spheres that were to be used in the repetition of the Cavendish experiment for determining the mean density of the earth; and the result has shewn that the execution of the task could not have been placed in better hands. Much time and labour were employed in even constructing the requisite tools; and the final turning of such heavy masses has been accomplished with great precision, and leaves nothing to be desired on this point. Mr. Bramah was fond of studying Chemistry, Medicine, and Music; in which latter art he was a considerable proficient. He possessed an acute understanding, with great intellectual powers; but the most estimable part of his character was his moral excellence, and his benevolent disposition.

In referring to Dr. Hunt, the Council cannot forbear remarking how great a loss has been sustained. He was a person of no ordinary character, whether we consider the endowments of his mind, or his numerous and great acquirements. In early life, he was remarkable rather for power of memory, than for what can be called great application; but as time advanced, and opportunities of acquiring information presented themselves, he was no less distinguished for diligence, than he was for retaining what he had learnt. Dr. Hunt was educated at the Grammar School of Newcastle upon Tyne, under the celebrated Abel Moyses; and afterwards at Trinity College, Cambridge. He was appointed Chaplain in Lord Elgin's embassy to Constantinople in 1798; and it was principally

at his suggestion, and through his patient and indefatigable perseverance, that the best parts of the frieze of the Parthenon were removed from their original position, and conveyed to this country. In 1806, he accompanied the Duke of Bedford to Ireland, in the capacity of private secretary, in which station he conducted himself with so much judgment and prudence, as to ensure his Grace's friendship through life. Shortly after this time, we find him settled upon church preferment which he held in Bedford; where, by the activity of his habits, his extensive information, his knowledge of the principles as well as practice of magisterial duties, his zeal for the public good, his willing attention to all kinds of public business, combined with the exercise of a charitable disposition, he obtained for himself the reputation of being one of the most useful and important persons in his neighbourhood, as well as a most agreeable and instructive companion. Dr. Hunt's favourite pursuit was the study of antiquities, and particularly those of Greece and Rome; and the splendid volume upon the Woburn Marbles, will remain a monument of his taste and critical knowledge. Yet he was by no means deficient in the exact sciences, and was well versed in physical Astronomy; and there is no doubt but had he applied himself to the subject more early in life, he would have excelled as a practical observer. He died, after a painful illness, on the 17th of September last, a Prebendary in the cathedral church of Canterbury, to which dignity he was preferred, without solicitation, by Earl Grey, in the year 1833.

In the last Report of the Council, it was stated that the late Mr. Groombridge had requested that his MS. books of observations should be deposited with this Society. This request has been complied with; and the Lords Commissioners of the Admiralty having forwarded them to the Council, they are now placed in the custody of the Society.

In was also mentioned, in that Report, that the 10th volume of the *Memoirs* of the Society was in the press. This volume has since been published, accompanied with an Index to the whole of the ten volumes. Its contents will be found not to yield in interest and importance to any that preceeded it.

A similar announcement was made respecting the Catalogue of the Books belonging to the Society; which is now printed, and may be had by the Fellows on application to the Assistant Secretary.

In the former volumes of the *Memoirs*, a collection of observations of moon-culminating stars, made at various observatories, was published from time to time: similar collections have also been published up to the middle of last year, in the Monthly Notices of this Society, for the purpose of affording to voyagers, and others, an easy and ready reference for corresponding observations. This plan was originally adopted with the view of introducing and facilitating the method of determining the longitude from such observations, then not sufficiently known in this country. The present regular practice,

however, of publishing annually the observations made at Greenwich, Cambridge, and Edinburgh, render it the less necessary to continue this course any longer; since such voyagers, and others, may now, without the chance of much delay, readily consult the original authorities for such information as may be required. The Council, therefore, have directed that such lists shall in future be discontinued in the Volumes and in the Monthly Notices.

There is one portion of the present by-laws with which the Council have in most instances found it extremely difficult to deal. This relates to the case of persons originally elected as non-resident members, but who afterwards become resident, and are then consequently liable to the annual payments. By the present law, such change of residence ought to be announced to the Society by the respective parties. This, however, is in most cases neglected; no doubt, from an inattention to, or ignorance of, the law: and when the fact becomes known, a large amount of arrears has probably accumulated, which, when applied for, creates surprise and dissatisfaction. To remedy, in some measure, this inconvenience, the Council have resolved to submit to the consideration of the present meeting, a new by-law, whereby it is proposed that all non-resident members elected prior to February 1831, and who have become, or may become, resident, may compound for their subsequent annual payments for the sum of twelve guineas. This motion will be put from the chair in the usual manner.*

The Council regret that they have to announce to the meeting the resignation of their two Secretaries, Professor De Morgan and Mr. Bishop, whose avocations prevent them from fulfilling, in so effectual a manner as they could wish, the respective duties of their office. To Professor De Morgan, in particular, the thanks of the Society are due, for the unremitted zeal and attention shewn on all occasions to the business of the Society, during the long period of eight years, and for the exercise of those abilities, which he is so well known to possess, in furthering their interests and the interest of science in general. Both these gentlemen will carry with them, in their retirement from office, the best wishes of the Council and of the Society for their health and happiness, and that they may long continue amongst us, encouraging by their presence and their labours the advancement and promotion of astronomy.

In the Report of the Council at the Anniversary in the year 1837, honourable mention was made of a catalogue of the right ascension of 1318 stars, by Mr. Wrottesley. Since the publication of that catalogue (which appears in the present volume of the Memoirs), it has been subjected to a more minute investigation and examination, and has been found to deserve all that had been said in its commendation. As this is one of the subjects which the Society, at its original formation, professed to encourage and reward, and as the catalogue in question is one of considerable merit, and exhibits, most distinctly, the great zeal and perseverance

* The resolution here alluded to was passed unanimously.

of the author, the Council have awarded him the Gold Medal of the Society. The Medal will accordingly be delivered to Mr. Wrottesley in the course of the proceedings of this day.

The Council cannot close this Report without again appealing to the Fellows, individually, to exert themselves in furtherance of the objects for which they are united. Every year furnishes new proofs of what may be done by private persons in some one department or another of the science of astronomy. The practical observer may take up subjects that would relieve the public observatories, already fully charged with laborious duties; whilst those attached to more recluse studies will find many points that require investigation and elucidation. One of our foreign members, now deceased, distinguished himself in a more especial manner by looking out for comets; and his zeal was, indeed, amply rewarded by the discovery of many; which, probably, but for his diligence, would for ever have escaped observation. Since his death, those remarkable bodies would seem to have left our system, as no new ones have now been discovered for a number of years. This is one instance (and it would be easy to mention many such) of the good that may be effected by individual exertions. Let us hope, then, that there is still enough of zeal in the several members to select and pursue, with perseverance, some single object that may add to the general stock of information and knowledge, and thus prove the beneficial effects of our union.

Titles of Papers read before the Society, between February 1838 *and February* 1839.

1838.

March 9. Immersion of λ *Cancri* at Moon's dark limb, March 6, 1838. By R. Snow, Esq.

Extract of a Letter from Sir John Herschel to the President, giving an account of a remarkable increase of magnitude of the Star η in the constellation *Argo*, observed by him at the Cape of Good Hope, December 16-17, 1837.

Value of the Mass of *Uranus*, deduced from observations of its Satellites, made at the Royal Observatory of Munich during the year 1837. By Dr. F. Lamont, Director of the Royal Observatory.

Stars observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the months of October, November, and December, 1837.

April 13. On the Correction of the Mean Distance, Excentricity, Epoch, and Longitude, of the Aphelion of the Orbit of *Venus*, by Errors of Heliocentric Longitude, derived from the Cambridge Observations of the years 1833, 1834, and 1835, and the Greenwich Observations of 1836. By the Rev. R. Main.

Moon-culminating Observations made at Rio Janeiro and Valparaiso in 1836. By Capt. F. W. Beechey, R.N.

Times of Emersion of the first and second Satellites of *Jupiter*, observed at Greenwich Hospital Schools, April 9, 1838. By E. Riddle, Esq.

Longitude of the Edinburgh Observatory, compiled from the corresponding Moon-culminating Observations made at Edinburgh and Greenwich, from August 24, 1836, till the end of 1837. By E. Riddle, Esq.

Eclipses of *Jupiter's* Satellites, observed at Edinburgh, by Professor Henderson.

Lunar Occultations observed at Edinburgh. By Professor Henderson.

Immersion of 47 *Geminorum* at Moon's dark limb, April 1, 1838. By R. Snow, Esq.

⁂ The President, at the close of the meeting, read an extract of a letter from Mr. Henderson, relative to the remarkable increase of magnitude in η *Argus*, recently noticed by Sir John Herschel, as mentioned at the last meeting of the Society.

May 11. On the constant of Lunar Nutation, as given by the circle Observations of Mr. Pond, from 1812 to 1835. By the Rev. T. R. Robinson, D.D.

Extract of a letter from Mr. Charles Piazzi Smyth, dated Observatory, Cape of Good Hope, December 17, 1837, on the flexure of a boarded Floor, of the best possible construction.

June 8. An account of some Experiments on two new invariable Pendulums. By Mr. Baily.

Right Ascensions of 125 Stars, chiefly of the 7th magnitude, observed at Ashurst. By R. Snow, Esq.

On the Total Eclipse of the Sun which will happen July 7, 1842, with the elements for calculation from the Solar Tables of Carlini, and the Lunar Tables of Burckhardt. Also, the results of calculations for the Observatories of Greenwich and Marseilles. By Mr. George Innis.

Observations of the Occultation of *Mercury* on the 25th of April, 1838, made at Aberdeen. By Dr. Cruickshank and Mr. Innes, in a letter to the President.

Extract of a letter from Professor Encke, of Berlin, to Mr. Stratford, transmitting the Ephemeris of Encke's Comet for 1838–9, in manuscript, and requesting Mr. Stratford to communicate some places from it to the Astronomers of Greenwich, Cambridge, and Edinburgh.

Observations of the Moon and Moon-culminating Stars, made in the Observatory S. Fernando, in the year 1837. By M. Cerquero.

Moon-culminating Stars observed at Greenwich, Cambridge, and Edinburgh, in the months of January, February, March, April, and May, 1838.

Nov. 9. Astronomical Observations made at the Imperial Observatory at Wilna, in the year 1835. By M. Slavinski.

A letter from Professor Bessel to Sir John Herschel, Bart., dated Konigsberg, October 23, 1838, on the parallax of 61 *Cygni*.

Dec. 14. Extract of a letter from Professor Bessel to Sir John Herschel, Bart., dated Konigsberg, November 4, 1838, containing further remarks on the parallax of 61 *Cygni*, &c.

Errors of the Heliocentric Longitude and Ecliptic Polar Distance of the Planet *Venus*, computed from the Tabular Errors of R.A. and N.P.D. given in the Cambridge observations of 1836. By the Rev. R. Main.

A Catalogue of 726 Stars, reduced to the year 1830, and deduced from the observations made at Cambridge in the years 1828–1835. By G. B. Airy, Esq. Ast. Roy.

Extract of a letter from Mr. Henderson to Mr. Baily, relative to the late Annular Eclipse of the Sun on May 15, 1836.

1839.

Jan 11. On the Obliquity of the Ecliptic. By the Rev. Dr. Pearson.

On the Parallax of α *Centauri*. By Professor Henderson.

*** The President, at the close of the meeting, read extracts from various printed accounts which had been received from America, relative to the remarkable phenomenon observed during the annular eclipse of the Sun, in September last.

List of Public Institutions, and of Persons, who have contributed to the Society's Library, &c. since the last Anniversary.

Lords Commissioners of the Admiralty.
American Philosophical Society.
Society of Arts.
Royal Asiatic Society of London.
Asiatic Society of Bengal.
Editor of the Athenæum Journal.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences of Brussels.
Cambridge Philosophical Society.
Institution of Civil Engineers.
L'Académie Royale des Sciences de l'Institut de France.
Le Bureau des Longitudes de France.
Royal Geographical Society.
Geological Society of London.
Society of Geneva.
Royal Irish Academy.
Italian Society.
Linnean Society of London.
Royal Academy of Sciences of Lisbon.

Royal Society of London.
Numismatic Society.
Imperial Academy of Sciences of St. Petersburg.
Royal Prussian Academy.
Royal College of Physicians.
Zoological Society of London.

R. C. Agnew, Esq.
G. B. Airy, Esq. Ast. Roy.
Professor Amante.
Professor Bessel.
Dr. Busch.
A. De Morgan, Esq.
Professor Encke.
Lieut. Fergola.
T. Foster, Esq.
Professor Hansen.
Professor Hassler.
J. Herapath, Esq.
Major T. B. Jervis.
Dr. Kaiser.
Professor Kupffer.
Richard Laming, Esq.
Professor Littrow.
Professor Quetelet.
Professor Rigaud.
R. W. Rothman, Esq.
Professor Schumacher.
Professor Slavinski.
Lieut. Stratford.
J. J. Sylvester, Esq.
R. Taylor, Esq.
Lieut. J. R. Wellsted.
T. G. Western, Esq.
J. W. Woolgar, Esq.

The President (Mr. Baily), then delivered the following Address, on presenting the Gold Medal to the Hon. J. Wrottesley.

It now remains only for me to carry into effect the decision of the Council, by presenting the Medal to Mr. Wrottesley for the services that he has rendered to astronomy, as alluded to in the Report which has just been read. But, previously to my so doing, it may be proper to say a few words on the nature of the subject now before us.

It is well known to all those whom I am now addressing, that the principal foundation of the science of astronomy is an accurate catalogue of the positions of the fixed stars; and the formation of such a catalogue has, therefore, always been a prominent feature in the very earliest works on that subject. But, notwithstanding the importance of this branch of astronomical information, it was not till within the last 150 years (on the publication of Hevelius's *Prodromus Astronomiæ* in 1690), that we could be considered as possessing a catalogue at all approaching to any requisite degree of extent or accuracy; and even this is, at the present day, laid aside as almost useless.

The earliest catalogue on record is that which is to be found in Ptolemy's *Almagest*, and which is supposed to have been constructed by Hipparchus, a century at least before the Christian era. Imperfect and incorrect as it is, it was the only one in use, over the whole civilised world, for upwards of 1500 years. And, although the stars therein contained were reobserved by Ulugh Beigh in the 15th century, and afterwards by Tycho Brahé, yet they added but little to the information contained in the original. Hevelius, indeed (as

already related), added both to its extent and accuracy; yet these improvements, important as they then seemed, did not keep pace with the great discoveries in physical astronomy that, about this time, were made by our illustrious and immortal countryman. In fact, it was not till the telescope was adopted in our observatories, that we could expect to arrive at any thing more than an approximate position of the stars.

The catalogue of Flamsteed is the first approach to an accurate and systematic catalogue; a work of great labour and merit, considering the disadvantages and difficulties under which it was obtained. And, although it must now yield to more recent ones in point of accuracy, yet, when its obvious errors are corrected, it will always be appealed to as the most faithful and correct record of the state of the heavens at a distant epoch. It is, indeed, principally in this point of view that it is now useful to the astronomer, as affording evidence of supposed motion or stability in some of the fixed stars, and for clearing up other doubts respecting their relative positions. Its importance, moreover, has been increased by the accidental circumstance of its being universally adopted as the best mode of arrangement and classification of the fixed stars; since we find that most astronomers have tacitly assented to quote the stars by the numbers and characters assigned to them by Flamsteed; a mode of reference, however, which may be materially improved at the present day, now that we are better acquainted with the number, position, and magnitude, of those bodies.

Flamsteed's Catalogue continued in general use, from the time of its publication in 1725, till the appearance of Mayer's Catalogue in 1775. This latter work, however, only partially superseded its immediate predecessor, since it contained only (or with very few exceptions), those stars which are situate in and near the zodiac; but the observations being made with better instruments, and by a very careful and skilful observer, were, as far as they extended, entitled to greater credit. In the meantime, the incomparable Bradley had completed a course of observations at the Royal Observatory at Greenwich, which will render his name imperishable as long as the science of astronomy exists. These invaluable observations, however, remained a *sealed book* for upwards of half a century, unavailable, without great labour and trouble, to any practical purposes, till they were eventually reduced to their present form by M. Bessel, who, in 1818, published his *Astronomiæ Fundamenta*. This work contains a catalogue of 3222 stars deduced from Bradley's observations, which has been of the most extensive use and benefit to astronomers; accompanied by a description of the instruments employed in the observations, and a detail of the methods adopted in the reduction. It is impossible to speak too much in praise of this admirable work, which is now in the hands of every astronomer, and therefore needs no further comment in this place.—I shall here omit any allusion to the important labours of Lacaille, as they refer principally to the other hemisphere.

A few years prior to the appearance of the *Fundamenta Astro-*

nomiæ (for I pass over the intermediate catalogue of Baron Zach, which, like Mayer's, was confined to the zodiacal stars), M. Piazzi published his *enlarged* catalogue of 7646 stars, situate in every part of the heavens visible above his horizon at Palermo: a work which, notwithstanding some errors that it contains (and which, indeed, are pardonable in a production of such an extent, and printed under such disadvantages), is of inestimable value, not only as giving us the positions of many stars, even of considerable magnitude, that had never been observed by any preceding astronomer, but also as affording the means of deducing the proper motions of such as were contained in former catalogues; thus enabling us to bring down the positions of any such stars, with the greatest accuracy, to any given epoch.

Taking advantage of the circumstance, here last mentioned, and bearing in mind the scarcity of the catalogues above noticed, and the difficulty of obtaining them, this Society, in the year 1825, resolved to form a new and uniform catalogue, reduced to the year 1830, of the principal stars, in each of those works; deducing their positions, as far as was practicable, from an union of the two catalogues; and, where that was unattainable, bringing up the positions from Bradley or Piazzi alone, according to the circumstances of the case. This catalogue is contained in the second volume of the *Memoirs* of this Society, and is the first attempt to compose a work of a similar kind, where every element is given, on the same page, for the complete reduction of the star to any given epoch that may be required for occasional purposes. Its utility has been found very great to the practical astronomer.

When this compiled catalogue was published, it was considered desirable that it should be reobserved; not only in order to ascertain its accuracy and precision, but also to place on record the elements for amending and enlarging it at any future period. Several of the stars also, especially those of the sixth and seventh magnitude, had been observed by only *one* observer; so that their reduction to the year 1830, might thus be discordant with their true place, on account of some proper motion not yet ascertained, or from some error in the original observations and reductions: and astronomers were invited to enter on this investigation. Much has been done, in this respect, at the Observatories of Greenwich, Cambridge, and Edinburgh: still, in a work of such extent and magnitude, the cordial co-operation of all is of great advantage, not only as adding to the mass of information, but also as affording occasional checks to errors and discordancies which will sometimes unaccountably intrude themselves, notwithstanding all the care that may be taken to avoid them. And here it is that the private astronomer may render such inestimable benefit to the science. For although he may not be able (nor is it much worth his while to attempt) to compete with the public observatories in fixing the fundamental points of astronomy, he may yet be of the most essential service in some of its collateral branches.

This now leads me to the more immediate object of the present

address. For, when Mr. Wrottesley fitted up his observatory at Blackheath, instead of considering it as a mere toy, to amuse the curious, and astonish the unlearned, he resolved to devote it to some useful and beneficial purpose: and I cannot better relate what I am here desirous to express, than to quote his own words on the occasion. "Being provided," says he, "with such means of "making astronomical observations, I determined to fix upon some "definite object, and steadily pursue that alone. The Astronomical "Society had then published their catalogue of 2881 stars, and "directed the attention of astronomers to the importance of reobserv-"ing them. I resolved, therefore, to select certain stars from that "collection, and determine their right ascensions; observing, if "possible, each of the stars so selected at least ten times. When I "made my selection, I believed that the astronomer royal had "already undertaken the charge of observing the larger stars of the "catalogue in question, up to the sixth magnitude; and I there-"fore formed mine of all the remaining stars; that is, of all the "stars of the sixth, and from that to the seventh magnitude, "inclusive; omitting some stars of the sixth magnitude, which were "contained in Mr. Pond's catalogue."

This was the laudable object that Mr. Wrottesley had in view, and the plan which he had laid down for his guidance; and when we consider the increasing value that is now attached to a knowledge of the accurate position of the smaller stars, in consequence of the discovery of the new planets, and certain periodical comets, the labours of Mr. Wrottesley assume a higher rank and importance than might at first be supposed. That he has executed his task in an efficient manner, any person may satisfy himself that will take the pains to compare his results with those deduced from observations made at the public observatories. This comparison, in fact, has been made in the fullest manner, and with the most scrupulous minuteness, as I shall presently have occasion more specially to mention. In the meantime, I would remark, that the catalogue is the result of upwards of twelve thousand observations; so that, on an average, each star may be considered as having been observed more than nine times. In making the observations, and in their reductions, he was materially assisted by Mr. Hartnup, now the assistant-secretary of this Society; of whose integrity he speaks in the highest terms. The elements used in the reductions, and the methods adopted in detecting and eliminating the errors of the instruments, are minutely detailed in the preface to the catalogue, and need not be further alluded to in this place.

Now, when we consider that, during a great part of the period here alluded to, Mr. Wrottesley was engaged in public duties which occupied a considerable portion of his time and attention, we are the more indebted to him for the boon which he has thus rendered to astronomy: more especially as observations, such as those which he has chosen for himself, are precisely those of which the Society were most in need. For (and I will here take the opportunity of mentioning a circumstance not sufficiently known), so useful to

astronomy has been considered the compiled catalogue of this Society above alluded to, that at the meeting of the British Association in Liverpool, in 1837, a proposal was brought forward and acceded to by that body, to appropriate the sum of £500 from its funds towards its recomputation and extension; and when the whole is finished, it will be the most complete and useful production of the kind. It will probably contain upwards of 8000 stars in all parts of the heavens, reduced to the year 1850, with the same arrangement of constants as in the present Catalogue of the Society, together with the addition of the secular variations of the precession, and the proper motions, when attainable.

In so extensive an undertaking, it was requisite to refer to every authentic source of information. Doubtful positions were to be verified, imperfect results were to be completed, lacunæ filled up by new observations, and sometimes even the very existence of a star to be ascertained. In this inquiry, every assistance has been cheerfully furnished by the public observatories, especially that at Greenwich, to which the most frequent appeal has been made. Still the co-operations of the private observer come in very opportunely, at the present moment, when the hand of every labourer is required. Mr. Wrottesley's exertions have, consequently, been pressed into the service, and, I am happy to say, with good effect; for when the comparisons have been made with the positions of the same stars obtained at the public observatories (and *every* star in Mr. Wrottesley's catalogue has undergone that investigation), the result has shewn that the catalogue of Mr. Wrottesley is of first-rate importance, and entitled to implicit confidence.

It is with a knowledge and full impression of the importance of these facts, that the Council has been desirous of expressing its opinion of the value of Mr. Wrottesley's exertions in this branch of science, by awarding him their gold medal; not only as a memorial of their sense of his merits, but as an encouragement to others to come forward, with the same public spirit, to assist in the great objects for the promotion of which we are united.

(*The President then, addressing Mr. Wrottesley, continued as follows:—*)

Mr. Wrottesley — In the name of the Royal Astronomical Society, I present you this medal, as a token of the high sense which they entertain of the valuable assistance which you have rendered to astronomy, in the formation of the Catalogue which will henceforth bear your name. In performing this pleasing duty, I can only add my best wishes that you may long enjoy the honourable distinction which this gift is intended to convey: accompanied with a hope that it may stimulate you to still further exertions in the cause of a favourite science, being well assured that they will bring their own reward, in the peaceful contemplation of the benefits conferred in their study and pursuit.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: Sir J. F. W. Herschel, Bart. K.H. M.A. V.P.R.S. —*Vice Presidents:* Francis Baily, Esq. V.P.R.S.; Augustus De Morgan, Esq.; Davies Gilbert, Esq. F.R.S.; Hon. John Wrottesley, M.A.—*Treasurer:* John Lee, Esq. LL.D. F.R.S.—*Secretaries:* Thomas Galloway, Esq. M.A. F.R.S.; Lieut. Henry Raper, R.N.—*Foreign Secretary:* Captain W. H. Smyth, R.N. F.R.S. & A.S.— *Council:* George Biddell Airy, Esq. M.A. *Astronomer Royal;* George Bishop, Esq.; Lieut. W. T. Denison, R.E.; Rev. George Fisher, M.A. F.R.S.; Major Thomas Best Jervis, F.R.S.; Rev. Robert Main, M.A.; Richard W. Rothman, Esq. M.A.; Edward Riddle, Esq.; William Simms, Esq.; Lieut. W. S. Stratford, R.N. F.R.S.

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *March* 8, 1839. No. 21.

The following communications were read :—

I. Observed Transits of the Moon, and Moon-culminating Stars, over the Meridian of Edinburgh Observatory, from June 1 to December 31, 1838. By Professor Henderson.

II. Lunar Occultations of Planets and Fixed Stars, and Eclipses of Jupiter's Satellites, observed at Edinburgh Observatory in 1838. By Professor Henderson.

III. Moon-culminating Stars observed at the Cambridge Observatory in the Months of November and December, 1838. By Professor Challis.

IV. Occultations observed at Dulwich and Ashurst, from July 31 to December 27, 1838. By Robert Snow, Esq.

V. On the Method of determining the Longitude by Moon-culminating Stars. By Mr. Epps, late Assistant-Secretary to the Society.

The author remarks, that the advantages of moon-culminating observations, for the purpose of determining the difference of longitudes, particularly in the case of distant meridians, are now universally admitted, every other method being found subject not only to greater trouble and difficulty, but to errors far exceeding the limits of those to which the results of moon-culminating observations are liable. But although this method, the merit of introducing which belongs chiefly to Mr. Baily, is justly regarded as the best known, yet the result of a single observation, or of a few observations, is liable to a considerable amount of error, even when the observations are made by the best instruments and the ablest observers; and the object of the present communication is to shew the extent to which the error may be expected to reach.

The moon's proper motion, on which the method entirely depends, is such, that an error in the observation is necessarily greatly augmented in the resulting longitude; in fact, the error in longitude will always be from 21 times to 35 times the amount of the error of observation. If, then, it be assumed that $0^{s}{\cdot}2$ of time is the probable error of an observed transit, and that the observed interval between the transits of the moon and star is liable to an error of $0^{s}{\cdot}4$, the resulting difference of longitude will be in error from 8 to 14 seconds of time. This is the probable effect, from

observations made at one station only; and as it is quite possible that the effect may be doubled by an equal amount of error, in a contrary direction, at the other station, the author thinks he will be within limits in assuming 10 or 12 seconds of time as the probable error of longitude, resulting from the comparison of a corresponding interval observed at two stations, admitting that the best means of making the observation exist at each.

In order to shew that the probable error is not overrated, by assuming it to amount to 10 or 12 seconds, the author refers to two lists of results given by Professor Henderson, in the introduction to the first volume of the *Edinburgh Astronomical Observations*. One of these lists contains the computed differences of longitude between Edinburgh and Greenwich, and the other between Edinburgh and Cambridge; each contains 36 combined results, some of which differ by a much greater quantity than the probable error above assumed. The mean of all the results, as deduced by Professor Henderson, likewise shews the degree of approximation which the method may be considered as capable of giving, in the most favourable cases. Each of the lists referred to consists of a *mean* of the differences of the observed intervals at the two places on the several days stated, so that each series of results contains about 100 corresponding intervals; and, consequently, the final difference of longitude, deduced from each list, is the mean result of 36 mean quantities, deduced from about 100 corresponding intervals, observed at both stations. In fact, there are 136 corresponding transits made at Edinburgh and Greenwich, and 133 at Edinburgh and Cambridge; and from all these the difference of longitude between Greenwich and Cambridge comes out $= 13^{m}\ 5^{s}{\cdot}5 - 12^{m}\ 44^{s}{\cdot}4$, or $21^{s}{\cdot}1$. But the difference of the longitude of the two places, ascertained by other means, and, doubtless, with scrupulous exactitude, is $23^{s}{\cdot}54$; so that the final result of the moon-culminating observations is wide of the truth by more than 2 seconds of time.

The author concludes by observing, that the results here referred to, instead of tending to depreciate the method, may be considered, on the contrary, as affording proof of its excellence; for, in respect of distant meridians, an error of 2 seconds is undeserving of notice. It is however evident, that, considering the inferiority of instruments, and other sources of error incident to a temporary station, a tolerable degree of accuracy can only be expected from an extensive series of corresponding observations.

VI. On the Position of Lacaille's Stations at the Cape of Good Hope. By Thomas Maclear, Esq. M.A. F.R.S., Astronomer Royal at the Cape. Communicated by Captain Beaufort, R.N. F.R.S. &c.

This paper was in part read.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *April* 12, 1839. No. 22.

The reading of Mr. Maclear's paper, on the Position of Lacaille's Stations at the Cape, was resumed, and concluded.

The astronomical celebrity of the Abbé de Lacaille's visit to the Cape of Good Hope in the last century, together with the remarkable results deduced from his arc of the meridian, naturally prompted Mr. Maclear to become acquainted with his stations, and to connect the southern, which was his observatory, with the present Royal Observatory. In undertaking this task, he soon found that the lapse of eighty-five years had obliterated all local evidence of the French astronomer's operations; and the fact that he was there at all, was only kept alive by the inquiries of Captain Everest in 1821. Having carefully perused the various Memoirs of Lacaille relative to his Cape operations, as well as his printed Journal, Mr. Maclear applied to his Excellency Sir Benjamin D'Urban, the governor of the colony, for leave to inspect the official documents. This was readily granted; but although several letters and notices interesting to the astronomer were brought to light, nothing was discovered tending to promote the object he had immediately in view, which was to identify the spot on which Lacaille's Observatory had stood.

Lacaille states, that he resided in the house of a person of the name of Bestbier, on whose premises the Observatory was built. Accordingly, Mr. Maclear undertook to trace the residences and property of all persons of the name of Bestbier; and he ascertained, on searching the records of the Transfer Office, that only one person of that name held property in Cape Town in the year 1751, and he answered to the description given by Lacaille. By tracing down the successive transfers of this property to the present time, Mr. Maclear was led to the house in Strand Street, now occupied by Mrs. De Witt; the house referred to by Captain Everest. This lady permitted him to inspect her title-deeds and diagram, and they were found to agree exactly with the records in the Transfer Office. The position and form of the premises also corresponded with several remarks made by Lacaille in describing his operations.

Having thus obtained undeniable proof of the identity of Bestbier's house with that now occupied by Mrs. De Witt, the search for the position of the Observatory was brought within narrow limits, for Lacaille states that it was in the court of the house where he lived. The author accordingly proceeded to take measures for connecting the house with the Royal Observatory by trian-

gulation, resolving to spare no pains in the execution of this part of his operations, inasmuch as he entertained a hope that he should thereby be able to ascertain whether Lacaille's plumb-line had been affected by the attraction of Table Mountain.

In the meantime, Mr. Maclear had communicated his views and proceedings to Captain Beaufort, and Mr. Airy, the Astronomer Royal, the latter of whom immediately wrote to the Secretary of the Admiralty, requesting their Lordships' permission to send out Bradley's zenith-sector, in order that Mr. Maclear might be enabled to verify at once the amplitude of Lacaille's arc. This was precisely what he himself had wished to accomplish, provided he could identify the northern station.

The northern station, at Klyp-Fonteyn, cannot be so readily traced as that in Cape-Town. On visiting the place, accompanied by Lieut. Williams of the Royal Engineers, Mr. Maclear found that a close investigation into the history of the proprietors, in connexion with the buildings and ruins, would be necessary; for, on looking at the old foundation described by Captain Everest, as the platform of the granary in which Lacaille had observed, and comparing its dimensions with Lacaille's statement, and its position and distance from the old house, so unlike the usual arrangements of the Dutch farmers, he perceived strong reasons for doubting its identity with the granary of Lacaille.

The platform alluded to by Captain Everest consists of a foundation-wall 63 feet long, by 24 in breadth; a considerable portion of it, on the west side, is two feet above the ground, and it is situated at the distance of 630 feet from the old house. But Lacaille states that the place in which himself, Mr. Bestbier, and his assistant, slept, was a part of the granary, 6 feet long, and 7 feet wide, separated from that in which the sector was by a curtain, which formed a sort of partition; and beyond was a little place for the slaves. Now if this platform was the site of the granary inhabited by Lacaille, it is very difficult to account for these confined dimensions. It can hardly be supposed that the place was filled with grain, for Lacaille's visit took place about a month before the time of harvest. Besides, it was distinctly asserted by Ferrit Cotsee, an aged inhabitant of the place, that the foundation in question was the ruins of his father's dwelling-house. It therefore became necessary, in order to arrive at the facts, to inquire into the evidence on which Captain Everest had fixed on the spot; to investigate the truth of Ferrit Cotsee's statement; and, lastly, to examine the place, by turning up the soil. As none of these plans could be carried into execution immediately, Mr. Maclear, in the meantime, returned to the Observatory.

Captain Everest's statement, which is given in the first volume of the *Memoirs*, p. 261, is as follows: "In reference to this matter, it may not be amiss to mention, that the daughter of the quondam proprietor of Klyp-Fonteyn, now an aged lady, named Letchie Schalkevyk, is still in existence, and not only gives a narrative perfectly agreeing, but has pointed out the very platform on which

the granary once stood." On applying to Mr. Hertzog, the assistant surveyor-general at the Cape, who had accompanied Captain Everest to Klyp-Fonteyn, for information respecting the particulars of their journey, Mr. Maclear ascertained that the aforesaid lady, who appears to have resided at some distance, was not brought to the place herself, but sent her son, who pointed out the site, according to directions from his mother. The son himself could have no local knowledge, for no one of the name of Schalkevyk had resided at Klyp-Fonteyn, within the memory of the oldest inhabitant living in 1838; it may therefore be conceived that he would be likely to point to the only ruin visible, as the site which his mother had described to him from memory. This evidence is obviously not to be put in comparison with that of Ferrit Cotsee, who had constantly resided at the place during sixty-eight years, and remembered having lived in the house over the foundation in question.

When preparing for his second journey to Klyp-Fonteyn, with Bradley's sector, Mr. Maclear applied to his Excellency General Napier, and obtained a corporal of sappers and private from the artillery corps, under his former companion, Lieut. Williams. Arrived at Klyp-Fonteyn, he found the widow of the brother of Ferrit Cotsee, who had been married at the age of seventeen, forty-eight years ago, and had known Klyp-Fonteyn ever since. At her marriage, she lived with her mother-in-law, who died at the age of eighty, and has been dead thirteen years. She had often heard her mother-in-law speak of the French astronomer's visit. When made acquainted with Mr. Maclear's object, she took him to a spot where was a ruin in her early years, stated by her mother-in-law to be the ruins of the house of Oker Schalkevyk, the father of the lady referred to by Captain Everest. Nothing in the shape of a ruin was to be seen; but the sappers were set to work, and, in a couple of days, exposed the foundations of a building, 54 feet long by 12, with three partition walls, and an oven at one end: evidently the site of a dwelling-house, as had been described.

While this operation was going forward, Ferrit Cotsee pointed out another spot, on which, he said, there had formerly been an old ruin. The sappers were again set to work; and, at the depth of 3 feet, encountered a wall, which they traced and cleared, with great labour, in three or four days. This foundation, like the other, is of stone and clay. It is 22 feet by 12, and generally from 2 to 3 feet below the surface. Mr. Maclear attributes the depth to the sliding down of the soil from above, the ground over it being much inclined. Portions of chaff or short straw were found deep in the clay; but little confidence could be placed in this, as affording indications of a granary; for they might have been carried down by ants or mice. Ferrit Cotsee could give no account of the purpose of this building; there had been no roofed building there within his recollection.

There were now three ruins exposed to view, and the question arose, whether any one of these was the granary of Lacaille? The first was asserted by Ferrit Cotsee to be the ruins of his father's

dwelling-house; the second was discovered exactly under the spot which his sister-in-law, from his mother's information, had described as the site of Oker Schalkevyk's house; with regard to the third, there was no direct evidence, but it stood relatively to the others in the position in which granaries usually stand in the country; and it was difficult to conceive any other purpose to which it could have been applied. Its dimensions were too contracted for a dwelling-house, and its masonry too good for carrying the flimsy hut of a hottentot or slave; which, besides, are not oblong, but circular, and never of more substantial materials than mud, except at the missionary institutions. After a minute and careful comparison of all the circumstances, Mr. Maclear came to the conclusion that the platform of Captain Everest was the dwelling-house of Cotsee's father, as the son asserts it to have been; and that the granary was on the foundation he had last exposed. This conclusion was subsequently confirmed by an entry discovered among the colonial records, from which it appeared that the proprietor of the place, in 1752, was Cornelius Cotsee, the grandfather of Ferrit; and that Oker Schalkevyk, supposed by Captain Everest to be the proprietor, was not a proprietor, but a householder, at the will of Cotsee. The meridional distance of the granary from the platform of Captain Everest is 210 feet, or rather more than 2".

The signals at the other two angles of the triangle, namely, Riebeck's Castle and Capoc Berg, were easily recognised. The charcoal remnant of the signal-fire still remains on Riebeck's Castle; and was carefully covered up, by Mr. Maclear, with stones. The top of this rugged mountain, he observes, presents nothing inviting, and the ascent is laborious and difficult: hence the reason of the signal remaining undisturbed; whereby the party were enabled to enjoy the sight of one undeniable mark of the work of Lacaille.

The author next proceeds to describe the operations for connecting Lacaille's southern station with the Royal Observatory. With these he included another position: "One which," he remarks, "must ever excite the feelings and enthusiasm of the admirers of genius, moral worth, and almost unlimited talent—namely, the scene of Sir John Herschel's recent labours." This position being invisible from the Observatory, and also from Cape Town, it became necessary to choose a fourth station; and, accordingly, King's Block-house Battery was fixed upon, which commanded a view of the other stations, and also of the base line.

The site of the base is on the sandy plane to the north of the Observatory. The east end is defined by the centre of the meridian pillar of the transit room; and the west, by a gun, let into a large flag-stone, which was sunk in the ground to the depth of seven feet, and firmly fixed by ramming down the soil. The line is so little elevated above the sea, that a large portion of it is covered by water at spring-tides.

Almost the whole of the apparatus for the measurement of the base was constructed at the Observatory; and although the screws, brass-work, &c., were homely in appearance, they sufficiently

answered the purpose. The twenty-feet measuring-rods, of which three were used, were of well-seasoned white deal, on the model of those employed by General Roy, on Hounslow Heath, excepting in a few minor details. The square brass ferrule covering each extremity, was perforated with a hole three-eighths of an inch in diameter; and a brass screw, one-fourth of an inch in diameter, and nearly two inches long, was passed into the wood through the hole, without touching the ferrule, and ground down perfectly flat. The head of this screw carried the division which defined the length of the rod; and it is evident from the arrangement, that it could only be affected by the expansion of the wood. Eight trestles were constructed on the model of General Roy's, having wood screws and movable teak-wood tables; and the usual equipment of boring-rods and piquets prepared.

A contrivance suggested by Sir John Herschel for measuring the space between the divisions on the contiguous ends of the measuring rods, when laid in line, was adopted, and found to answer well in practice; and which obviated the possibility of any mistake in registering. This consisted in rendering the space a constant quantity, which was effected in the following manner. A couple of crosses being drawn with a fine point on a slip of mica, about 2·3 inches from each other, and the divided surface turned downwards to prevent parallex. The crosses were seen through the transparent mica, like spider lines; and as they were placed, when used, over a fine line on brass, a neat bisection of the cross was easily obtained, with the assistance of a common magnifier.

The twenty-feet measuring-rods were compared with the four-feet brass scale belonging to the Observatory. This scale is by Dollond. It is a thin brass bar, enclosed in a mahogany case, lined with baize; and was brought to England by Sir John Herschel, on his return from the Cape, for the purpose of comparison with the Royal Astronomical Society's standard.* On making the proper reductions for temperature, the standard measuring-rod was found to exceed 20 feet of the scale by about 1-50th of an inch.

The measurement of the base line was commenced on the 17th of June, Sir J. Herschel, Lieut. Williams, and Mr. C. Piazzi Smyth, taking part in the work; but it had only proceeded a short distance, when it was interrupted by an accident. While Mr. Maclear was adjusting the 66th rod, a sudden gust of wind blew it and the next off the trestles, whereby one of them was entirely destroyed, and the other injured. This accident compelled him to postpone the measurement until November, for before a new rod could be got ready the winter had set in, and the floods spoiled the line. The author remarks, that although the contrivances adopted for preventing similar accidents proved effectual, rods of this description are unfitted for a windy country like the Cape, being liable to short vibrations, which no clamping can control.

* The comparison has since been made by Sir John Herschel and Mr. Baily. The mean of sixty-three comparisons gave its length = 47·997083 standard inches of the Society's standard yard.

The measurement was recommenced from the meridian pillar on the 8th of November, and proceeded without further interruption. In the preceding June a pig of lead, weighing 56 lbs., was sunk in the ground, at the end of the 27th rod, and a cross on its surface adjusted to the plummet. On reaching the 27th rod, in the second operation, the lead was uncovered, and the plummet fell short of the cross upon it by nearly half an inch. As the place was frequently covered with water during the winter, it is probable the lead had shifted its position.

The measurement was completed in four days. On each day the rods were compared with the standard, and the proper reductions made to obtain the length in terms of the standard rod, which also was corrected to the temperature of 70°, this being the temperature engraved on the brass scale. The final results gave the distance between the centre of the meridian pillar and the centre of the gun = 2919·364 feet.

The angles were measured in the latter part of 1836, while the apparatus for measuring the base was in progress. The instrument employed for the purpose was the repeating instrument by Dollond, described in the first volume of the *Memoirs*. The signals on the Block-house Battery, the Observatory, and Base-line stations, were tripods, surmounted by hoops, and covered with white cloth. At Mrs. De Witt's house the signal was a circular disc, painted on the east chimney.

On computing the triangles, and making the proper reductions, the distance between Mrs. De Witt's chimney and the transit instrument was found to be 17,096 feet, and its distance from the perpendicular to the meridian 4579·5 feet. Assuming the compression $= \frac{1}{298}$, 1″ in latitude 33° 56′ = 101·739 feet, therefore the difference in latitude = 45″·01; and the latitude of the Observatory being 33° 56′ 3″·25, that of the chimney is 33° 55′ 18″·24. The chimney is south-west of Lacaille's Observatory, about 120 feet, or 115 on the meridian, therefore the latitude of his Observatory was 33° 55′ 17″·11. Lacaille assumed it to be 33° 55′ 15″.

The remainder of the paper is devoted to a description of the heights, distances, and bearings of the mountains about Klyp-Fonteyn, which were ascertained with considerable exactness, and a map constructed, to convey an idea of their form, and probable iufluence on the zenith sector. The sector, and the repeating circle, for taking the angles, were placed on the corn-floor, on Jacobus Cotsee's foundation, before the foundation of the granary was discovered; and, as the place was comparatively convenient, and the influence of the surrounding masses nearly the same on both, it was considered unnecessary to remove the instruments. The height of the sector above the level of the sea was found, by barometrical comparisons, to be nearly 400 feet.

J. Moyes, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *May* 10, 1839. No. 23.

The following communications were read :—

I. Occultations of the Pleiades by the Moon, observed at Ashurst, March 19, 1839. By Robert Snow, Esq.

II. Occultations of the Pleiades, observed at the Royal Naval Asylum, Greenwich, March 19, 1839. By the Rev. George Fisher, A.M.

III. On the suspected Variability of the Star α *Cassiopeiæ*. (Extract of a Letter from the President, Sir J. F. W. Herschel, to Mr. Baily, dated Slough, April 28, 1839.)

"My attention was attracted to the star α *Cassiopeiæ* on the 18th of October last, by noticing that it was on that night very decidedly less than the star γ of the same constellation, and that in fact γ was then the chief star. On referring to my father's catalogues of comparative brightness however, α is found placed above γ. It was, therefore, evident that a change had taken place. On two or three subsequent nights the fact was verified, and other eyes than my own were called in to establish its reality. A considerable succession of cloudy nights intervened before other comparisons were procured; and when a favourable opportunity again occurred, viz. on the beautifully clear night of Nov. 12, the order of magnitude was found restored to that assigned by my father, viz. α, γ, β. On the 27th and 28th of Dec. and the 22d of Jan., 1839, the observations are positive to this effect.

"In this state it remained till I began to suspect some illusion in the October observations; but, on the 24th inst., being a remarkably clear and beautiful night, γ was again the principal star, and α was inferior not only to that, but to β. And so I observed it to be only about an hour ago. It is true the constellation is now low, but the stars are so near together that the difference of altitude can by no means account for the very marked difference in brightness, though its tendency is certainly in that direction. I should not think of making this observation the subject of a distinct announcement, were it not that my own attention is necessarily so much distracted from these objects as to make me desirous that some other observer may take up the subject, and verify or disprove the variability of the star in question; and, if verified, assign the period of change.

"I may take this opportunity to point out δ *Orionis* as a star of whose variability I am almost certain."

IV. Extract of a letter from M. Gautier, Director of the Observatory at Geneva, to the President, accompanied with the Observations of Encke's Comet, made at Geneva in the months of October and November 1838.

The observations were made with an equatorial of Gambey, having a telescope by Cauchoix, of 4 inches aperture, and 42 feet in focal length; and the circles, which are 30 inches in diameter, giving by means of verniers the arcs to every three seconds. The instrument required no adjustment during the whole time of the observations, and the comparison of observations of the best known stars shewed that it possessed great stability and accuracy of division. The method of observing which was practised does not, however, admit of a degree of precision comparable to that which may be attained when it is possible to employ a wire micrometer illuminated upon a dark field. On account of the extreme feebleness of the comet's light, at least during a part of the observations, a micrometer of this kind could not be used, and in lieu of it a simple cross was employed, formed of small thin plates of silver, about 1′ of a degree in breadth, visible without illumination with a magnifying power of about 25 times. All the observations of the comet and stars were made by referring each body to this cross, noticing the instant of observation by a sidereal clock, and reading the position of the telescope by the verniers of the two circles. The thickness of the plates of the cross necessarily impairs the precision of the determination, especially for the declinations; but with a little practice and care, values were obtained with sufficient exactness, and of about the same precision as those of comets generally are. The right ascensions were found by taking the differences of the times of observation and the horary angle of the equatorial circle, and the declinations by taking the complement of the polar distances indicated by the declination circle. The advantages of this method of proceeding are, that the observations can be multiplied in a short space of time, and that the best known stars can be taken for a comparison.

Of the three tables which accompany this communication, the first contains all the observations of the comet, in number 189, corrected only for the error of the clock, which, however, is scarcely of any consequence, as it effects equally the observations of the comet and of the stars used for comparison; the second contains the observations of the stars, and their comparisons with the mean positions deduced from the catalogue; and the third contains the reduction of the observations of the comet and their comparison with the ephemeris of M. Bremicker.

In comparing the results of the observations of the comet at Berlin and Geneva, there is a sufficient agreement on the whole, although the positions observed at Geneva are in general a very little advanced in the sense of the comet's motion in respect of

those observed at Berlin. M. Gautier thinks the results plainly indicate that a diminution is required to be made in the value of the mass of *Mercury* adopted by M. Bremicker in calculating the effects of the perturbations; and this is also the opinion of Encke: but M. Valz, who observed the comet at Marseilles, from the 23d of November to the 16th of December, is of a contrary opinion. The observations of the latter, it may be remarked, confirm entirely a circumstance remarked on a former occasion by himself, and also by Struve, namely, the contraction of the nebulosity in proportion as the comet approaches to the sun.

V. On Ptolemy's Catalogue of Stars. By Francis Baily, Esq., Vice-President of the Society.

The catalogue of stars, which goes under Ptolemy's name, will always be interesting to the astronomer, as containing the first record of the state of the heavens. The precise epoch for which it was formed, is not clearly ascertained. Ptolemy himself says, that it is reduced to the first year of the reign of Antoninus, which corresponds to the year A.D. 138; but there is some mistake, or confusion, on this point, which has led many persons to believe that Ptolemy himself did not actually make the observations from which the catalogue was deduced, but merely reduced, or brought up, a more ancient catalogue of Hipparchus, by means of an erroneous precession, to his own time. It is very evident that the longitudes of all the stars in Ptolemy's catalogue are above 1° too great; but, from what cause this has arisen, it is not my object here to inquire. The point to which, in the present view of the case, I am more desirous of directing attention is, how far the existing editions of that work may be considered as faithful transcripts of the catalogue as it issued from Ptolemy's hands.

This important question can only be decided by a careful examination of various manuscripts, and by comparing the discordant readings with the actual positions of the stars, as determined from modern observations. Unfortunately the public have not been in possession of much varied information on this head; the manuscripts hitherto employed having, until lately, been confined to three only; to which two others have been recently added, as I shall presently explain. But all these are, in many cases, so grossly discordant that it would appear, at first sight, almost a hopeless task to reconcile the different readings that present themselves, or to account for the introduction of so many discrepancies in so small a portion of Ptolemy's great astronomical treatise. The five sources of information here alluded to, are,

1st. The Latin translation published by Liechtenstein at Venice, in 1515; the name of the translator is not known, nor is it stated whence the manuscript was obtained. The translation, however, bears internal evidence of having been made from an Arabic manuscript, and throws great light (as I shall presently shew) on the subsequent translations and editions from the Greek manuscripts.

2d. The Latin translation made by Trapezuntius (George of

Trebizond) and published by Gauricus, also at Venice, in 1528. This translation, from which most of the subsequent editions seem to be copied, is said to have been made from a copy of a Greek manuscript which the Abbé Laurentius Bartolinus caused to be made from one in the Vatican library.

3d. The Greek edition published at Basil, in 1538, by Grynæus. It is said that he made use of a manuscript that belonged to Regiomontanus, who had it from the Cardinal Bessarion, and deposited it in the library of Nuremberg. This, however, has been since doubted; and it is not quite certain where the manuscript above mentioned is now to be found. This is the more to be regretted, as it is the only Greek edition extant, except the recent one which I am now about to mention. It is evidently not the manuscript from which either of the preceding translations was made. The work is dedicated to our Henry VIII.

4th. The two other sources are comprised in the Greek edition (accompanied by a French translation), published at Paris, in 1813, by M. Halma. In editing that part of the volume which contains the catalogue of stars, M. Halma availed himself of two additional manuscripts which were in the public library of Paris: one of these he calls the Paris manuscript, which is made the ground-work of his publication, and the other the Florence manuscript. But some other sources must have been appealed to, as he occasionally inserts readings which are not to be found in either of these manuscripts, or in the Basil edition above mentioned.

These several sources of information are all that the public press affords us in our inquiries relative to this interesting subject; but they are lamentably deficient for the purpose. And, as it is probable that the public are not fully aware either of the amount or the frequency of discordance that exists in this matter, I trust I shall not encroach too much on the patience of the meeting by stating, as briefly as possible, the result of my own investigations and researches. Having carefully compared the position of every star, as given in each of the several copies above alluded to, I have found that out of 1028 stars, of which the catalogue consists, there are about 780 (or more than three-fourths) of them that are discordant, either in longitude or latitude; and this, not merely in 10, 20, or 30 minutes, which is no uncommon difference, but sometimes to an amount involving whole degrees. That these errors have arisen mostly from the carelessness of the copyists, and that they may be partly corrected, in some cases, by a reference to the true position of the star, I am ready to admit; but still there are numerous cases where this tentative method will not avail us, and where we are, after all, left in doubt as to the identity of the star, or the true reading of the original numbers.

One source of error I have discovered to be very common, and in the correction of which I have found the translation from the Arabic to be of essential service. It is this. The Greek notation for minutes being denoted by some fractional part of the degree, and such fraction being expressed by a dash annexed to the letter

which in common notation denotes the integer, frequent mistakes occur from the copyist having affixed the dash when it ought not to be inserted, and from omitting it when it ought to have been annexed. Thus, 23° is correctly denoted by κγ; and 20° 20′ (or $20^\circ\frac{1}{3}$) is correctly denoted by κγ′: again 34° is correctly denoted by λδ; and 30° 15′ (or $30^\circ\frac{1}{4}$) is correctly denoted by λδ′. Now it is very readily seen that mistakes of great moment may be made by the omission or misplacing of the dash to the second letter: and here it is that the translation from the Arabic frequently comes in to our aid; for, as their notation was not liable to the same sort of confusion, we are oftentimes led to the true reading by a reference to their copies. I could point out numerous instances of this kind: one, however, will be quite sufficient to illustrate my meaning. The star in *Capricornus*, the fourteenth of that constellation in Ptolemy's catalogue, whose longitude in all the abovementioned editions, except the first, is said to be 26°, is in the translation from the Arabic stated to be in longitude $20\frac{1}{6}^\circ$, or 20° 10′; which is in fact its more correct value. And it is clear that the erroneous translation from the Greek has arisen from not affixing the dash to the second letter in the expression κϛ, which ought to be κϛ′. Still there remain numerous other cases where this mode of explication will not avail, and where it would be desirable that other sources of information should, if possible, be thrown open to us: and it is on this point, as I have before alluded, that I am more especially induced to make the present appeal.

Now, it appears that there are several works in this country that might assist us very materially in the elucidation of this subject. In the Bodleian library at Oxford there is a Greek manuscript of Ptolemy, which was presented by Selden: and there is also, in the same library, Bernard's copy of the Basil edition of 1538, wherein he has copied out all the longitudes and latitudes of the stars in the catalogue from this same manuscript. In the library of All Souls' College, it is stated, by Fabricius, that there is the manuscript of a Latin translation from the Arabic: and I understand that there are also manuscript Latin translations from the Arabic at New College and at Magdalen College. There is also a manuscript commentary in Persian, belonging to St. John's College. In the library of the Archbishop of Canterbury at Lambeth there is also a Greek manuscript of Ptolemy. In the library of the British Museum there is an Arabic manuscript, of the date 1218; but I have not been able to find there any Greek copy.

It is evident, therefore, that we have in this country several sources of original information, of which we might avail ourselves, to render the catalogue of Ptolemy more perfect than it is: and, lest it might be supposed that this would be an useless labour at the present day, when the state of the heavens is so much better known, I would remark that it is on this very account that more accurate information is required; since we now know that many minute and gradual changes are going on, which were not suspected or thought of in former times, and which are only per-

ceptible after a lapse of many centuries. Thus *Sirius* is described in all the original documents that I have seen, as *ὑπόκιρρος*, *subruffa*, reddish; whereas at the present day, it is remarkable for its freedom from all colour. Now, this is a point on which Ptolemy could not well be mistaken. Again, a star of the fourth magnitude (the seventeenth in the constellation of *Eridanus*) is clearly laid down by Ptolemy, but cannot now be found: and there are some others, of smaller magnitude, that cannot be identified, according to the positions given in the present editions of the catalogue. But whether these bodies have vanished wholly from our sight, or have been erroneously copied from the original observations, can only be satisfactorily explained (if, indeed, they ever can be) by reference to other authorities. It is needless, however, to dwell further on so obvious a principle.

I had taken the liberty of suggesting to the late Professor Rigaud (a name ever dear to the lovers of astronomy, and more especially to those engaged in historical researches in that science) the propriety of requesting the University of Oxford to print the catalogue of Ptolemy, from the Greek manuscript in their possession: a request which, I understand, was favourably received. We are all sensible of the obligations, under which we lie, to the University of Oxford, for its noble and spirited conduct, on former occasions, in publishing the works of some of the best ancient authors on scientific subjects, which otherwise might never have seen the light; and certainly not in so splendid a dress. Witness the works of Euclid, Apollonius, Archimedes, &c.; and in more recent times, a continuance of the same liberal and enlightened course on various occasions, in the publication of works that reflect honour and credit on the University, and from which they can never expect to reap any pecuniary benefit.

Since that application, however, was made, the information relative to the additional manuscripts above mentioned has been obtained; and it may now become a question whether it may not be presumed that a more accurate copy of Ptolemy's catalogue is more likely to be deduced from a careful collation of all the manuscripts within our reach, compared with the several original editions and translations above alluded to, than from the printing and publication of a single Greek manuscript. Should a plan of this kind be attempted, I would propose that a few copies of the Basil edition of 1538 be reprinted (for I fear that the original work is too scarce to be met with in sufficient quantity for this purpose), and distributed amongst those persons who would each undertake to collate such copy with some one or other of the manuscripts in the several archives above mentioned. This would be no great task or labour to those who feel an interest in the cause, and who are zealous in the promotion of science. Copies even might be sent abroad, to some of our foreign members, residing in places where original manuscripts are known to exist; and who might thus add to the common stock of information. But, whichever course may be adopted, I trust there is no difference of opinion as to the propriety

of taking some steps relative to this matter: and I hope that what I have here stated may induce others to join in carrying so desirable an object into execution.

Mr. Baily stated to the meeting that further accounts had been received from America, relative to the annular eclipse of the sun on the 18th of September last. Professors Henry and Alexander observed it at Princetown College, New Jersey. About eighteen seconds before the formation of the ring, the moon's limb became brightly illuminated. An appearance similar to a row of beads was regarded as the formation of the ring: the drops continued for a second or two. Professor Alexander remarks that the luminous arch round the moon's dark limb, and the brush of light, were only partially visible in his 4-feet Fraunhofer, with a yellow screen-glass, having a slight tinge of green: but he saw them distinctly in his 3½-feet Dollond with a red screen-glass, for about four minutes after the rupture of the ring; whence it is inferred that the appearance of the beads of light and the dark lines frequently noticed, may be completely modified by the colour, and consequently the absorbing power of the screen-glass through which they are observed.

It was noticed by most of the observers, that before the formation and after the rupture of the ring, the edge of the moon *off* the sun was distinctly visible, and illuminated for some distance within the moon's surface.

London: — Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. IV. *June* 14, 1839. No. 24.

The following communications were read :—

I. On the Method of determining the Longitude by Moon-culminating Stars. By Mr. Epps, late Assistant Secretary to the Society.

This may be regarded as a continuation of the former paper by the author on the same subject, of which an account has been given in the *Monthly Notice* for March of the present year. Considering the moon-culminating method as the best known method for determining the difference of distant meridians, it is much to be regretted that observations for this purpose, however carefully made, should become useless, if it so happen that *corresponding* observations cannot be found. It would, therefore, be a most important improvement on the method, if, in the absence of corresponding *observed* intervals, *computed* intervals could with safety be adopted to supply their place, even to a tolerable degree of approximation; and the object of the present paper is to shew the amount of error, both in respect of the moon and stars, which, in the present state of the data, may be expected to arise from adopting such a procedure.

For this purpose the author selects the list of observations made at the Observatories of Greenwich and Cambridge (where the errors of observation may be considered as the smallest possible), from January 2d to May 30th, 1838, published in the *Monthly Notice* for June of last year; and gives a Table, shewing the differences of longitude which result from comparing *observed* intervals of the moon and stars, with the intervals computed from the *Nautical Almanac*, using the moon's variation, as given in the Almanac, reduced for the half interval. The results appear generally so wide of the truth, that any practical application of the method must be considered as of little value, so long as the existing errors remain in the lunar tables, and the positions of the culminating stars.

With respect to the errors of the lunar tables, from which the moon's places are computed for the *Almanac*, the author finds, from a comparison of the Greenwich observations in 1836 and 1837, that the error may be assumed as amounting, on the average, to $\pm$ 0·5 of a second; and this, in reference to resulting differences of meridians, amounts to between 12 and 17 seconds of

time. In fact, several instances occur, where the errors are double the amount here stated. With respect to the culminating stars given in the *Almanac*, he remarks that it appears from Mr. Wrottesley's Catalogue, and also from occasional observations, that the mean places of most of the small ones stand in need of considerable corrections; and he thinks it may not be far from the truth to assume, that the errors in right ascension of many of them are not less, generally considered, than those assigned to the moon. Now it is obvious, that whilst such an accumulation of error is likely to occur, the comparison of observed with computed intervals can only be productive of very uncertain results; and the position of any station must be extremely doubtful, for the determination of which they would be received, even in the light of an approximation.

The author remarks, in conclusion, that the corrections of the lunar tables that would be necessary, in order to render this application of the method available, are scarcely to be expected, since notwithstanding the immense number of observations of the moon which have been made, for many years past, the correction of the elements of her motion approximates so very slowly as to place the desired point of accuracy at an almost hopeless distance: but there is no reason why the places of the stars employed in moon-culminating observations should remain in so uncertain a state; and it would be of great advantage if their places were more accurately determined.

II. On the Optical Glass of the late Dr. Ritchie. By Mr. Simms.

The author states, that as some interest is felt respecting the optical glass of the late Dr. Ritchie, most of which he purchased at the sale of the Doctor's effects, his object is to communicate to the Society such particulars respecting it as his experiments have enabled him to collect. Besides several pieces of very excellent crown, Mr. Simms obtained twenty-nine discs of flint-glass, varying in diameter from 4 to 7½ or 8 inches. These were circular, and had evidently been cast in a mould; in thickness they varied exceedingly, some being scarcely thick enough for the purpose for which they were intended; and others at least three times as thick as necessary. Of these twenty-nine discs, only three give promise of being fit for telescopes at all; two of 4, and one of 4½ inches diameter, and even these not likely to prove of the first order. The rest are, literally, full of veins and striæ; and the bestowal of any labour on them would obviously end in loss and disappointment.

The results of a series of comparative experiments are stated in the following table, in which the refractive and dispersive powers, for the lines of the spectrum distinguished by Fraunhofer as B and G, are pretty accurately given for the four specimens of flint-glass that were examined. The first is of glass, made by Guinand of Neuchâtel; the second was made several years since, at the Stangate Glass-works, near Lambeth; the third, by Dr. Ritchie; and the fourth, at the Falcon Glass-house, Whitefriars; and it will be seen, that in all those qualities which were made the subject of

investigation, the flint-glass of Dr. Ritchie differs little, if at all, from that which is generally made in England. The fact, however, of a disc of flint-glass, 7½ inches in diameter, having been made sufficiently perfect for the construction of a good achromatic telescope, is unique in the history of glass-making in this country.

Flint-glass.	Specific Gravity.	Index of Refraction for B.	Index of Refraction for G.	Dispersive Power B. to G.
Guinand..	3·6459	1·616	1·644	·044
Stangate...	3·3747	1·594	1·619	·041
Ritchie ..	3·2269	1·573	1·597	·041
Falcon ...	3·1964	1·570	1·593	·039

III. On the Determination of the Longitudes of the Observatories of Edinburgh and Makerston, by means of Chronometers. By Mr. Dent.

The object of this communication is to describe an experiment for determining the longitude of Edinburgh and Makerston (the observatory of Sir Thomas Macdougall Brisbane), by means of twelve chronometers, furnished for the purpose by Messrs. Arnold and Dent. The chronometers, after their rates and errors had been ascertained at the Royal Observatory of Greenwich, were conveyed to Edinburgh in a steam-ship, under the care of Sir T. Brisbane; and, after remaining some time at the Observatory there, were taken to Makerston (a distance of 40 miles) by Professor Henderson in a stage-coach, whence, after an interval of four days, they were brought back to the Observatory at Edinburgh, and ultimately returned by a steamer to Greenwich, where their errors and mean daily rates were again determined. The interval between the last comparison at Greenwich (in the first series) and the first at Edinburgh, was 54 hours 49 minutes; and between the last at Edinburgh and the first at Greenwich, after their return, 51 hours 47 minutes. The short distance between Edinburgh and Makerston allowed the comparison to be made at both observatories on the same day. The whole time the chronometers were absent from the Royal Observatory was 29 days.

The differences of longitude were computed by Mr. Henderson, who explains the method of computation, and gives the results in a letter which forms an appendix to the present paper. Mr. Henderson calculated the observations by two different methods, described in a former communication by Mr. Dent (see *Monthly Notice* for January 1838). The mean result of all the chronometers, computed by the first method, gives the difference of longitude between Greenwich and Edinburgh $=12^{m}\ 42^{s}\cdot 99$; and Mr. Henderson states that, in consequence of this determination, he now adopts $12^{m}\ 43^{s}\cdot 0$ as the longitude of Edinburgh, in place of $12^{m}\ 43^{s}\cdot 6$, which he had formerly used, and which was the result of the trigonometrical survey.

The mean results obtained from the second computation are as follows:—

	m	s
Voyage to Edinburgh	12	42·25
Voyage back	12	43·69
Mean..........................	12	42·98

Mr. Henderson remarks, that the difference between the results of the two voyages are nearly of the same amount, and operating in the same manner, as was observed in the Paris experiment described in Mr Dent's former paper, the place of destination being made more easterly in the journey from Greenwich than in the journey back.

By the present determination, the longitude of Makerston from Greenwich is $10^m\ 3^s{\cdot}66$. The longitude given in the *Nautical Almanac* is $10^m\ 4^s{\cdot}0$, as obtained by Sir T. Brisbane, by joining his observatory with some of the stations of the trigonometrical survey.

The performances of the chronometers appear to have been, on the whole, very satisfactory.

IV. Some Account of the Progress of the Trigonometrical Survey now carrying on in India. Extracted from the Correspondence of Colonel Everest, and communicated by Major T. B. Jervis.

The narrative commences with the season of 1833–34, during which period the operations appear to have been confined to the selection of proper stations, and the arrangement of convenient methods for carrying the survey along the Doab territory; for measuring a base-line of verification near that district, and also for extending the triangulation to the very foot of the Himalaya mountains.

In all the previous operations of this great trigonometrical survey, the principal stations were on the highest points of the mountains, or hilly land, which were visible in broad day from each other, so that there was no difficulty in selecting the most convenient and favourable spots. A new district, however, was now about to be traversed, where such advantages would not present themselves. Colonel Everest describes the Doab as a country which, though coming under the denomination of a flat alluvial formation, totally devoid of all natural elevations, has yet many mounds, which appear to have been raised by the inhabitants as a protection against inundations and external violence. It is very highly peopled and cultivated, and the villages lie so thickly scattered, that it is difficult to trace a line in any direction, so as to pass free of all habitations; and quite impossible to calculate on seeing through the intervals of the trees, which, though few, would probably intercept the visual ray from one station to another. Much time, therefore, was taken up in reconnoitering the country for the selection of such spots as might be most favourably situated for carrying on the triangulation.

The plan which Colonel Everest contrived and adopted for the signals was the erection of lofty masts, 70 feet in height, the nucleus at the base being formed of an upright of Sāl timber, and the superstructure consisting of bamboos tied together, and linked with the protruding ends of those which enclosed the nucleus. A pulley, fixed at the top, furnished him the means of raising a bamboo staff in a horizontal position to that height: on attaining which a sway-rope, appended to one end of it, enabled him to raise an ignited blue-light, fixed at the other end, 20 or 25 feet above the mast, or a quantity equal to half the height of the staff. The instrument for taking the angles between the stations was supported on a temporary scaffolding, in the construction of which every precaution was taken to prevent any tremulous motion.

Whoever has travelled in the upper provinces of India, will be aware of the difficulty of bringing together a large mass of materials, such as was here required; but this was not the only difficulty Colonel Everest had to encounter: for at this time he had scarcely any establishment at all; and of the men at his disposal, there were not half a dozen who had ever seen a heliotrope, and not one who knew how to use that instrument. Even the European officers and assistants attached to the survey, had little or no experience in operations of this kind; and were utterly unacquainted with the methods pursued, and the precautions necessary to ensure accuracy. In order to procure the requisite assistance, he sent a party into the country to seek for recruits; and they returned with about a hundred: but a more unpromising assemblage could not easily be conceived. However, the difficulties disappeared one by one, under patience, system, and good example; and, before the operations of the season were over, many of those who at first seemed so untractable, became efficient and valuable servants.

In looking about for a favourable spot for measuring a base-line, he had the good fortune, in the first essay, to select one which, in the end, proved the most convenient and proper. This was at Dhera Dun, near Sisee Bara, on the verge of the Asan river. The site presented an uninterrupted view for several miles, nearly free from trees; and two adjacent mountains were admirably adapted for transferring points.

The season of 1834–35 was occupied in measuring the base; the inequalities of the ground were removed as much as possible; and for crossing such land as was swampy, or otherwise treacherous, pickets of various sizes, from 3 to 8 feet in length, shod with iron at both ends, were driven into the ground. Tressels also, of various heights, were provided, in order to preserve the horizontality of the alignment. As soon as the medical officer pronounced that the Dun could be entered without danger (at some seasons it is a deadly country), Colonel Everest sent a party to clear away the high grass, and set up the stone pillars on which the microscopes were to be placed, for the comparisons of the measuring rods with one of the iron standards brought by him from England. Colonel Everest himself took the management of the boning instrument,

and the other officers and assistants had charge of the microscopes. The east end of the base is 185·2 feet higher than the west end; but the measures are reduced, on an assumed height of the spot above the level of the sea. Colonel Everest divided the whole line into three sections; and, when the measurement was completed, he selected three stations on the northern face of the Sevalic range, with which the two subdividing points, and two ends of the base, formed a series of small triangles: his object being to try one portion of the base against another by triangulation, as well as by measurement. The result was very satisfactory; the difference being less than one inch. As this, however, might be the result of accident, or of a fortunate combination of circumstances, he resolved to measure the base a second time. Every precaution for insuring accuracy was scrupulously attended to; the bars were never, for an instant, exposed to the direct action of the sun's rays, but were constantly under the cover of a range of tents: nor was one side exposed to a greater heat than the other. The microscopes were frequently compared with their scales, their errors noted, and either taken into account or eliminated. The length of the first measurement was 470,206·77 inches (nearly 7½ miles), and the length of the second, 470,204·37 inches; there being a difference of only 2·4 inches between the two. The length of the three sectional parts above alluded to, as determined by triangulation, was 470,205·95 inches. The mean of the whole is 470,205·70 inches.

Colonel Everest took out with him a complete set of compensation measuring-rods, similar to those adopted in the Irish survey by Colonel Colby, together with an iron bar for the purpose of occasional comparisons with the rods; a similar iron bar, with which it had been carefully compared, being left in England for the purpose of obtaining its length, in terms of the Imperial standard measure. *It does not appear that this has yet been determined: so that the length of the base can only, at present, be considered as approximate.*

The measuring apparatus here alluded to was set up, and continued for several weeks, in Lords' Cricket-grounds: where Colonel Everest, and the officers and assistants about to be employed in the measurement, had an opportunity, under the able advice of Colonel Colby, of perfecting themselves in the manipulation of the instruments, and the methods of proceeding.

The season of 1835–36 was employed in carrying on the triangulation; in which Colonel Everest was much impeded by a long continuance of hazy and cloudy weather. It appears, however, that the principal triangulation was brought down to the line from Juktipura to Pagara; and that the stations immediately to the north of the latter place, had been visited by the large instruments: so that, except for the purpose of observing the supplemental, or third angles, at the two above-mentioned stations, the series may be considered as completed to the north of the river Chumbul.

For his station-lights, Colonel Everest found the Argand lamp, with a parabolic reflector, to be very effective, even in the most

stormy weather. The lamp was enclosed in a wooden case, with a tin chimney at top, and a circular glass aperture in the door. As a proof of the efficacy of this arrangement, he states, that during a violent storm which set in from the east, and which lasted three days and nights without intermission, accompanied by much thunder and lightning, the rain falling in torrents, and the wind blowing in violent gusts against the two stations (Deri and Pahira), situated to the westward of Karol tower, at which he was observing, the lamps never waned for an instant during the night; and although the distance was nearly twenty-seven miles, the light was as vivid and steady as a star of the third magnitude.

During the operations in the Doab, Colonel Everest encountered some very extraordinary phenomena of refraction. He says, "That in many instances the distant heliotrope has, in the morning, instead of exhibiting a round disc, displayed itself in the form of a tall column. One of these was found, by actual measurement, to occupy 440″ in the vertical plane; which, at that distance, is equivalent to a tower of 193 feet in height; and some of them seemed to occupy a still greater space." Again, the heliotrope, in one instance, when observed in the morning at $7^h\ 20^m$ stood at an angle of 4′ 32″ *elevation;* and on the following morning, at the same hour, the angle of *depression* was 4′ 36″: the difference in the two days being 548 seconds. In some instances, in the month of January, the distant heliotrope had its periodic hour of rising before sunset; but was never seen at any other hour of the day. In the months of March and April, however, the heliotropes, at sunrise, and sometimes for an hour after, were seen projected high upon the sky, and frequently in the form of a tall column.

Colonel Everest remarks, that the afternoon rise of the distant heliotrope is curious and beautiful. The first rays spread themselves like a running fire along the surface of the obstructing land, as if the disc were throwing out wings; the light then descends and re-ascends, till after a few oscillations it ultimately rises into a clear round disc, and remains visible till the sun becomes too feeble for reflection. The descent in the morning is equally remarkable. In favourable weather the round disc appears immediately subsequent to sunrise, projected high up in the sky; and, after having displayed itself in this form for a short time, it either gradually descends in the reverse order to that of the afternoon, or assumes the columnar form, and suddenly vanishes as if by an explosion.

Colonel Everest complains of a practice, too frequently followed by the natives in some of the districts, of destroying the stone platforms erected at the stations, on which the instruments are placed, as soon as the surveying party has left the place. Superstition, bigotry, and ignorance, have in all cases been the cause of this barbarous conduct; as the most absurd stories have been propagated respecting the incantations and ceremonies adopted by the English in erecting these stations.

Colonel Everest reserved for the last operation the determination, by celestial observations, of the zenith distances of a certain

number of fixed stars, for the purpose of ascertaining the difference of latitude between certain stations. For this object, two altitude and azimuth circles were sent out from England, whose vertical circles were 3 feet and lower circles 2 feet in diameter: these instruments, however, were found to require certain alterations, which have considerably retarded the execution of the measure. In the meantime, he was induced to re-measure with the compensation bars the base line at Seronge, which he had measured in the year 1825 with the steel chain formerly used by Colonel Hamilton. Colonel Everest had remarked in his book, pages 132 and 264, that there was a discordance of 78·72 inches in the Beder base, as shewn by actual measurement and as deduced by triangulation from the base at Takal Khera; whilst the difference in the Seronge base was only 33·2 inches, whether deduced from actual measurement or by triangulation from the Takal Khera base. This led him to suspect the accuracy of the measuring chain, and induced him to compare it with one of the standard iron bars belonging to the new apparatus, in order that its state at that time might be placed on record. These comparisons were very carefully made in the year 1832, and as they were highly satisfactory, he is of opinion that the measurements, both at Takal Khera and at Seronge, may, as far as the capability of the chain extends, be considered as reducible to the same standard with the base at Dehra Dun. But he apprehends that the case is very different with all the bases that were measured prior to that of Takal Khera: for it appears that, in the intermediate time, owing to the want of due precaution, the joints of the steel chain had become thickly covered with, and in fact eaten into by, rust; in the process of clearing away which, the length of the standard of reference was lost for ever, and beyond remedy or retrace.

On re-measuring the Seronge base with the compensation bars, the length came within 7½ inches of the length computed all the way down from Dehra Dun, a distance of 460 miles: thus confirming Colonel Everest's suspicions respecting the true cause of the discordancy in the other bases, and shewing the necessity of re-measuring such of them as may exhibit any anomaly. In fact, it is his intention to re-measure the base at Beder with the new compensation rods, prior to the termination of his labours in India: and it is his opinion, that it will be requisite to revise the whole of Colonel Lambton's work, before it can be properly joined on to the series of triangles recently completed with the new instruments.

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TO

VOL. IV. OF THE MONTHLY NOTICES.

LONDON :— PRINTED BY MOYES AND BARCLAY, CASTLE STREET, LEICESTER SQUARE.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1839, *TO JUNE* 1843.

VOL. V.

LONDON:

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1843.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *November* 8, 1839. No. 1.

Sir John F. W. Herschel, Bart. President, in the Chair.

Beriah Botfield, Esq. F.R.S., the Rev. W. Dealtry, D.D. F.R.S., and Major Edward Sabine, R.A., F.R.S., were severally balloted for, and duly elected Fellows of the Society.

The following communications were read :—

I. On the Determination of the Orbits of Comets, from Observations. By G. B. Airy, Esq. Astronomer Royal.

The author begins by remarking, that the generality given by Laplace to the investigation of the orbits of comets is so complete, and the variations on the method introduced by other writers so numerous, that, as regards generality and facility, the subject may probably be considered as exhausted. The method which is developed in the present Memoir professes to be merely a modification of Laplace's method, directed by considerations of a purely practical nature, which are known to the working astronomer; but which, probably, have not occurred to the distinguished mathematicians, who have laboured on the theoretical difficulties of the problem.

"Every method," the author remarks, "which I have yet seen requires that the observed geocentric places of the comet be reduced to longitude and latitude. The places must, however, in the first instance, be observed in right ascension and declination. Now, the conversion of right ascension and declination into longitude and latitude is one of the most troublesome operations that commonly occurs. It requires the use of 7-figure logarithms, and is liable to errors. An alteration in one original Æ, or declination, requires a complete repetition of the calculations; and when all is done, the elements of the comet's orbit are obtained as referred to the ecliptic; and, for convenience of calculating predicted places, it is generally necessary to refer them back to the equator. For these reasons, it has long since appeared to me desirable that the orbits should be deduced at once from the right ascensions and declinations. Since I have become familiar with the instruments used for observing comets, an additional reason has suggested itself. It is known that on the assumption of a parabolic orbit, the equation given by three complete observations, or by observations which furnish the Æ and declination at a certain time, and their first and second differential coefficients, are one more than are necessary; and, therefore, it rests with the computer to use his discretion in reject-

ing one of the observations. Now, it often happens that the instrumental or observing errors in right ascension are of an order quite different from those in declination; and, if the method of computation proceeds at once from right ascensions and declinations, the astronomer can at once determine which of the observations ought to be rejected in the calculation, on the score of possible inaccuracy in the observation."

The principal objection which has been made to Laplace's method is the trouble of investigating the differential coefficients of the spherical co-ordinates. It must be avowed, that the process pointed out by Laplace is very laborious; but it may also be asserted, that the principal part of the labour is introduced without any necessity. Three observations, made at proper intervals, are sufficient to give the motion of the comet in either direction, and its two differential coefficients, with an amount of labour that is quite insignificant; or a great number may be introduced by a simple process well known to every computer, and involving very little trouble. In the present paper it is shewn, that by adopting for epoch the middle time between the first and second observations, the great mass of the calculations of every kind may be made immediately after the second observation; and the operation, therefore, completed in a very short time after the third.

The author divides his paper into three sections. In the first he gives the "theory," or analytical solution of the problem. On substituting, in the general equations of motion, the right ascension and declination of the comet at the epoch with their first and second differential coefficients, which are given by the observations, he arrives at two equations in which the unknown quantities are ϱ and $\frac{d\varrho}{dt}$ (ϱ denoting the comet's distance from the earth). On eliminating $\frac{d\varrho}{dt}$ an equation is found of the following form—

$$C \cdot \varrho = \frac{D}{(\varrho^2 - E\,\varrho + F)^{\frac{3}{2}}} + G,$$

where C, D, E, F, G, are known numerical quantities. The solution of this equation may be obtained with great facility (in respect of the general difficulties of the problem) by the method of trial and error; and the author recommends, that in all cases which admit of it, the equation be formed, and the solution found; not only because the method is comparatively easy, but also because it is perfectly general, no assumption of parabolic, circular, or any other form of orbit, having been made.

The author next proceeds to consider the cases in which the equation fails. These are, first, when the comet is in conjunction with, or in opposition to, the sun; or when the sun, the earth, and the comet, are in the same straight line. In this case the first side of the equation becomes 0 divided by 0; and, as the two equations which involve the first differential coefficient of the comet's distance, taken with respect to the time, also vanish in the same circumstances, the failure is absolutely beyond remedy, and we can only wait until the comet is in a different part of its orbit. Secondly,

the equation fails when the apparent path of the comet is directed to or from the sun's place; but in this case, the two equations involving the first differential coefficient of the distance do not necessarily fail; and, in fact, they cannot both fail, excepting under the supposition of the first case; therefore, by using one of them, or a new combination of them, together with some new single assumption (as for instance, that the comet is moving in a parabola of unknown perihelion distance), we may still determine the comet's distance. Thirdly, the application of the equation may fail from causes connected with instrumental observations; for as the second differential coefficients of the right ascension and declination both occur on the first side of the equation, and as these coefficients are affected by the whole of the errors of observations, which, if the interval between the observations is short, receive very small divisors, any failure in the instrumental determination will produce a large error in their proportionate values. As it will sometimes occur that the observations made in declination are far more accurate than those made in right ascension, or *vice versâ*, in most cases one of the two equations which contain the comet's distance and its first differential coefficient, will be preferable to the other; and the combination of this with the equation deduced from the assumption of a parabolic orbit, will lead to the elimination of the differential coefficient, and, consequently, give the distance.

Among the various changes to which the comet's apparent path is subject, and of which an arbitrary choice may be made, for the purpose of determining the distance in the cases in which the general equation fails, or becomes unsafe, the author considers the following to be the best:—viz. first, the curvature of the comet's path, produced by the sun's action (or the deflection measured only in the direction perpendicular to the apparent path); second, the acceleration in its path, produced by the sun's action (or the deflection measured only in the direction of its path); third, the deflection in the direction in which both the sun's action on the comet and the sun's action on the earth would cause a change of the comet's apparent place (or the deflection measured along the great circle joining the comet with the sun). These changes are severally considered, and the method of forming the equation proper for each condition explained, and rules deduced for the guidance of the computer in all the particular cases in which the direct method cannot be followed. In these investigations the correction of observed places of the comet for parallax is entirely omitted, as it is most convenient, when ϱ is approximately found, to correct the observations for the corresponding parallax, to make the proper alteration in the second differential coefficients, and then to repeat the process of approximation to the value of ϱ.

Having given the methods for finding the distance and its differential coefficient, the author concludes his first section with an indication of the process by which the elements of the orbit are computed. In the rules for the selection of the equations on the parabolic assumption, some considerations are introduced which are new and important.

The second section contains remarks on the method of obtaining numerical values of the differential coefficients of the right ascension and declination from the observations. In the use of these quantities, what we have to consider is, not the effect of absolute error in their values, but of proportional error. An error of a single second in the value of the second differential coefficient of Æ may produce an ultimate error as great as would be produced by twenty seconds in the value of the first differential coefficient; or as great as would be produced by ten minutes in the Æ itself. This consideration allows the computer to determine many of the numbers which enter into the equations after the second observation: the method of proceeding is as follows:—

"Adopt for the epoch the middle time between the first and second observations; then the first differential coefficients of α and β (α denoting the right ascension, β the declination) will be obtained accurately by dividing the changes of α and β by the intervening time: and the values of α and β for the epoch will be obtained with sufficient accuracy, by taking the means of α and β for the two observations."

The third and last section of the Memoir gives practical rules for the computation of the observations. The successive steps of the process, from the first observations to the determination of the different elements of the orbit, and the values of the quantities required for predicting geocentric places, are minutely and distinctly stated, so that the ordinary computer will find no difficulty in applying the method.

II. Extract of a Letter from Professor Schumacher to the Astronomer Royal, relative to the determination of differences of Longitude, by observations of Shooting Stars.

M. Schumacher states that, although observations of shooting stars have long since been proposed by Mr. Benzenberg as a means of determining differences of longitude, no attempt has yet been made to carry the plan into practice. With a view to ascertain the degree of exactness with which such observations can be made, he resolved to make some trials on the night of the 10th of last August. He preferred to observe the *extinction* of the meteor, because its apparition gives warning, and in some measure prepares the observer for the phenomenon. Having given no notice of his intention to other astronomers, he had no expectation of obtaining corresponding observations; but was agreeably surprised when he subsequently obtained them from Bremen, Breslau, and even Königsberg. They did not give very accurate differences of longitude, because the observers at those places had observed the apparition and not the extinction; and because, not having the same object in view, they did not ascertain the equation of the clock with precision. Nevertheless the observations gave approximate differences, and shewed that the method is practicable.

London:—Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *December* 14, 1839. No. 2.

Francis Baily, Esq. Vice-President, in the Chair.

The Rev. J. W. Maher, B.A. of Queen's College, Cambridge; the Rev. Temple Chevellier, Professor of Mathematics and Natural Philosophy in the University of Durham; Lieut. Henry D. Harness, R.E.; and Stephen J. Rigaud, Esq. Fellow of Exeter College, Oxford, were severally balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. On the Parallax of Sirius. By Thomas Henderson, Esq. Astronomer Royal for Scotland.

The parallax of Sirius, the brightest star in the heavens, has been several times the subject of investigation among astronomers. From the variations of the zenith distances observed at Paris, the second Cassini inferred a parallax in declination amounting to six seconds of space; and, from similar variations in the observations of La Caille made at the Cape of Good Hope, some astronomers have deduced a parallax in declination of four seconds. Piazzi has also obtained from his observations a parallax of the same amount. On the other hand, La Caille's observations of zenith distances made at Paris, more numerous and certain than those made at the Cape, do not exhibit any sensible parallax; and the observations which have since been made in the observatories of Europe, would appear to lead to the same result, as no parallax has ever been deduced from them. In the *Fundamenta Astronomiæ*, M. Bessel has investigated, from Bradley's Observations of Differences of Right Ascension of Sirius and *α Lyræ*, the sum of the parallaxes of the two stars, and has found it to be an insensible quantity.

The extensive series of observations of Sirius, made with the mural circle of the Observatory at the Cape of Good Hope, is well adapted for the investigation of the parallax, as the observations possess some advantages over those made in Europe. The star is near the zenith of the Cape, and the temperature is nearly the same when it passes the meridian at noon in June, and at midnight in December, the periods of the greatest parallaxes in declination; so that the irregularities and uncertainties of refraction, which affect observations in Europe, may be supposed to disappear.

From May 1832 to May 1833, ninety-seven observations of Sirius were made by Mr. Henderson with the mural circle at the Cape Observatory, of which sixty-three were made by direct vision, and

thirty-four by reflexion; and in Mr. Maclear's printed observations of zenith distances, made with the same instrument, there are sixty-seven observations of the double altitude of the star, made between August 1836 and December 1837. Each of these series of observations was made in one position of the telescope upon the circle, so that in each series the similar observations were referred to the same divisions.

The observations made by Mr. Henderson have been reduced in the same manner as were those of α *Centauri*, given in his memoir on the parallax of the latter star. (*Monthly Notice*, Vol. IV. No. 19.) The declinations of Sirius have been determined by comparisons with such of the principal or standard stars as were observed on the same day; and it is consequently assumed that, in the observations of the stars of comparison, any errors which may arise from supposing their parallaxes to be insensible, and the coefficient of aberration to be correctly assumed, neutralise each other. The mean declinations of the standard stars of comparison have been taken from the catalogue annexed to the author's "Memoir on the Declinations of the Principal Stars;" the absolute places of the stars are not required, but only their relative positions with regard to each other.

Mr. Maclear observed the double altitudes of Sirius, or the angular distances between the star seen by direct vision and by reflexion at the same transit over the meridian. These are independent of the observations and assumed positions of other stars, and are affected by twice the amount of the parallax in declination. Observations of this description appear to be the best adapted of any which can be made with the mural circle for the investigation of small variations in the declination of a star.

In the reductions of Mr. Henderson's series the constant of aberration has been assumed $= 20''\cdot50$; of Mr. Maclear's, $= 20''\cdot36$; in the reductions of both the coefficient of lunar nutation has been assumed $= 9''\cdot25$, and the annual precession and proper motion have been taken from the *Tabulæ Regiomontanæ*.

Mr. Maclear's observations are suitable for determining the constant of aberration; the correction to be applied to it has been therefore introduced as an unknown quantity into the equations of condition; and, from the value which is obtained, we may judge of the degree of accuracy with which the parallax is determined.

On resolving, by the method of minimum squares, the two sets of equations, and combining the results according to their relative weights, the greatest effect of parallax in declination is found, from the whole of the 231 observations, $= + 0''\cdot15$; and the greatest effect of aberration in declination, $= 13''\cdot07$. These quantities are to the total effect of parallax and aberration in the proportion of $13''\cdot13$ to $20''\cdot50$, whence the final results are —

Parallax of Sirius (or the angle subtended by the radius of the earth's orbit, at a distance equal to that of the star)......	=	$0''\cdot23$
Constant of Aberration..	=	$20\cdot41$

The error of this determination of the parallax may be estimated not to exceed a quarter of a second, as it is almost certain that the

constant of aberration is not in error to a greater amount. On the whole, it may be concluded that the parallax of Sirius is not greater than half a second of space, and that it is probably much less.

II. A Catalogue of Twenty-seven Stars of the Pleiades. By M. Bessel, Director of the Observatory of Königsberg.

The catalogue was computed by M. Bessel from meridian observations made by himself and his assistant, Dr. Busche. It contains the positions, annual precession, and its secular variations in Æ and declination, together with the proper motions, and a comparison with Piazzi's catalogue.

In a letter addressed to Mr. Baily, containing the above catalogue, M. Bessel announces, that the observations respecting the parallax of 61 *Cygni* (of which an account is given in the *Monthly Notices*, Vol. IV. No. 17), have been continued through a second year; and that the result of this new series will agree very nearly with that of the first. The publication of the observations will be delayed for a few months, in order to obtain a more certain determination of the proper motions which the two small stars compared seem to possess; and he adds, that although the *weight* of the former result was sufficiently great to leave no doubt about the real existence of the parallax, it is gratifying to see its quantity so very nearly confirmed by a second series of observations.

III. A Letter from M. Valz, Director of the Observatory at Marseilles, to the President, Sir J. F. W. Herschel, Bart., relative to the Variation of the Apparent Diameter of Encke's Comet.

After adverting to some objections suggested by Sir John Herschel (*Memoirs*, Vol. VI. p. 102) to the theory by which M. Valz explains the changes observed in the apparent diameters of some comets, when near their perihelia, namely, the condensation of volume produced by the pressure of an ethereal medium growing more dense in the vicinity of the sun, the author proceeds to give his own observations on Encke's comet, at the time of its last perihelion passage in 1838, when it appeared under circumstances favourable for observing the nebulosity. He states, that he was able to follow the comet till the evening before the perihelion passage; that he observed it to diminish rapidly, and, after being prodigiously reduced, to melt away, as it were, under his eyes, disappearing only in consequence of its extreme smallness, inasmuch as its brilliancy should, from its position, have continued to increase. The observations are as follows:—

On the 9th and 10th of October, the nebulosity subtended an angle of 20′, but it diminished continually after that time. On the 15th of October, he first remarked it to be elongated in the direction of the sun; and the elongation continued to increase until the 25th of October, when the greater diameter appeared to be double the smaller, after which it began to diminish. The most luminous part was not at the centre, but at the point opposite the sun. On the 25th of October, the nebulosity was reduced to 15′, and the real volume was then eighteen times smaller than on the 10th. On the 6th of November, the nebulosity was 13′, and the volume reduced

to 1–40th. On the 13th of November, the nebulosity was 11′; on the 16th, between 8′ and 9′; on the 20th, between 6′ and 7′; on the 23d, 4′; on the 24th, 3′, and the real volume, 826 times less than on the 10th of October. On the 29th of November, the comet could no longer be seen in the evening twilight, but it reappeared on the morning of the 7th of December. On the 12th of December it appeared as a star of the fifth magnitude; and its diameter was less than 20″, being entirely covered by a wire of that thickness. The volume deduced from this apparent magnitude would be 80,242 times less than on the 10th of October. On the 14th of December it appeared feebler, and equal to a star of the sixth magnitude, with which it was compared; its diameter was then estimated at 15″. On the 16th, the comet appeared as a star of the seventh magnitude, and its apparent diameter was from 10″ to 12″. On the 17th, it was reduced to the eighth magnitude at most, and its apparent diameter was from 7″ to 8″. On the 18th of December it was entirely invisible, although stars of the seventh and eighth magnitudes were seen in its neighbourhood. From these comparisons it appears that the real diameter must have undergone a diminution from the 10th of December, when it was first observed in the morning, until the 18th, when it finally disappeared.

IV. A Letter from Professor Schumacher, to Francis Baily, Esq., announcing the Discovery of a Comet by M. Galle, assistant in the Berlin Observatory.

The comet was discovered on the 2d of the present month, $17^h\ 45^m$ mean time (Berlin), in the constellation *Virgo*. Comparing it by the great refractor, with a star of the tenth magnitude (which star was immediately compared with γ *Virginis*), M. Galle obtained the following positions :—

Sidereal Time, Berlin.	AR. of Comet.	Declination of Comet.
h m s	h m s	° ′ ″
11 1 14	12 38 25·18	
11 9 42	12 38 28·26	− 2 10 22·8
11 21 45	12 38 32·38	− 2 10 13·9
11 40 39	12 38 39·63	− 2 9 57·3

These observations give its daily motion in Æ, + 2° 12′, in decl. + 0° 19′. It has a well-defined point, as a nucleus, within the uniform nebula, which, opposite to the sun, expands in the form of a tail.

V. Tables for the Calculation of Precession, for the year 1825, of Stars observed by M. Bessel in the several Zones, from — 15° to + 15° Declination. By Dr. Max. Weisse, Director of the Observatory at Cracow.

VI. Observations of Moon and Moon-culminating Stars, Eclipses of Jupiter's Satellites, and Occultations of Fixed Stars by the Moon, made at the Observatory of Paramatta, in New South Wales, in the year 1838, by Mr. Dunlop. Communicated by Sir Thomas Macdougal Brisbane, Bart.

London : — Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *January* 10, 1840. No. 3.

Sir John F. W. Herschel, Bart. President, in the Chair.

The Rev. George Wright, B.A. of Trinity Terrace, Southwark; Thomas John Main, Esq. Fellow of St. John's College, Cambridge; John Caldecott, Esq. Astronomer to His Highness the Rajah of Travancore; and Capt. John T. Boileau, of the Bengal Engineers, were severally balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. Ephemeris of the Comet now visible. By Mr. C. Rumker, of Hamburg. Communicated by Dr. Lee.

This ephemeris gives the daily right ascension and declination of the comet from the 4th to the 24th of the present month, at 4 P.M. mean equinoctial time: the author stating that he was desirous of establishing the fact of its visibility *after sunset.*

II. A Letter from Mr. Henry Lawson to the Secretary describing the appearance of the Comet, as seen at Hereford.

The comet was observed by Mr. Lawson on the mornings of the 23d and 29th of last month, and of the 8th instant. It had a tolerably well-defined nucleus, with a brushy tail on the side opposite to the sun. The nucleus subtended an angle nearly equal to half the visual angle subtended by *Jupiter;* and the tail filled the whole field of view, the diameter of which was three minutes of time.

III. Apparent Positions of the Comet observed at Edinburgh. By Professor Henderson.

Date.	Mean Time at Edinburgh.	Apparent Right Ascension.	Apparent Declination.
	h m s	h m s	° ′ ″
1839. Dec. 28	18 8 34	16 37 45·8	....
	18 12 52	....	+3 18 14
1840. Jan. 4	18 2 55	17 26 35·4	2 23 7
5	18 11 23	17 32 52·7	....
	18 32 49	....	2 10 55

IV. Observations of the Comet made at Ashurst and Dulwich. By Robert Snow, Esq.

Mr. Snow found the comet on the 28th of December. The observed diameter of the head was then 58″, and the tail extended beyond the field of view. It was again observed on the 29th, and also on the 5th and 6th of the present month, when it was very bright and easily found. The nucleus was large, but not stellar.

V. Occultations of Stars by the Moon to the end of 1839. By Robert Snow, Esq.

VI. Catalogue of the *Pleiades*. By Robert Snow, Esq.

The author states that this catalogue does not lay claim to strict accuracy, but was constructed in order to form a chart, which might be consulted with advantage when occultations of stars in the *Pleiades* by the moon take place. Piazzi's stars falling within the limits of the chart were taken as standards, and the differences between them and the other stars determined by a wire micrometer. For some stars, too faint to allow of illumination, the ring micrometer was used; but they were more usually put down by estimation, which may be done with nicety when many are in the field together. This communication was accompanied by a chart.

VII. On the Variability of α *Cassiopeiæ*. By R. Snow, Esq.

In the *Monthly Notice* for May last (Vol. IV. p. 195), the attention of the Society was directed to the supposed variability of this star; and it has, accordingly, been watched, with the naked eye, since June 9, 1839, up to the present time, January 8, 1840. The relative brightnesses of α, β, γ *Cassiopeiæ* have been registered on sixty-eight evenings. The result at present is that γ has been generally put down as brightest, and never faintest; β generally as faintest; α faintest twelve times out of sixty-eight, and generally so about the 5th day of the month. In the Society's Catalogue, the order of magnitude is β, $\alpha = \gamma$. Mr. Snow remarks that the star α appears to his eye at all times sharper and better defined than γ or β; and it is also more readily obscured by fog or haze, although it is a reddish star.

VIII. Observations of α *Cassiopeiæ* in 1831 and 1832. By Mr. Birt. Communicated by the President.

These observations were commenced in April 1831, and extend to November 1832; and the earlier part of them, from April to December 1831, have already, with some others, been communicated to the Society. See *Monthly Notices*, Vol. II. No. 11. In a letter to Sir John Herschel, Mr. Birt states, that since 1832 his attention had not been directed to this star until he read the *Monthly Notice* for May last, when it immediately occurred to him that his observations might probably assist in determining the period in which the brightness of the star completes the circle of its gradations. When Mr. Birt commenced observing the star in April 1831, the lustre appeared to be at its minimum. In December, of the same year, he again observed it to be less than β. His observations were then discontinued until June 1832, when it again appeared less than β. Taking the extreme observations, we have thus two periods completed in about fifteen months, or one period in about 225 days. Assuming this as the period of the variation, and computing from April 26, 1831, the number of days elapsed until April 28, 1839, is 2924, which gives thirteen periods of about 225 days each. Sir John Herschel's observations give the [illegible] from November 12, 1838, to January 22, 1839; and

calculating from July 7, 1831, twelve periods, the maximum would be obtained on December 4, 1838. On the whole, Mr. Birt concludes that the period of 225 days may be regarded as a first approximation, which may receive correction from a comparison with the earlier observations of Sir William Herschel.

IX. On the Variability and Periodic Nature of the Star *α Orionis*. By Sir John F. W. Herschel, Bart. President.

"In a communication which was read to this Society on the 10th of May last, I pointed out the star *α Cassiopeiæ* as variable and periodical. That the fluctuations in splendour of this star should have escaped general notice is not extraordinary, since the difference between its greatest and least brightness can hardly be estimated at more than half a magnitude. But that a periodical variation to a very much greater extent, in so important and remarkable a star as *α Orionis*, should, up to this time, have been completely unnoticed by astronomers, does appear to me, I confess, not a little extraordinary, and might be taken as an argument to shew, more than any thing, the comparatively neglected state of this highly interesting branch of Physical Astronomy. Perhaps, however, in this, as in many other cases, the very prominence of the object has been the cause of its being neglected; as it might easily be supposed by any one entering on this research, that had a star so familiar to every practical astronomer presented any striking peculiarity of this kind, it could not *but* have been observed. Hence, while the attention of observers has been directed, and with success, to much inferior stars, it seems to have been taken for granted, that among stars of the first magnitude nothing, in fact, remained to be discovered.

"Having bestowed much attention, during my residence at the Cape, on the estimation of the magnitudes of the southern stars, both by direct photometrical measurements, assigning numerical values to about sixty or seventy of them, selected as offering convenient gradations of brightness, and also by very assiduous and often-repeated comparisons by the naked eye, with the view to completing a graduated scale down to the fifth magnitude, at least, it became important to connect these magnitudes by similar comparisons with those of the northern hemisphere, by means of stars in the vicinity of the equator admitting of observation at both stations. My method in these observations has been invariably on each night to establish, in the first instance, a sort of skeleton-scale, beginning with the stars of the first magnitude actually visible, and extending as far as was judged convenient for the occasion, then filling in this scale by the insertion of fresh stars between the members. The stars of the first magnitude actually above the horizon at the time of commencing observation were first arranged, and others of that magnitude inserted among them as they rose and gained altitude.

"On the very clear and brilliant night of the 26th November last, being engaged in a process of this kind, I was surprised, and I may almost say startled, by the extraordinary splendour of *α Orionis*, which far exceeded my idea at the moment of what was

its natural state. Proceeding to compare it with other stars of the first magnitude (*Sirius* being, of course, out of the question), their arrangement for the night was found to be as follows:—

Capella || α Orionis | Rigel || Procyon ||| Aldebaran | Pollux.

"In this and subsequent arrangements of the same sort, the number of vertical strokes between the names indicates the estimated amount of interval, or the *grades* or steps of magnitude by which the stars differ. Thus, the step from *Capella* to *α Orionis* is a great one; that from *α Orionis* to *Rigel*, a distinct but moderate one; from *Rigel* to *Procyon*, a great one, admitting of the easy insertion of a star decidedly inferior to one, and superior to the other, between them; from *Procyon* to *Aldebaran*, a very great step, admitting, at least, two such insertions of imaginary stars decidedly diverse in lustre between them; and so on. Now as I distinctly recollected having, on a great many occasions, placed *α Orionis* nearly on a par with *Aldebaran*, there could be no doubt of a change. Referring next morning to my father's Catalogues of Comparative Brightness, I find that he makes the star in question slightly inferior, or at most equal, to *Procyon*, and much greater than *Aldebaran*.

"In consequence of this observation, I proceeded forthwith to draw out in order all the comparisons of *α Orionis* with other stars made at the Cape, on the voyage homewards, and since my return. In so doing, I must confess I was hardly less surprised than at the sight of the star itself to find in my star-lists, containing the results of a partial reduction and arrangement of my Cape observations, *α Orionis* not merely marked as *variable*, but distinct entries made of it in that list at its maximum and minimum,—the maximum being stated as above *Rigel*, the minimum below *Aldebaran*. This, however, had entirely escaped my memory, but being thus recalled, and so forcibly corroborated, I resolved to watch the star more narrowly in future; the more especially as it seemed to follow, from the tenour of the observations, that its diminution of brightness was likely to be rapid: and so, in fact, it has proved to be."

The author then proceeds to give the observations on which the evidence of the former changes of the star is grounded. They extend over the years 1836, 1837, 1838, and 1839, and are as follows (denoting, for brevity, *α Orionis* by the word *Orion*):—

1836.		
March	22.	Rigel, Procyon, α Crucis, Orion, Regulus, Pollux.
Nov.	12.	$\left\{\begin{matrix}\text{Orion,}\\ \text{Rigel,}\end{matrix}\right\}$ Procyon, Achernar, α Crucis, Aldebaran, Pollux.
	13.	Orion = Rigel.
	26.	Rigel, Orion, Achernar.
1837.		
Oct.	24.	Orion (high), Achernar, Orion (low), Rigel, Aldebaran.
Dec.	16.	Rigel, Achernar, Orion.
	29.	Rigel, Achernar, Procyon, Orion, Aldebaran.
1838.		
Jan.	2.	Rigel, Procyon, Achernar, Orion, Pollux, α Crucis.
	6.	Rigel, Procyon, Achernar, Orion, Aldebaran, α Crucis, Pollux.
	13.	Rigel, Procyon, Achernar, Orion, Aldebaran, α Crucis.
Feb.	25.	Rigel, Procyon, Orion, α Crucis, Pollux, Regulus.
April	14.	Procyon, Rigel, Orion, Aldebaran, Pollux, Regulus.

1839.		
Jan.	17.	Procyon, Aldebaran, Orion, Pollux, Regulus.
	22.	Rigel, Procyon, Aldebaran, Orion, Pollux, Regulus.
Nov.	26.	Orion, Rigel, Procyon, Aldebaran, Pollux.

On examining the above series, the general order of arrangement (leaving out Orion) is found to be *Rigel*, *Procyon*, *Achernar*, *Aldebaran*, *α Crucis*, *Pollux*, *Regulus*; and the instances in which the arrangement is different are accounted for by some peculiar circumstances connected with the observations. Thus, with respect to the observation of October 24, 1837, the author states that the misplacement of *Achernar* is accountable for by the circumstance of the two comparisons of *Orion* having been made (as appears by the notices *high* and *low*) first when rising with *Achernar* then high, and *Rigel* low; and at a later period of the night with *Rigel* then high, and, consequently, *Achernar* low. On January 2, 1838, *α Crucis* is set down as inferior to *Pollux*; but these two stars are difficult of comparison, both from situation and difference in colour, and from being, in fact, not very different in lustre. The transposition of *Procyon* and *Rigel* in the observation of the 14th of April, 1838, is unaccountable, except from some unsuspected partial haziness in that part of the sky. This observation was made at sea.

With regard to *Orion*, the observations evidently shew three maxima, viz. in Nov. 1836, Oct. 1837, and Nov. 1839; and also three minima, viz. those of March 1836, Jan. 1838, and Jan. 1839. "Reasoning from this, the most obvious conclusion is that of an annual, or nearly annual period. But in that case, we must admit the decrease to be comparatively sudden, and the increase slow; whereas, if we admit of a period of about six months, this supposition will not be necessary, and as the star cannot be observed (for this purpose) in the summer months, there is no *primâ facie* reason against adopting the latter period; respecting which, however, further observation will soon enlighten us."

The observations subsequent to Nov. 26, 1839, confirmed the expected decrease of the star in a very decided manner:—

1839.	Nov.	30.	Rigel | Orion, Procyon || Aldebaran.
	Dec.	11.	Rigel | Orion | Procyon || Aldebaran.
		29.	Rigel, Procyon, Orion, Aldebaran.
1840.	Jan.	2.	Rigel || Procyon | Orion || Aldebaran.
		5.	Rigel || Procyon | Orion || Aldebaran.
		6.	Rigel || Procyon || Orion || Aldebaran.

In a note to this last observation, it is stated that "the difference between *Orion* and *Aldebaran* is evidently and rapidly on the decrease. The stars are all high, at nearly equal altitudes, and admirably arranged for comparison."

Jan. 7, 1840. "*Procyon*, *Orion*, *Aldebaran*, form a succession by nearly equal steps."—"Upon the whole, I think it may be stated, that in the interval from November 26 to the present date (January 8), *Orion* has sustained a loss of nearly half its light. It may easily be supposed that a diminution, thus evidently still in

rapid progress, will, in no long time, carry down the rank of this star below that of *Aldebaran*, and that the confirmation or disappointment of this expectation is awaited with no small interest."

The author concludes with the following remarks :—

"The subject of variable and periodical stars has been of late rather unaccountably suffered to lie dormant; a state of neglect in which, as I have already observed, it ought not to be suffered to remain, and from which I have endeavoured to rescue it on two former recent occasions, by pointing out the stars *α Hydræ* and *α Cassiopeiæ*, both large and conspicuous stars, as belonging to the latter class. A periodical change, however, existing to so great an extent in so large and brilliant a star as *α Orionis*, cannot fail to awaken attention to the subject, and to revive the consideration of those speculations respecting the possibility of a change in the lustre of our sun itself which were put forth by my father. If there really be a community of nature between the sun and fixed stars, every proof that we obtain of the extensive prevalence of such periodical changes in those remote bodies, adds to the probability of finding something of the kind nearer home. It is only in comparatively very recent meteorological observations that we can expect to find that precision in the determination of temperatures which is necessary to establish the absence or presence of periodical change in the intensity of solar radiation; and if the period be not annual (as there is no reason why it should be), the usual mode of combining observations of temperature followed by meteorologists is altogether inappropriate to the research, which can only be carried on either analytically, by the introduction of a periodical term with unknown coefficient, epoch, and period, or graphically, by projecting in a continuous curve the *mean daily* temperatures during a long series of years. For the detection of a period of great length, extending over more than a year, the *continued* observation of the temperature of the water a few feet below the surface in open sea, under the equator, on the principles pointed out by M. Arago in his instructions for the voyage of the Bonite, would suffice. But we are far from possessing as yet sufficient records of such observations to be worth discussing in this point of view. Such observations must of their nature be casual. Even granting that in every ship which traversed the equator the requisite observations were made, the identity of their thermometric standards would be still open to question.

"The assiduous observation in fixed physical observatories of the temperature of the earth, at several depths below the surface, extending from three to thirty feet — an element which we know to be (in its mean amount) *solely* dependent on solar radiation — would be in every respect more immediately and practically applicable to the inquiry, and we may expect to see it carried out into effect. The direct measure of the solar radiation too, by the actinometer,* ought by no means to be neglected in this inquiry.

"M. Poisson, in a late *Memoir*, has considered the possible

* "This instrument was devised by me for the dynamical measure of the solar radiation in the spring of 1824; and I have had it in use ever since, with continually increasing confidence in its indications."

consequences in a geological point of view of the sun and solar system having, in long by-gone ages, passed through a region in which the actual *temperature of space* should be much greater than in its present locality. The great authority justly attributed to every idea thrown out by this philosopher must render it a matter of diffidence and difficulty to maintain a contrary view. Without, however, as a matter of abstract speculation, denying this possibility, I would observe that the temperature at any given point of space can arise only from two sources: 1st, That of the ether, as a fluid susceptible of increase and diminution of temperature; and, 2dly, The radiation of the stars. Of the temperature of the ether as a fluid, I confess I have no conception. Of the existence of such a fluid as the efficient cause of *light*, we have demonstrable evidence. But the properties of *heat* are so linked and interwoven with those of light, that it is asking more than can be granted to demand our admission that the ether is a fluid *capable of being heated and cooled*, while it is yet undecided (with a leaning to the affirmative side) whether it be not the *efficient cause* of heat itself.

"As regards the radiation of the stars.—There is a region in the heavens where starlight is decidedly more dense than elsewhere — the milky way. And we have, I may almost say, ocular evidence that our system is excentrically situated within that zone, and nearer to its southern than to its northern portion. Granting a perfect transparency of the celestial spaces, the brightness of any given region of the sky must be alike at all distances, whether we conceive that brightness to be uniformly diffused over its surface or to emanate from a finite number of undistinguishably small points. Now, although the brightness of the southern regions of the milky way may, for argument's sake, be admitted to be three or four times that of the northern, yet, as that light is *almost* completely obliterated by the presence of a full moon in *any* part of the sky above the horizon, it follows that the brightness of *the general firmament* to a spectator placed within the brightest part of the milky way (supposing him not within the range of an individual *sun*), must be less than that of (*not the full moon itself, but*) that general illumination which the moon communicates to the whole sky by atmospheric reflexion; *i. e.* an almost infinitesimal quantity compared to the direct light of the lunar disk, the intensity *of which* can hardly be to that of the sun in a higher ratio than one to half a million.

"The brightest *regions in the sky* — *i. e.* the brightest spaces having a visible area — are those occupied by the planetary nebulæ. Of these, there is none which can be compared to *Uranus* in intrinsic brightness, to say nothing of the moon. Supposing, then, our system to be suddenly plunged into the bosom of one of these nebulæ, an increase of temperature would take place less than that which would arise from superadding to our own that which the surface of *Uranus* receives from the sun, or less than the 400th part of that which we actually receive from it; and this supposes *Uranus* to reflect *all* the light incident on it.

"Leaving to others to judge, however, how far these arguments

are to be considered as militating against the view of climatological changes in remote antiquity above alluded to, I may remark that it is a matter of observed fact, that many stars *have* undergone in past ages, within the records of astronomical history, very extensive changes in apparent lustre, without a change of distance adequate to producing such an effect. If our sun were ever *intrinsically* much brighter than at present, the mean temperature of the surface of our globe would, of course, be proportionally greater. I speak now not of periodical, but of secular changes. But the argument is complicated with the consideration of the possibly imperfect transparency of the celestial spaces, and with the cause of that imperfect transparency, which may be due to material non-luminous particles diffused irregularly in patches analogous to nebulæ, but of greater extent—to *cosmical clouds*, in short — of whose existence we have, I think, some indication in the singular and apparently capricious phenomena of temporary stars, and perhaps in the recent extraordinary sudden increase and hardly less sudden diminution of η *Argus*."

The following elements of the comet now visible, computed by Dr. Petersen, have been received from Professor Schumacher, who remarks that they represent all the observations so well, that any hope of finding an ellipse for this comet must be relinquished:—

Passage, 1840, Jan.	4·5019, mean time at Altona.	
Log. perihelion distance	= 9·791272	
Longitude of perihelion	= 192° 13′ 5″	
Longitude of ☊	= 119 58 7	
Inclination	= 53 5 38.	Motion direct.

From the Berlin observation of Dec. 2, the Hamburg observation of Dec. 14, and the Edinburgh observation of Jan. 5, the following parabolic elements have been computed by Professor Henderson:—

Passage, 1840, Jan.	4·4936, mean time at Edinburgh.	
Log. perihelion distance	= 9·79105	
Longitude of perihelion	= 192° 17′ 58″	
Longitude of ☊	= 119 58 17	
Inclination	= 53 5 36.	Motion direct.

London:—Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

VOL. V. *February* 14, 1840. No. 4.

Report of the Council of the Society to the Twentieth General Annual Meeting held this day.

TWENTY years have now elapsed since this Society was instituted: during which period, the advances in Astronomy, both theoretical and practical, must be evident to the most ordinary observer. For, not only have public and private observatories been multiplied beyond any former example, but a great impulse has, generally, been given to every department of the science. There is now scarcely a single branch of astronomy that is not pursued in detail by one or other of the many active astronomers of the present day; whether requiring the laborious exertions of the observatory, or the equally arduous investigations of the closet. In the construction and manipulation of instruments also, great and important improvements have been made, which have introduced a system of accuracy and minuteness unprecedented in former times. If this Society has not, indeed, been the originator of this happy state of things, it has at least cordially assisted in the impulse: and let us hope that the next twenty years will be distinguished by similar exertions, and crowned with equal success.

Before proceeding to make any allusion to the state of the finances of the Society, the Council regret that they have to announce the resignation of Dr. Lee as Treasurer; a gentleman well known to you all, as one of the most liberal friends of the Society, and one who has most zealously fulfilled the duties of his office. The Council, desirous of expressing their sense of his services, and of his great zeal in the cause of the Society, have proposed him as one of the Vice-Presidents for the ensuing year: a measure which, they trust, will be sanctioned by the present meeting.

The state of the finances of the Society will be best shewn by the following abstract from the Report of the Auditors. From this document it will be seen, that not only have all the claims on the Society been discharged, but that a balance is left in the hands of the Treasurer, more than sufficient to defray the ordinary expenses of the ensuing year.

RECEIPTS.

	£.	s.	d.
Balance of last year's Account	260	7	2
1 year's dividend on £500 Consols	15	0	0
½ year's ditto on £1640 18s. 7d. New 3½ per Cents	28	14	3
½ year's ditto on £1831 6s. 6d. ditto	32	0	11
Sale of Memoirs, per Harrison and Co.	20	9	6
On account of arrears	90	6	0
83 annual contributions (1839–1840)	174	6	0
12 compositions	226	16	0
18 admission fees	37	16	0
11 first year's contributions	14	14	0
	£900	9	10

PAYMENTS.

				£.	s.	d.
Purchase of £253 12s. 4d. New 3½ per Cents				252	0	0
W. Magnay, for 50 reams of paper for Vol. XI.				45	2	0
J. Basire, for engraving Plates 1–6 of Vol. XI.				28	0	0
Moyes and Barclay, for printing Monthly Notices 18–24				20	14	6
J. W. Rumfitt, for book-binding				4	1	6
Harrison and Co. for stationary				8	18	10
Ditto, for commission on collecting				15	2	0
Ditto, for commission on sale of Memoirs				1	0	0
W. Cubitt, for carpenter's work				1	0	6
1 year's salary to Mr. Hartnup				80	0	0
Coals, candles, &c.				11	1	6
Tea, sugar, cakes, &c. for the evening meetings				13	13	0
Conveyance of Memoirs and Greenwich Observations				7	15	5
Carriage of parcels, letters, &c.				5	12	3
Porter's and charwoman's work, &c.				7	18	11
Sundry disbursements by the treasurer				4	15	0
Taxes: Poor's rate	2	2	6			
Taxes: Land tax	3	2	6			
Taxes: Sewer's rate	3	2	6			
Taxes: Church and rector's rate	2	11	0			
				10	18	6
Balance in the Treasurer's hands (January 31, 1840)				382	15	11
				£900	9	10

The assets and present property of the Society may be estimated as follows:—

				£.	s.	d.
Balance in the hands of the Treasurer				382	15	11
Arrears on January 31, 1840.						
2 contributions of six years' standing	£25	4	0			
1 ——— of five ditto	10	10	0			
1 ——— of four ditto	8	8	0			
4 ——— of three ditto	25	4	0			
14 ——— of two ditto	58	16	0			
22 ——— of one ditto	46	4	0			
5 admission fees	10	10	0			
3 first year's contributions	3	3	0			
2 ditto ditto	4	4	0			
1 non-resident become resident	2	2	0			
				194	5	0
£1831 6s. 6d. 3½ per Cent Annuities, £500 3 per Cent Consols, valued at				2300	0	

Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

With respect to the instruments just alluded to, the following changes have taken place. The *Fuller* theodolite has, on the application of the Lords Commissioners of the Admiralty, been sent to the Cape of Good Hope, for the use of Mr. Maclear, in the triangulation which he is about to undertake there, for the purpose of verifying the measurements of Lacaille. The standard scale has been intrusted to the care of Mr. Simms, not only for comparison therewith of some measuring-rods that were lately sent to the same place, and for the same purpose; but also for the construction of a nearly similar standard scale for the Neapolitan government. The *Beaufoy* circle and clock have, at the request of the Society, been returned by the Rev. Mr. Ward; and the clock has been lent to the Royal Society for the use of the Antarctic Expedition, which has recently sailed under the command of Captain James Clark Ross, to whom also the two invariable pendulums have been intrusted. The *Lee* circle has been returned by Capt. Smyth, and is now lent to Mr. Hopkins. The following statement, therefore, will shew the present disposition of the several instruments:

In the apartments of the Society,

The *Harrison* clock; the *Owen* double portable circle; the *Owen* quadruple portable sextant; the *Beaufoy* circle.

In the apartments of the Royal Society, for safe custody,

A brass quadrant, said to be Lacaille's.

The remainder are lent, during pleasure, to the several parties undermentioned, viz.:

The *Fuller* Theodolite, to the Lords of the Admiralty.
The Standard scale, to Mr. Simms.
The *Beaufoy* clock, } to the Royal Society.
The two invariable pendulums, }
The *Beaufoy* transit, to Mr. T. Jones.
The other *Beaufoy* clock, to Col. Pasley, R.E.
The *Lee* circle, to Mr. Hopkins.
The *Wollaston* telescope, to Professor Schumacher.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year, viz.

	Compounded.	Annual Contributors.	Non-resident.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1839	74	137	91	6	308	37	345
Since elected	6	12	...	...	18	1	19
Deceased, &c..........	−1	−4	...	−1	−6	...	−6
Resigned	...	−8	...	...	−8	...	−8
Removals	6	−4	−2	...	...	...	...
February 1840	85	133	89	5	312	38	350

Amongst the losses by death, during the past year, the Council have to announce the decease of his Majesty the King of Denmark, one of the honorary members of the Society, and a great patron of the science of Astronomy. In the year 1832, his Majesty founded a gold medal, for the first discovery of a telescopic comet not previously known; and in November 1835, the conditions on which the medal was to be awarded were read at the meeting of this Society, and published in the *Monthly Notices*. It is somewhat remarkable, that from that period till within a few hours of his Majesty's decease, when the recent comet was observed by M. Galle at Berlin, no discovery of the kind had been made; although, some years previously, a considerable number of comets had made their appearance: and his Majesty's liberal intention had, consequently, been nearly frustrated. It will be gratifying to the Society to know that his Majesty's successor, the present King of Denmark, is also a patron of our science; and it is, therefore, with much pleasure, that the Council propose that he be elected an honorary member, in the place of his late predecessor.*

The other losses that the Council have to deplore, are those of Mr. Davies Gilbert, Professor Rigaud, and Mr. Epps.

Mr. Davies Gilbert was born on the 6th of March, 1767, in the parish of St. Erth, in the west of Cornwall. His father was the Rev. Edward Giddy, his mother Catherine Davies, a descendant of William Noye, attorney-general in the reign of Charles the First. The subject of this memoir was reared with great care and attention as a child of promise but not of robust health. His early education was conducted at home by his father, who was well qualified for the task as a scholar, and as a man of acknowledged ability and attainments; but his pupil's taste soon led him to prefer the study of the severer sciences to the elegancies of classical literature; and these studies were pursued with an ardour not to be repressed. As he grew up it was thought expedient to place him in the grammar-school of Penzance (of which the Rev. James Parkin was then the master); and for that purpose, his parents removed, for about eighteen months, to that town. In the year 1782 they removed to Bristol, where their son's studies were assisted, for a time, by Mr. Benjamin Donne. In the year 1786 he was matriculated at Oxford, after having been entered as a gentleman-commoner of Pembroke College. He had already made himself master of considerable mathematical and physical knowledge, and had acquired the greater portion of it by almost unassisted application. His efforts had been guided and aided, indeed, by a very friendly intercourse with the Rev. Malachy Hitchins, vicar of St. Hilary (whose connexion with the late Dr. Maskelyne and the *Nautical Almanac* is well known), from whom, as he has himself recorded, he obtained, whenever it was asked, information " in those sciences which afforded him uninterrupted entertainment and delight throughout the whole continuance of a protracted life."

* A resolution to this effect was proposed at the meeting, and unanimously carried.

During his residence at Oxford, he was a regular attendant at the lectures on anatomy and mineralogy of Dr. Thomson, at Christ Church. He likewise attended with assiduity the lectures on chemistry and botany of Drs. Beddoes and Sibthorp, and formed with them alliances of reciprocal friendship, which terminated only with their lives. His society was, in fact, courted by his seniors, and included a long list of the most eminent among the professors and other distinguished men of the University.

He took the honorary degree of M.A., and continued to reside much in his college, until, in 1793, he returned to Cornwall to serve the office of sheriff, where his time was divided between the cultivation of science and literature, and the duties of a magistrate in a populous and busy county.

Soon after this, we find him brought into closer contact with the world, and enabled to display, upon the wider stage of the metropolis, those powers of mind which had been hitherto confined to the narrower sphere of his native county, and a few private admirers and friends. He represented Helston in parliament in the year 1804; and at the general election in 1806, he was chosen to represent Bodmin, and continued to sit for that borough, till, in December 1832, he ceased to be a member of the legislative body.

On the 8th of April, 1808, he married Mary Ann Gilbert, only niece of Charles Gilbert of Eastbourn, in Sussex, under whose will he came into possession of considerable estates in that county; and, in compliance with its injunctions, assumed the name and arms of Gilbert.

Mr. Gilbert's exertions to promote the objects of science in general, and to forward the views of the different scientific Societies of London and other parts of the United Kingdom, with which he was connected, both in the House of Commons and with the leading men of the state, are well known, and will be long held in remembrance. He had been early admitted into the Royal Society, of which he never ceased to be an active and distinguished member; and, in 1827, was elected, without opposition, their President, on the resignation of Sir Humphry Davy, whose friend and patron he had been in the early life of that illustrious chemist and philosopher. He resigned that chair in November 1830, and was succeeded in it by H.R.H. the Duke of Sussex. He filled the office of President of the Royal Geological Society of Cornwall, from its origin till his death.

Mr. Gilbert, as a member of the legislature, and a distinguished cultivator of science, appeared to government a proper assistant in all public inquiries for which station in society, and a competent knowledge of mathematics, formed necessary or useful qualifications; and he was accordingly appointed a member of the Board of Longitude, a Commissioner of Weights and Measures, and a Commissioner to settle the Boundaries of Boroughs under the first Reform Bill. He was also Chairman of the Committee appointed to consider the subject of the Measurement of the Tonnage of Shipping; whose plan (proposed by one of our Fellows, Mr.

Riddle) was passed into a law. He was, indeed, at all times willing, when called upon, to devote his time and acquirements either to the public service of his country, or to the private aid of those who appealed to him for assistance or advice.

After his retirement from Parliament and the Chair of the Royal Society, he did not resign himself entirely to repose; but continued, notwithstanding his increasing infirmities of body, actively to co-operate with his fellow-labourers in the great field of science. He accepted a commission of investigation into the Stannary Laws of Cornwall, and frequented the Societies of which he was a member with undiminished alacrity and zeal, always prepared to obey the calls of duty or friendship with readiness of purpose and gentleness of manners.

Mr. Davies Gilbert was not a very large contributor to the *Philosophical Transactions*. In 1826, he gave a paper on Suspension Bridges, accompanied by tables, which is the most complete account yet published of the points on which it treats. In 1827, he read a paper on the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; and in 1830, a description of the progressive improvements made in Cornish steam-engines. Very different from these, in the subject treated, is a paper on the nature of negative and imaginary quantities, read in 1830. His last contribution, a continuation of the tables relative to suspension bridges, was made on May 19, 1831.

He had for some years exhibited symptoms of decay. What were the causes of the atrophy so visible in his form and countenance is unknown; but in 1839, he became weaker in strength and spirits; and though he made a journey to Durham, and afterwards into Cornwall, where he presided for the last time at the Anniversary of the Royal Geological Society of Penzance, and attended that of the Polytechnic Society of Falmouth, he was evidently unequal to the task he had imposed upon himself, though his powers of mind were clear, quick, and apparently unimpaired. His last visit was to Oxford, which had, some years before, conferred on him one of the highest titles in her power to bestow. From that period he never went into public, but took his last journey from London to his house at Eastbourn, on the 7th November. He expired in the presence of his family, on the 24th of December, 1839; and his memory will be long cherished as that of an ardent lover of science, a liberal patron of merit, a kind friend, a mild and accomplished gentleman.

Mr. Rigaud was descended from a French family of consideration, who, on the revocation of the Edict of Nantes, resigned their property, and fled to a foreign land for conscience sake. His maternal grandfather and father had each successively filled the office of Observer to the King, at Kew; an office which was afterwards graciously conferred on Mr. Rigaud himself. Mr. Rigaud was born in the year 1774; and in 1791, when little more than sixteen years of age, was matriculated at Exeter College,

Oxford. He was elected a Fellow of that Society before he was of sufficient standing to take the degree of B.A.; and, as soon as his age permitted, was appointed one of the College tutors. On the death of Dr. Hornsby, in 1810, Mr. Rigaud was appointed Savilian Professor of Geometry, and Reader on Experimental Philosophy; the duties of which latter office he had for some time before been discharging for Dr. Hornsby. In 1827, Mr. Rigaud succeeded Dr. Robertson in the office of Savilian Professor of Astronomy and Radcliffe Observer, thereby relinquishing the Professorship of Geometry. As an astronomer, Mr. Rigaud's attention was principally directed to the literary and historical department of the science, for which his extensive learning, and accuracy of research, especially qualified him. In 1831, he published, in a 4to. volume, "The Miscellaneous Works and Correspondence of the Rev. James Bradley," to which he prefixed copious memoirs of Bradley himself. This work contains, besides several other unedited papers of Bradley, his original observations for the determination of the constants of aberration and nutation. These observations had been lost sight of for upwards of seventy years; and, but for Mr. Rigaud's zeal and exertion, would, in all probability, never have been recovered. Fortunately, however, it came to his knowledge that several MSS. of Bradley still existed among the papers of Dr. Hornsby, then in the possession of his family, by whom they were readily given up, on application from the University. Mr. Rigaud was requested to undertake the office of editor: a task which was not an easy one, owing to the confused state of the materials from which he had to derive his information. He succeeded, however, in producing a work which will ever be regarded as a most valuable record in the history of our science. So highly was it esteemed on the Continent, that in the very next year after its publication, the Royal Academy of Sciences of Copenhagen made the reduction of the observations for aberration and nutation the subject of their prize; which was adjudged to Dr. Busch of the Konigsberg Observatory, the assistant of that illustrious astronomer to whom the reputation of Bradley is so deeply indebted. As connected with this subject, it may be mentioned, that it was through the instrumentality of Mr. Rigaud, that his late Majesty, King William IV., was pleased to cause a monument to be erected at Kew, to mark the spot where Bradley made the observations which led to his great discoveries. In 1835, Mr. Rigaud published a small pamphlet, containing an account of the "Astronomiæ Cometicæ Synopsis" of Halley; and, in 1838, the last work he lived to complete, "An Historical Essay on the first publication of Sir Isaac Newton's Principia;" which exhibits, in every page, the author's minute acquaintance with the events of that important period. Besides these works, Mr. Rigaud was the author of many papers read before the Ashmolean Society in Oxford; and also of one "On the Principal Instruments at Greenwich in the time of Dr. Halley," which is inserted in the ninth volume of our *Memoirs*.

In 1827, Mr. Rigaud met with a severe domestic affliction in the loss of his wife; an event which left him sole guardian of a large family of young children; to the superintendence of whose education much of the attention of the latter years of his life was devoted. The affection and solicitude with which he discharged this duty was rewarded by his being spared to witness the academic distinction of his eldest son, who is now a Fellow of our Society; and the Council are happy in being able to state that he is completing the publication of a collection of letters from scientific men in the beginning of the last century, upon which his father was engaged at the time of his death; and the original documents of which formerly belonged to Mr. Jones, the father of Sir William Jones, but now in the possession of the Earl of Macclesfield.

Many members of this Society have had opportunities of observing the kindness and unaffected simplicity of manner which marked Mr. Rigaud's intercourse in private life; and some of them, the more sterling qualities of his character. For many years past, however, he had entered but little into society. His almost constant residence, for nearly half a century, was Oxford: and there he has left a large circle of friends, who had abundant opportunities of knowing his virtues, and who will long regret his removal. In him the University has lost a most devoted son; and it is now a consolation to remember that he was ever foremost among those whom she delighted to honour.

Mr. James Epps was appointed Assistant-Secretary of this Society in 1830, and during the eight years he officiated in that capacity, he not only merited the approbation of the Council by the ability and zeal with which he discharged the duties of his office, but also rendered himself acceptable to the Fellows at large by his uniform urbanity, his cheerful disposition, and his readiness to oblige on all occasions. Although he had not the advantages of a regular education, and the occupations of his early life left but little leisure for the cultivation of the sciences, he had acquired, nevertheless, very considerable knowledge both of theoretical and practical astronomy; and he had also much skill and experience in astronomical computation. He was the author of several papers printed in our *Memoirs*; namely, one in the fourth volume, accompanied by some useful tables for computing the azimuthal deviations of a transit instrument from the observed passages of two stars through the vertical it describes; another, in the same volume, on the errors of the same instrument occasioned by the inclination of the axis to the horizon; one, in the sixth volume, on the method of ascertaining the comparative rates of chronometers; and one, in the ninth volume, on the investigation of formulæ for reducing observations made with the annular micrometer. He likewise recently contributed another paper on the errors that may be produced in determining differences of longitude by observations of moon-culminating stars, when there are no corresponding observations; accompanied by a table of results deduced from comparisons of such observations, with the places given in the *Nautical Almanac*,

which has been ordered by the Council to be printed in the forthcoming volume.

In 1838, Mr. Epps resigned his office in the Society, and removed to Hartwell to superintend the private observatory of our excellent Treasurer, Dr. Lee. For this appointment he was eminently well qualified. He entered on its duties with his usual ardour; thus meriting the friendship and esteem of his patron, which he continued to enjoy without interruption to the hour of his death. On his removal to Hartwell, he was elected a Fellow of this Society.

Mr. Epps was a man of varied accomplishments and extensive general information; and knew well how to turn theoretical knowledge to practical account. He was born in 1773, of humble but respectable parents residing in Kent; and died at Hartwell on the 10th of August last, regretted by all who knew him.

The Council have to regret the resignation of Captain Smyth as Foreign Secretary; a gentleman well known to you all for his varied acquirements, and for his great zeal in the cause of science in general, and more especially that branch of it which is more immediately connected with this Society, and which he has so successfully cultivated. Capt. Smyth's occupations in a more distant part of the country will prevent his continuing in the office, which he has so admirably filled: but it is to be hoped that he will occasionally assist us with his counsel and advice.

It is well known to members frequenting the meetings of this Society, that the names of three Fellows have, for the last three years, been suspended in the meeting-room as defaulters: and although notices have been regularly issued to them, agreeably to the by-law, Sect. v. § 6, the arrears have not yet been paid. The Council therefore have considered it their duty (as on a former occasion in the year 1836) to call a special general meeting, for the purpose of removing or expelling those Fellows. This special general meeting will take place after the termination of the present annual general meeting.

In the Report of the Council to the Society in the year 1838, it was stated that the pendulum observations made by the late lamented Lieut. Murphy in Asia, had been received, and placed in the hands of Mr. Baily, for examination and reduction. Since that period, the pendulums themselves have arrived, and Mr. Baily has repeated his experiments on them, for the purpose of comparing the results before and after the voyage, with those made by Lieut. Murphy. A Report on the whole of these experiments, and on their general result, will be made by Mr. Baily, and read to the Society at one of the evening meetings. These pendulums are now intrusted to Capt. James Clark Ross, as already mentioned, for the purpose of making further experiments at such places as he may find it convenient, during his present scientific voyage: and they are thus, for the third time, placed in active operation.

As connected with this subject, it may be mentioned that when Mr. Maclear departed for the Cape of Good Hope, to take the

superintendence of the Observatory there, he took with him one of Kater's invariable pendulums, that had been previously swung in this country by Mr. Baily. That pendulum has recently been returned to this country, together with a detail of Mr. Maclear's experiments. The whole have been placed in the hands of Mr. Baily, who will, in this case also, report upon the general result.

In alluding to the labours of Mr. Maclear at the Cape of Good Hope, the Council may mention also his intention of remeasuring the arc of meridian formerly measured by Lacaille. Already have the stations of Lacaille been satisfactorily identified, and the latitudes of the extreme stations been observed, as we have seen by the paper recently read to this Society: and the requisite apparatus has been sent out for finishing what has been so auspiciously begun. Under the direction of this able and zealous astronomer, there is every reason to expect a satisfactory result to so important an operation.

The fourth volume of the *Monthly Notices* closed with the account of the proceedings at the meeting in June last. The fifth volume commenced with the present session. As these detached papers are distributed only to Fellows of the Society, and cannot now be purchased, only a limited number of copies being printed for distribution, it is requisite that those Fellows, who are desirous of preserving them, should endeavour to prevent their being mislaid or lost.

The Council have great pleasure in stating that the eleventh volume of the *Memoirs* of the Society is now in the press, and that considerable progress has been made in the printing. Amongst the papers that will appear therein, is a very valuable catalogue of all the stars that were observed by Mr. Airy during the time that he had the superintendence of the Observatory at Cambridge. Partial lists of those stars had from time to time been printed in the several volumes which annually proceeded from that Observatory; subject however to a slight correction for reducing them to one and the same equinox. In the catalogue about to appear in our *Memoirs*, they are all uniformly reduced to the epoch 1830, with the annual precessions annexed; and are thus immediately available for occasional reference and application. This catalogue will be found to fill up many *lacunæ*, and tend to rectify many errors in former catalogues, arising either from imperfect observation, or from mistakes in transcribing or computing authentic records. It has been found of great assistance in perfecting and enlarging the catalogue which goes under the name of the Catalogue of this Society; as it contains several stars that had not been observed since the original observations by Hevelius, Flamsteed, and Bradley. With a view to extend its utility still further, the Council have directed that an additional number of copies should be printed, and presented to the Rev. Mr. Challis, the director of the Cambridge Observatory, with a request that they might be distributed with the forthcoming volume of the *Cambridge Observations*: a request with which he has readily complied.

It is also with much pleasure that the Council can state that the extension of the Society's *Catalogue of Stars*, just alluded to, and which has been undertaken at the suggestion and at the expense of the British Association, is in great progress, and will probably be completed before the next anniversary. It is intended that this enlarged catalogue shall contain not only every known star in the catalogues of Hevelius, Flamsteed, Bradley, Mayer, Lacaille, and Zach, but also every star, in any of the more modern catalogues, of the sixth magnitude, in whatever part of the heavens it may be situated, and every star in such catalogues not less than the seventh magnitude, within 10° of the ecliptic; together with every other star that, from its peculiar position, suspected proper motion, or other extraordinary circumstance, may be deserving of being thus recorded, and pointed out for further observation.

In the execution of this work, considerable assistance has been afforded by the four several catalogues published by Mr. Thomas Glanville Taylor, at Madras. The second of those catalogues contains nearly all the stars in the Society's catalogue, visible in that latitude; and the last two exhaust nearly the whole of Piazzi's celebrated catalogue. The total number of stars contained in these four volumes is upwards of 8800; most of which have been observed more than once, and many of them more than five times. The whole have been of essential advantage in completing and perfecting the extension of the Society's catalogue above mentioned: since it has enabled the computers not only to verify the positions of nearly all the stars, but also, in most cases, to deduce the proper motion (if any) that belongs to each of them respectively. The establishment of this observatory is highly honourable to the East India Company, and the fruits which it has produced reflect great credit on the zeal and assiduity of Mr. Taylor, the active superintendent.

Another subject also undertaken by the British Association, is the reduction of the stars in the *Histoire Céleste* (a work containing about 50,000 observations), together with the annual precession annexed to each star. About one-half of this work is already executed; and, when completed, it will afford a ready and convenient reference to almost all the stars (not circumpolar) that are visible in this latitude with an ordinary telescope. The positions of the stars are reduced to the epoch 1800, by means of the very convenient tables of M. Schumacher; a work which renders it scarcely necessary that the computations should be done in duplicate; for, as every star will be referred to its original authority in the printed work, the astronomer will have an easy and ready mode of verifying any suspected result, and of rectifying any error that may be discovered.

To the British Association also, astronomers are indebted for another work of a similar kind: namely, the reduction of *all* the observations of stars made by Lacaille at the Cape of Good Hope. It is well known that only 1942 of those stars were reduced by Lacaille himself, and formed into a catalogue, which is printed at

the end of his *Cœlum Australe Stelliferum:* but the great mass of his observations, consisting of upwards of 10,000, have never yet been reduced, although they are of equal authority with those in the published catalogue. The execution of this work is proceeding under the direction of Professor Henderson of Edinburgh, who has been kind enough to supply the elements for the reduction, and to superintend the process of the computations: and there is every reason to believe that Lacaille's new and enlarged catalogue will, as far as the stars in the old catalogue are concerned, be more entitled to confidence (since it is founded on more accurate elements) than the original catalogue published by Lacaille himself. It will, moreover, contain a great number of other stars of equal authority, unknown to astronomers till the appearance of the recent catalogue of Sir Thomas Brisbane.

Connected with this subject is another work of great interest, likewise proposed by the British Association: namely, a revision of the nomenclature of the stars, and of their division into constellations. It is well known that much confusion at present exists in the notation that has gradually crept into practice: a notation now without order, system, regularity, or uniformity. Hevelius was the first to break in upon the arrangement of the constellations as propounded by Ptolemy: and Flamsteed (or his editors), although he did not disturb the order or number of the constellations in the catalogue of Hevelius, yet introduced some confusion by inserting stars in one constellation that had previously been considered as belonging to another: Bayer's mode of indicating the relative magnitudes of the stars in each constellation was in a great measure lost sight of; and thus the way was led for that mass of confusion which is contained in Bode's large catalogue of 17,240 stars. In the southern hemisphere, we find Halley and Lacaille introducing totally new constellations, frequently overlapping each other, and the stars themselves indicated by such a profusion of letters (many of them precisely similar and several times repeated in the same constellation), that it is often difficult and not always possible to identify them. With the view of applying a remedy to this species of scientific annoyance, the British Association has appointed a Committee, and placed funds at their disposal, for a new arrangement and classification of the stars; preserving as much as possible the old constellations, and Flamsteed's system of numerical order: but correcting gross errors in such arrangement, and confining the adopted constellations to known and definite limits. The present time appears peculiarly favourable for such an undertaking, when so many catalogues are about to be formed into one uniform system. Those only, who have had much experience in such matters, can fairly estimate the convenience and advantage to be gained by such a reform. It is to be hoped that one of the leading members of the Committee will favour this Society with his views and proposals on this subject, in order that it may have the opportunity and benefit of a free discussion, prior to the adoption of so important an alteration.

From amongst the several distinguished names that were proposed for the Medal this year, the Council have selected that of M. Plana, for his elaborate treatise entitled *Théorie du Mouvement de la Lune:* and they trust that this award will meet the approbation of the meeting. The medal will be delivered in the usual manner at the close of the meeting; and the President will, in his address, explain the grounds on which the Council have formed their decision.

The recent alterations in the system of the Post Office have, in a few instances, caused inconvenience and dissatisfaction to some of the Fellows, by reason of the usual mode of distributing the *Monthly Notices* having been continued; whereby they have been charged with double postage. The Council however, in order to remove every objection of this kind, have directed that, in future, all letters and notices, transmitted through the Post Office on the business of the Society, shall be pre-paid.

In conclusion, the Council trust the same unanimity and zeal which have hitherto distinguished the Society will continue to prevail: and that in each succeeding year they may have to congratulate the members on an increase of activity and force. Much is now doing, and much is yet to be done. Let us then not cease from our united and individual exertions, but assist, as much as in us lies, to promote the cause which is the immediate object of our union.

Titles of Papers read before the Society, between February 1839 *and February* 1840.

1839.

Mar. 8. Observed Transits of the Moon and Moon-Culminating Stars over the Meridian of Edinburgh Observatory, from June 1 to December 31, 1838. By Professor Henderson.

Lunar Occultations of Planets and Fixed Stars, and Eclipses of *Jupiter's* Satellites, observed at Edinburgh Observatory in 1838. By Professor Henderson.

Moon-Culminating Stars observed at Cambridge Observatory in the Months of November and December, 1838. By Professor Challis.

Occultations observed at Dulwich and Ashurst from July 31 to December 27, 1838. By R. Snow, Esq.

On the Method of determining the Longitude by Moon-Culminating Stars. By Mr. Epps.

April 12. On the Position of Lacaille's Stations at the Cape of Good Hope. By Thomas Maclear, Esq., Astronomer-Royal at the Cape.

May 10. Occultations of the *Pleiades* by the Moon, observed at Ashurst, March 19, 1839. By Robert Snow, Esq.

May 10. Occultations of the *Pleiades*, observed at the Royal Naval Asylum, Greenwich, March 19, 1839. By the Rev. George Fisher, A.M.

On the Suspected Variability of the Star α *Cassiopeiæ*. (Extract of a Letter from the President, Sir John F. W. Herschel, to Mr. Baily, dated Slough, April 28, 1839.)

Extract of a Letter from M. Gautier, Director of the Observatory at Geneva, to the President, accompanied with the Observations of Encke's Comet, made at Geneva in the Months of October and November, 1838.

On Ptolemy's Catalogue of Stars. By Francis Baily, Esq., Vice-President of the Society.

June 14. On the Method of Determining the Longitude by Moon-Culminating Stars. By Mr. Epps.

On the Optical Glass of the late Dr. Ritchie. By Mr. Simms.

On the Determination of the Longitudes of the Observatories of Edinburgh and Makerston, by means of Chronometers. By Mr. Dent.

Some Account of the progress of the Trigonometrical Survey now carrying on in India. Extracted from the Correspondence of Colonel Everest, and Communicated by Major T. B. Jervis.

Nov. 8. On the Determination of the Orbits of Comets, from Observations. By G. B. Airy, Esq., Astronomer-Royal.

Extract of a Letter from Professor Schumacher to the Astronomer-Royal, relative to the Determination of Differences of Longitude, by observations of Shooting Stars.

Dec. 14. On the Parallax of *Sirius*. By Thomas Henderson, Esq., Astronomer-Royal for Scotland.

A Catalogue of Twenty-seven Stars of the *Pleiades*. By M. Bessel, Director of the Observatory of Konigsberg.

A Letter from M. Valz, Director of the Observatory at Marseilles, to the President, Sir John F. W. Herschel, Bart., relative to the Variation of the Apparent Diameter of Encke's Comet.

A Letter from Professor Schumacher to Francis Baily, Esq., announcing the Discovery of a Comet by M. Galle, Assistant in the Berlin Observatory.

Tables for the Calculation of Precession for the year 1825, of Stars observed by M. Bessel, in the several Zones from $-15°$ to $+15°$ declination. By Dr. Max. Weisse, Director of the Observatory at Cracow.

Observations of the Moon and Moon-Culminating Stars, Eclipses of *Jupiter's* Satellites, and Occultation of Fixed Stars by the Moon, made at the Observatory at

Paramatta, in New South Wales, in the year 1838. By Mr. Dunlop. Communicated by Sir T. M. Brisbane, Bart.

1840.
Jan. 10. Ephemeris of the Comet now visible. By Mr. C. Rumker, of Hamburgh. Communicated by Dr. Lee.

A Letter from Mr. Henry Lawson to the Secretary, describing the Appearance of the Comet, as seen at Hereford.

Apparent Positions of the Comet, observed at Edinburgh. By Professor Henderson.

Observations of the Comet made at Ashurst and Dulwich. By Robert Snow, Esq.

Occultations of Stars by the Moon, to the end of 1839. By Robert Snow, Esq.

Catalogue of the *Pleiades*. By Robert Snow, Esq.

On the Variability of *α Cassiopeiæ*. By Robert Snow, Esq.

Observations of *α Cassiopeiæ*, in 1831 and 1832. By Mr. Birt. Communicated by the President.

On the Variability and Periodic Nature of the Star *α Orionis*. By Sir John F. W. Herschel, Bart., President.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Lords Commissioners of the Admiralty.
American Philosophical Society.
Society of Arts.
Royal Asiatic Society.
Asiatic Society of Bengal.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences at Brussels.
Cambridge Philosophical Society.
Institution of Civil Engineers.
L'Académie Royale des Sciences de l'Institut de France.
Royal Geographical Society.
Geological Society of London.
Society of Geneva.
Royal Irish Academy.
Linnean Society of London.
Royal Society of London.
Numismatic Society.
Society for the Diffusion of Useful Knowledge.
Botanical Society.
Royal Observatory of Munich.
Royal Society of Edinburgh.
Dépôt de la Marine de France.
Executors of the late Dr. Bowditch.

Hon. East India Company.
Meteorological Society.
Zoological Society.

G. B. Airy, Esq. Ast. Roy.
Capt. M. A. Bérard.
Professor Bessel.
Capt. J. T. Boileau.
William Bollaert, Esq.
John Caldecott, Esq.
Professor Challis.
A. M. R. Chazallon.
Augustus de Morgan, Esq.
Professor Encke.
Thomas Forster, Esq.
Lieut. Frome.
Thomas Galloway, Esq.
James O. Halliwell, Esq.
John Herapath, Esq.
Major T. B. Jervis.
Professor Kupffer.
Dr. J. Lamont.
Professor Littrow.
John William Lubbock, Esq.
Professor Quetelet.
Major E. Sabine.
Professor Schumacher.
James J. Sylvester, Esq.
Lieut. Stratford.
Richard Taylor, Esq.
John Vaughan, Esq.
Colonel Visconti.

The President (Sir J. F. W. Herschel, Bart.) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council to which we have just listened (with the painful exception of the losses the Society has sustained in the persons of those respected and lamented members who have been so ably commemorated in that Report) is one calculated to afford most lively satisfaction; and it is so full and complete on every point as to have left me nothing to say, except on that one subject on which, by ancient usage, it has been considered right for the Chairman of this meeting to add a few words of explanation—I mean the award of the Medal for the year.

The award of our medal for this year, gentlemen, to Signor Plana is an act, as it may at first sight appear, of somewhat tardy justice. Those great works on the lunar theory (for which that award is made), and on the perturbations of the planets, especially of *Jupiter* and *Saturn*, have now been so long before the public, that it may almost appear as if, in the dearth of matter of sufficient interest of later date, your Council had been ransacking the annals of modern astronomy to find something on which they might rely in a kind of inglorious safety for a justification of their award.

This would be a very erroneous view, indeed, to take of this subject. So far from experiencing a lack of matter to choose from—so far from a deficiency of interest in the subjects which have shared the consideration of the Council in coming to the conclusion they have done—there have been, in fact, on probably no occasion, such powerful countervailing claims—and so far from seeking, in this award, a merely safe and justifiable course—it has required no common share of boldness and decision in your judges to put aside those claims, in favour of M. Plana's — of that

boldness I mean which is based on justice and a long-sighted view of public utility.

Before I proceed, therefore, to state the reasons which have weighed with the Council to take the step they have done, it will be right for me to mention, at least in general terms, two of the subjects which have chiefly divided their attention on the occasion; and this I am fortunately enabled to do, infinitely better than I could pretend to do it on my own knowledge and reading, by the aid of most excellent reports on those subjects laid before the Council by Professor Airy and Mr. Main—the one on the subject of Professor Hansen's general researches in physical astronomy, the other on Professor Bessel's and Mr. Henderson's observations on the parallax of those remarkable double stars, 61 *Cygni* and *α Centauri*—observations which it would appear, beyond question, have brought us to the very threshold of that long-sought portal which is to open to us a measurable pathway into regions where the wings of fancy have hitherto been overborne by the weight or baffled by the vagueness of the illimitable and the infinite.

M. Hansen's researches on the lunar and planetary theories are every way most remarkable, and seem likely to lead to results of the utmost generality and importance. He has attacked the great problem of three bodies (extended, in the conception and application of his methods, to the mutual perturbations of *four*) by a method entirely novel in its idea, although based on and starting from Lagrange's idea of the variation of the elements. Of this method, it would not be easy, in words unaided by symbolic expression, to give any distinct account; but its principle may be stated in general terms, as assuming not the elliptic *elements*, but the elliptic *time*, to be subject to perturbation; or, in other words, as considering the perturbed co-ordinates, each to arise from the combination of invariable *elements* with a varied or perturbed *time*, the amount of whose variation shall exactly account for all that the variation of the elements accounts for in Lagrange's method. The mere mention of this refined and abstruse mode of conceiving the problem must suffice to shew, that, to carry it into effect, must require at every step a contention of mind, a degree of intellectual effort, far surpassing what is required for the mere management of algebraic symbols and developements, however intricate.

Whatever be the skill and dexterity, however, exhibited by the author of this truly original conception, and whatever promise it must be considered as holding out for the future advancement of our knowledge in this intricate research, it can hardly yet be regarded as having attained that extent of developement which it will require to supersede in the construction of tables, and the actual calculation of the lunar and planetary perturbations, the methods already in use, which the researches of Clairaut, Laplace, Lagrange, Poisson, Damoiseau, and Plana, have wrought up to such a pitch of practical perfection. Hansen's theory appears to afford what, in the actual state of our knowledge, must be regarded as most precious—a new handle by which to seize this refractory problem—

one of universal applicability and gigantic power and *purchase*, but of which the management is not yet fully reduced to practice, and of which even the author himself can scarcely yet be said to have acquired the entire mastery. In the theory of *Jupiter* and *Saturn*, indeed, the final numerical results are obtained, and tables calculated; but in the lunar theory, which (in the words of Mr. Airy) "must be considered as the ground of his chief analytical triumph, there exist at present only what may be termed the *foundations* for such a theory." "No man living" (I continue to use the words of the eminent geometer last mentioned), "No man living, probably, except M. Hansen himself, could work it into a complete lunar theory; and the exhibition of numerical results is here, therefore, still distant." Let us hope that he will not long leave them so.

On the other subject to which I alluded—the parallax of the fixed stars—it would be doing an injustice to the valuable report of Mr. Main, which, as a beautiful specimen of astronomical history, I hope to see ere long adorning our *Transactions*, if I were to avail myself more largely of it on this occasion than is absolutely necessary. It has long been understood by astronomers, that the research of parallax ought not to be confined to the largest stars, but that, in order to determine our choice of stars for this research, other *primâ facie* grounds for suspecting a proximity to our system ought to be taken into consideration; such as great proper motion, or, in the case of a double star, great apparent dimensions of the orbits described about each other. In the case of the double star 61 *Cygni*, both these indications combine to point it out as deserving inquiry. In that of *α Centauri* they also conspire; for it is well known that this fine double star has a considerable proper motion: and my own observations prove, that the mutual orbit described by its individuals about each other, is of unusually large angular dimension. The great brilliancy of the star also, and its situation in a region of the heavens in which the stars, generally speaking, seem to be less remote than in others, all favour the expectation of a measurable parallax being detected in it: and such Mr. Henderson, from his own observations, assigns to it. I am not about to criticise this result; on the contrary, I am disposed to attribute much weight to his conclusion; but it is only on a very long series of observations of *absolute places*, affected as they are by instrumental error and uncertainty of refraction, that any conclusion of this kind can rest with security.

Bessel has attacked the question in a different way, by measuring at all times of the year the angular distance of the stars composing the double star 61 *Cygni* from two small stars visible in the same field of view, and within limits adapted to secure micrometrical measurement. The method is unexceptionable, the measurements conducted with consummate skill, and their reduction executed with all possible regard to every thing likely to influence the result. And that result is, to assign a minute, it is true, but perfectly unequivocal amount of parallax, in

a way so striking as hardly to allow a doubt of its reality. Such is the impression on merely reading the numerical statement; but, put in the light in which Mr. Main has placed it, by the graphical projection of the measures, the conclusion seems quite irresistible:

> "Segnius irritant animos demissa per aures,
> Quam quæ sunt oculis subjecta fidelibus atque,
> Ipse sibi tradit spectator."

It may now be reasonably asked, If all this be so, why have your Council hesitated to mark this grand discovery with that distinct stamp of their conviction and applause, which the award of their annual medal would confer? A problem of this difficulty and importance solved, so long the cynosure of every astronomer's wishes—the ultimate test of every observer's accuracy—the great landmark and *ne plus ultra* of our progress, thus at once rooted up and cast aside, as it were, by a *tour de force*, ought surely to have commanded all suffrages. It is understood, however, that we have not yet all M. Bessel's observations before us. There is a second series, equally unequivocal (as we are given to understand) in the tenour, and leading to almost exactly the same numerical value of the parallax, and not yet communicated to the public. Under these circumstances, it became the duty of your Council to suspend their decision. But, should the evidence finally placed before them at a future opportunity justify their coming to such a conclusion, it must not be doubted that they will seize with gladness the occasion to crown, with such laurels as they have it in their power to extend, the greatest triumph of modern practical astronomy.

M. Plana is well known to the astronomical world as the director of the Observatory at Turin, from which have emanated some valuable series of observations. In conjunction with M. Carlini, he also carried on that extensive and important triangulation of the Savoy Alps, which have made his name celebrated as a geodesist. His works, too, on many other subjects, both astronomical and purely analytical, are of great importance; particularly his investigations on the subject of refraction prefixed to the *Turin Observations*, from 1822 to 1825, published in 1828; those on the motion of a pendulum in a resisting medium, &c. But it is of his researches on the lunar theory for which our medal has been actually awarded; and of these it behoves me now to speak; and I cannot do so in more clear, concise, and discriminating terms, than those used by Mr. Airy in his Report already alluded to:—

"The method pursued by Plana, in his *Théorie de la Lune*, is slightly, but not importantly, different (I mean in the fundamental equations) from those of his predecessors, Clairaut, Laplace, and Damoiseau. He first starts with the method of variation of elements, and pursues it to such an extent as to ascertain generally the form of the expressions connecting the longitude, the latitude, and the time. He then reverts to Clairaut's equations; and, as

these equations require for the successive substitutions an approximate expression for time in terms of longitude, he adopts a peculiar form (suggested by the variation of elements) for the principal part of it, and attaches to that principal part a subordinate part marked with the prefix δ. The same thing is done for the latitude. The process then is tolerably direct, and is almost similar to that of antecedent writers. In the fundamental algebra, therefore, there is no very great originality in the plan; but the mode followed in the detail of the work is beyond all praise. In the whole of the analytical combinations of this immense work, every part arising from the combination of any one term (however small) with any other term, is given separately, in such a form as to leave no difficulty in the detection of error to any careful examiner. The terms of peculiar difficulty (as, for instance, that depending on twice the distance between the node and the perigee) are made the subject of special discussion; and, in some instances, the origin of discordance between the author's results and those of Laplace is investigated with the same clearness which prevails through the other operations.

"In one respect, the plan of investigation differs much from those of his predecessors, as well as from Hansen's. The investigation is wholly symbolical: no numerical value is introduced, and no consideration of relation of values entertained, till the final substitutions are made. As an example of theory, there can be no doubt of the beauty of this process. As a subject for practical accuracy, it may not be so certain whether it is advisable. The convergence of the series is sometimes extremely slow. As far as I can observe, the accuracy of this method is exactly and properly that of successive substitution: but, in various parts of the lunar theory (in all places where the terms rise two orders by integration), the method of successive substitution is not sufficient; in fact, it is necessary to assume a term in order to find its correct value. Adopting this method, however, the author has pushed it as far as, probably, it will ever be carried. The whole is worked to the fifth order, and some parts to the seventh order.

"Finally, the author has determined from observations the principal constants which require to be substituted in the symbolical expressions, and has substituted them, and has thus produced a set of numerical expressions which may immediately be used for the formation of lunar tables.

"In terminating the remarks on the works of these two authors, Plana and Hansen, I must again express my very great admiration for both. But their merits are of very different kinds. The theory of Hansen is undoubtedly of the higher order, but it can hardly yet be said to be practical (at least in the lunar theory): many years will yet elapse before it will influence the lunar tables. The theory of Plana is very good, and probably adequate in all respects: it is eminently practical in form: it has already influenced the investigations of other writers, and will probably soon influence the tables."

There is but one thing more to add to this clear and powerful summary, and I will supply it by a quotation from the work itself:—

"Je n'ai pu me faire aider par personne; j'ai du traverser *seul* cette longue chaîne des calculs, et il n'est pas étonnant si par inadvertence j'ai omis quelques termes qu'il fallait introduire pour me conformer à la rigeur de mes propres principes." When we look at the work itself there seems something almost awful in this announcement.

A very important memoir of M. Plana, on the theory of the planetary perturbations, has adorned the *Transactions* of this Society. The points of which it treats are miscellaneous, and some of them, perhaps, not of the highest importance, except in one point of view, and that, perhaps, the most important of all. Every one who is at all conversant with these researches must be impressed with the enormous interval which separates — I will not say the mere differential equations of the planetary motions — but their integrals after much and intricate developement — from the final numerical results on which their tables are to be constructed; that is to say, the computed values of the co-efficients of terms having the same argument, when assembled from all the points whence they arise in the algebraic processes and amalgamated together. M. Plana appears to have proposed to himself the gigantic task of revising and correcting not only those algebraic developements, but the actual numerical calculations of the whole *Mécanique Céleste;* and this paper contains many examples sufficiently proving the necessity of such revision, and leading the way to those further and more elevated researches on the theory of *Jupiter* and *Saturn*, to which the latter part of this memoir must be considered as having given occasion; and which are further developed in several other memoirs published in various academical and other collections.

Neither the time nor the nature of this occasion would allow of my entering into any history of the controversy to which the revision thus set on foot, and the discordant results arrived at in this memoir, gave occasion. Suffice it to say, that errors — venial, no doubt, and such as it would be miraculous did they not exist — were discovered on all sides, and the absolute necessity established not merely of a thorough revision of every part of these immense computations, but of printing and publishing the steps in that regular and methodical form, which alone can put it in the power of subsequent calculators to lay their finger on the precise point where error shall have crept in; and to resume the calculations from that point without sacrificing the whole of what precedes.

It is this methodical clearness — this letting in of the light on every dark corner of every intricate combination and heart-breaking numerical calculation, which may be regarded as marking from this time a new era almost in the planetary theory itself. In the *Mécanique Céleste*, we admire the elegance displayed in the alternate interlinking and developement of the formulæ, and exult in the power of the analytical methods used; but when we come to the statement of numerical results, we quail before the vast task of filling-in those

distant steps, and while cloud rolls on after cloud in majesty and darkness, we feel our dependence on the conclusions attained rather to partake of superstitious trust, or of amicable confidence, than of clear and demonstrative conviction. Let me not be misunderstood as by these expressions casting any reflection on the conduct of that immortal work. The surest proof of its titles to such immortality which can be given, is that microscopic examination subsequently lavished on every point embraced in its immense outline. It is no disparagement to the agriculturist, whose energies have extirpated the wilderness, and established in its place cultivation and wealth, that a period shall arrive when his furrows shall, in their turn, be replaced by the garden, and his system of culture by a measured and calculated succession. Neither would I be understood to lay the sole stress of our applause of M. Plana's researches on the luminousness of their statement. His analysis is always graceful, his combinations well considered, and his conceptions of the ultimate results to be expected from them perfectly just, and justified by the results when obtained.

It cannot but be agreeable to this meeting to know that our award is duly appreciated by M. Plana himself, and regarded by him in the light which it is ever most desirable it should be,—as a stimulus to fresh researches and further exertions of his powerful talents in the same line where they have already reaped so rich a harvest. No sooner had the Council decided on their award, than, as in private regard no less than in public duty bound, I communicated to him the result; and his reply, which breathes the warmest spirit of attachment to the Astronomical Society, and of undiminished zeal in his own peculiar line of research, is now before me. In the absence of any personal friend to receive it for him, I shall now, therefore, present our medal to Mr. Rothman (in the absence of our Foreign Secretary, Captain Smyth), in his name, and request him to forward it to him, with our best wishes for his health and happiness.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: Sir John F.W. Herschel, Bart. K.H. M.A. V.P.R.S. —*Vice Presidents:* George Biddell Airy, Esq. M.A. *Astronomer Royal;* Francis Baily, Esq. V.P.R.S.; Augustus De Morgan, Esq.; John Lee, Esq. LL.D. F.R.S.—*Treasurer:* George Bishop, Esq.—*Secretaries:* Thomas Galloway, Esq. M.A. F.R.S.; Lieut. Henry Raper, R.N.—*Foreign Secretary:* Richard W. Rothman, Esq. M.A.—*Council:* Rev. W. Rutter Dawes; George Dollond, Esq. F.R.S.; Bryan Donkin, Esq. F.R.S.; Rev. George Fisher, M.A. F.R.S; Rev. Robert Main, M.A.; Edward Riddle, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Captain W. H. Smyth, R.N. F.R.S.; Lieut. William S. Stratford, R.N. F.R.S.; Hon. John Wrottesley, M.A.

SPECIAL GENERAL MEETING.

A Special General Meeting was holden after the business of the Annual Meeting was concluded, agreeably to the By-Laws (Sect. iv. § 9), to consider the following Resolution, which was proposed on the part of the Council, the President in the chair:

"That Sir Francis Charles Knowles, Bart., Joshua R. Marshman, Esq., and Charles Perkins, Esq., being resident in England, and having treated with neglect the repeated applications made by the Council, agreeably to the 5th Section of the By-Laws, for payment of the arrears due by them, and having suffered their names to be suspended in the Meeting Room as defaulters since the 12th May, 1837, be expelled from the Society."

It having been ascertained, at the commencement, that considerably more than twenty-four Fellows were present, a ballot was taken upon each name mentioned in the resolution; at the close of which the President announced, that the three Fellows in question were severally expelled from the Society.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *March* 13, 1840. No. 5.

Sir John F. W. Herschel, Bart. President, in the Chair.

H. W. Jeans, Esq. of the Royal Naval College at Portsmouth, was balloted for, and duly elected a Fellow of the Society.

The following communications were read:—

I. On the Regulator of the Clock-work for effecting uniform Movement of Equatoreals. By G. B. Airy, Esq. Astronomer Royal.

The subject of this communication is a mathematical investigation of a mechanical problem of great importance in practical astronomy. The author remarks, that the accuracy given to a most delicate and valuable species of observations, by the use of clock-work attached to equatoreals, is so great, and the importance of the application so evident, that any investigation which assists in elucidating the principles on which such apparatus should be constructed, and especially any which points out the nature of one important defect to which it may be liable, cannot but be regarded as interesting to the practical astronomer and the instrument-maker.

After adverting to the different methods of giving motion by a train of wheel-work to the polar axis of the equatoreal, which have been adopted in the principal instruments hitherto erected, as the Dorpat telescope, the Konigsberg heliometer, the Cambridge equatoreal, &c., the author proceeds to consider the various means which have been put in practice for effecting the regulation. In the mountings constructed by Fraunhofer, the axis of the regulator is vertical; it carries a horizontal cross arm, to the extremities of which are attached springs nearly transverse in direction to the cross arm, carrying at the ends small weights. When the regulator is made to revolve with a certain velocity, the centrifugal force of the balls bends the springs till the balls just touch the inner surface of a drum which surrounds the regulator: the smallest additional velocity causes the balls to press against the drum and create a friction which immediately reduces the velocity; and the drum is made slightly conical so that by raising or depressing it the velocity may be altered at pleasure This construction not only partakes of the defects common to all the others, but is liable besides to this peculiar objection, that the determinate rate will depend most essentially on the strength of the springs, and will therefore depend on temperature and other varying causes. The other constructions

(which were practically introduced by Mr. Sheepshanks) depend upon the same principle as that of the governor of the steam-engine. Two balls suspended from the upper part of a vertical axis, by rods of a certain length, are made to expand by the rotatory velocity of the axis; and this expansion, when it reaches a certain extent, is made to press a lever against some revolving part, and thereby to create a friction which immediately checks the velocity. In some cases the balls are suspended by rods from the extremities of a horizontal arm carried by the vertical axis. This construction, adopted in the south equatoreal of the Royal Observatory, may be considered analogous to Fraunhofer's, substituting for the springs the gravity of the balls;—a change which can hardly fail to be advantageous.

Now, the uniformity of rotatory motion of the spindle, in these various constructions, depends entirely on this assumption; that if, upon the whole, the retarding forces are equal to the accelerating forces, the revolving balls will move in a circle and in no other curve. But this assumption is not correct. If, for instance, we consider the case of balls, suspended as in the governor of the steam engine; the motion of each of the balls may be the same (omitting the moments of inertia of the various parts of the machine, which are trifling) as that of a ball, suspended by a string, and put in motion by an arbitrary impulse; and a ball so suspended may move in a curve differing insensibly from an ellipse. Now, this elliptic motion actually takes place. In some instances observed by the author, the balls of the regulator, instead of revolving in a circle, revolved in an ellipse of considerable excentricity, and the rotatory motion of the spindle was therefore exceedingly variable. The effect of this irregularity on the motion of the equatoreal, whether the inequalities of motion are followed by the polar axis, or merely communicate a general tremor to the frame, must be injurious.

The inequality now mentioned is only one case of a very extensive theorem, which may be thus enunciated:—"Whenever the equilibrium of forces requires that a free body be brought to a determinate position, either absolute or relative to other parts of the mechanism with which it may be connected, the body will not remain steadily in that position of equilibrium, but will oscillate on both sides of that position, and (so far as the action of those forces affect it) will have no tendency to settle itself in the position of equilibrium." This theorem supposes that some cause of disturbance has once put the body into a state of oscillation; and renders it necessary to take account of such oscillations in planning any mechanism which depends upon assuming the position of equilibrium to be nearly preserved.

If we examine the theory of the regulator, we shall see that the friction which checks the motion takes place when the balls are most distant from the axis, and (as the equable description of areas is nearly observed) this occurs when the angular motion is least. The whole maintaining force acts without check when the balls are

nearest to the axis, that is, when the angular motion is greatest. Therefore, when the angular motion is least, the acting forces tend still to diminish it; when greatest they tend to increase it. Hence the inequalities of angular motion will increase till some new forces come into play, which act in some different manner: and thus is explained the obstinate adherence of the governor balls in some cases to their elliptic motion.

The author next proceeds to consider the ways in which an attempt may be made to counteract the injurious effects of such oscillations. These appear to be only two: one, to make the oscillations of velocity much slower (or to make their periodic time longer); the other, to make the oscillations quicker (or to make their periodic time shorter). The first of these methods has the effect of giving greater smoothness to the motion (an object of great importance); and it is the principle which was adopted with success in the clock-work of the Cambridge equatoreal. The second method endangers the smoothness of the motion; but, as the error has but a short time for accumulation, it ensures that the object shall remain steady under the view of the telescope far more completely than the first. The construction attached to the clock-work of the south equatoreal of the Royal Observatory is on this principle; and it appears to answer extremely well.

The mathematical problem proposed by the author in the present communication is an investigation into the motion of governor balls, for the purpose of deducing the time of rotation corresponding to a given expansion of the balls, and the periodic time of their oscillations, and the consequent oscillations in the angular speed of the spindle; and the subject is discussed on four different suppositions, which, with their several principal results, are as follows: 1. When the balls are supposed to be acted upon by no forces. The result is, that the periodic time of oscillation is somewhat greater than half the time of rotation. 2. When the axis which carries the balls has a fly-wheel attached to it. In this case the periodic time of the oscillations cannot be less than half the time of rotation, and may be in any proportion greater. 3. When the balls are suspended by rods from a horizontal arm carried by the regulator-spindle. The result is, that the periodic time of the oscillation may be made small in any proportion to the time of rotation. 4. On an assumed law of accelerating force and retarding friction. The result is, that the effect of these forces is to increase continually the inequality of motion.

II. Note on an Arabic Globe belonging to the Society. By R. W. Rothman, Esq. M.A., Foreign Secretary.

The instrument in question is a small bronze globe, about six inches in diameter, brought some time ago from the East, having the positions of the principal stars marked by silver studs, with their Arabic names engraved; and the object of the present Note is to point out the differences between the names of the stars as found on the globe, and those given in the Catalogue of Ulugh

Beg, with which, in general, the globe agrees, though in some instances the differences are worthy of notice. From the position of the colures, &c. it is inferred that the globe is not of ancient date; but it bears no mark indicative of the precise period of its construction.

III. Elements of Galle's Second Comet; computed by M. Petersen, and communicated by Professor Schumacher.

Passage, 1840, March 12,77822, M. T. Altona.		
Log. perihelion distance	= 0·0877164	
Longitude of perihelion	= 80° 30′ 34″	M. Eq. March 12.
Longitude of ☊	= 236 40 13	
Inclination	59 10 41	Motion retrograde.

After the reading of the papers, the President announced that the Council had agreed that the following Address of Congratulation be presented to the Queen.

To the Queen's Most Excellent Majesty.

The Humble Address of the President, Council, and Fellows of the *Royal Astronomical Society* of London.

We, your Majesty's most dutiful and loyal subjects, the President, Council, and Fellows of the Royal Astronomical Society of London, beg leave to offer to your Majesty our most humble, but at the same time most warm and hearty congratulations on the recent auspicious event of your Majesty's marriage with His Royal Highness Prince Albert of Saxe Coburg and Gotha, an event uniting all hearts in fervent wishes for the long continuance of that happiness, prosperity, and glory, with which Providence has been graciously pleased to surround the earlier years of your Majesty's life and reign.

Among the noblest trophies that can adorn civilised power, the successful cultivation of science under the fostering influence of royal protection, has ever been deemed to confer additional lustre on the happiest and most glorious reigns which history commemorates. In that of your Majesty we have already the assurance of experience that such protection will be largely accorded: and should our humble labours in the science to which we are specially devoted, patronised as they are by your Majesty's gracious countenance, be fortunate enough to add one ray to the glory that surrounds your Majesty's throne, we shall consider our exertions richly overpaid. But, whatever may be our fortune in this respect, we shall ever pray, in common with every class of your Majesty's subjects, that prosperity and happiness, public and domestic, with length of days on earth and the rich reward hereafter of a well-spent life, may be the lot of your Majesty and your Majesty's most illustrious consort.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *April* 10, 1840. No. 6.

Francis Baily, Esq. Vice-President, in the Chair.

William Sharp, Esq., President of the Bedford Philosophical Society, was balloted for, and duly elected a Fellow of the Society.

The following communications were read: —

I. Observations made at the Cape of Good Hope, in the year 1838, with Bradley's Zenith Sector, for the Verification of the Abbé De Lacaille's Arc of the Meridian. By Thomas Maclear, Esq. F.R.S. Communicated by the Lords Commissioners of the Admiralty.

The present paper is the second which has been received from Mr. Maclear on the subject of the important and interesting operations now going on at the Cape, relative to the measurement of an arc of the meridian.

In the former communication, an account of which is given in the Monthly Notice for April 1839 (*Monthly Notices*, Vol. IV. p. 189), Mr. Maclear detailed his proceedings for the purpose of identifying the terminal stations of Lacaille's arc; the present contains the sector observations, with their reduction, and the determination of the amplitude of the arc.

The sector was delivered to Mr. Maclear in Table Bay, by Captain Maitland, of H. M. ship Wellesley, on the 9th of December, 1837, and was conveyed the same day to the Royal Observatory under his personal superintendence; and on the 10th the instrument was put together and erected in the centre room. As this room was originally constructed for a zenith-tube of limited dimensions, it became necessary to enlarge the apertures by sawing through a portion of the iron bars of the grating forming the floor of the lantern, and of the rafters above. In this tedious operation nearly a month was consumed; but, in the meanwhile, a tent and a tripod for the support of the sector were prepared, a list of stars selected, and a variety of details settled, in which the author states Sir J. Herschel cordially assisted with his advice.

The site of Lacaille's Observatory in Cape Town being covered by a large building, erected since 1752, the sector could not be placed exactly over Lacaille's station; it was accordingly raised in the court-yard of the house, under a tent, and every disposition

made, which the confined locality admitted of, to secure the canvass against the effects of the wind. The instrument was erected and adjusted on the 29th of January, and the observations commenced the same evening. They were carried on until the 19th of February, but under very disadvantageous circumstances, principally from the violence of the north-east wind acting on the unsettled canvass, and the showers of sand carried into the tent from the street. This series of observations proved to be unsatisfactory, and was not used for the determination of the amplitude.

As one of the objects for which the present observations were undertaken was to determine the influence of Table Mountain on the direction of the plumb-line, the sector was next transferred to a station close up towards the precipitous front of the mountain, on its north side, and about 1000 feet above the level of the sea. Previous, however, to the commencement of the observations at this station, Mr. Maclear removed the bisecting wires, which had been found too thick for several of the stars employed, and substituted cobweb. The observations began on the 24th of February, and were continued till the 13th of March, when the sector was dismounted and carried, as before, by Coolies to the office of the Engineer Department in Cape Town.

The next step in the proceedings was to transport the sector to Klyp Fonteyn, the northern extremity of the arc. The party arrived at the station on the 24th of March, and immediately prepared to erect the sector on the corn-floor described in the former *Notice* as Jacobus Cotsee's foundation, which is situated at, and rather within, the south extremity of the ruin supposed by Captain Everest to be the granary of Lacaille. Before the instrument was set up, the several pieces forming the bearings and upper adjustments of the tube were separated, and carefully cleared from sand and dust. A tent having been raised, and fixed to iron pins driven into the floor, the tube was placed on its bearings, and the two barometers suspended from the sector tripod. At the distance of twenty-one feet exactly, and due east of the sector axis, a nail was driven into the floor, on which the axis of the repeating circle was placed.

The observations for zenith distances began on the 28th of March, and were continued to the 21st of April, a sufficient number having then been made for settling the question of the amplitude. Before leaving the station plans were made of the place, and of the foundations which had been discovered by Lieutenant Williams and the sappers; a base-line was measured, and the nature of the country to the north of the station examined. It was on the 6th of April that the foundation was discovered, whose dimensions correspond in some measure to the description given by Lacaille in his *Journal* of the granary he had occupied. The sector having been taken down, and a bottle containing an inscription deposited in a hollow chiselled into the solid rock about three feet below the surface, to mark the spot over which it had been erected, the party quitted Klyp Fonteyn on the 25th of April, and arrived at the Observatory on the 1st of May.

A cursory comparison of the observations having shewn that those made at the southern station did not deserve the confidence required in a work of this kind, where the length of a few feet is of importance, and experience having proved that good observations could not be obtained under a tent in Mrs. De Witt's yard, it was resolved to look out for some solid building close to the station, where the sector could have fair play. Mr. Maclear fixed upon the Raggebay Guardhouse, and the necessary permission having been obtained, a hole was made in the roof, the floor taken up and sunk to the requisite depth, and the sector erected on the 7th of May, one week after the return of the party from Klyp Fonteyn.

From the state of the weather, no observations could be obtained until the 12th, and frequent interruptions afterwards occurred from the same cause; so that six weeks were expended on a work which, in favourable weather, might have been accomplished in two.

The sector was dismounted on the 30th of June, and carried back to the Observatory, where it was again set up in the sector-room on the 2d of July. On examination, it was found to be as perfect as when first received, without the slightest mark of injury.

The author next proceeds to give the reduction of the observations. The barometers employed at Klyp Fonteyn were made by Mr. Thomas Jones, and from a comparison of their results with the Journal kept at the Royal Observatory, it appears that the station at Klyp Fonteyn is 485 feet above the mean level of the sea.* The station in the Guardhouse is close to the sea beach; the feet of the sector tripod could not be more than two or three feet above high-water mark. The chronometer employed at both stations was by Arnold, and beats half-seconds. Its performance at Klyp Fonteyn was good; at the Guardhouse, the reverse. At the former place, its rate was usually obtained by altitudes near the prime vertical; in Cape Town, by means of journeymen pocket-chronometers, carried to and from the transit-clock at the Observatory.

The collimation of the middle wire was deduced from the successive transits of stars in the alternate positions of the limb, east and west.

The corrections for aberration, precession, and nutation were calculated by means of the constants in the Royal Astronomical Society's Catalogue, recomputed for 1838.

The number of stars observed was 40; of which 20 were to the north, and 20 to the south of the zenith at the Cape. The number of observations at Klyp Fonteyn is 464, and at the Guardhouse 669; in all, 1133. It may therefore be supposed that errors of observation are reduced to nothing.

The final results are deduced as follows: — The amplitude being found from the mean of the reduced zenith distances of

* In the former communication the height of the station is stated to be nearly 400 feet.—*Monthly Notices*, vol. iv. p. 194.

each star observed at both stations, and each result having a *weight* assigned to it equal to the product obtained by multiplying the least number of observations of the star in question at one station, and in one position of the sector, into the least number for the same star at the other station, the resulting mean amplitude from the stars north of the zenith is 1° 13′ 14″·173, and from the stars south of the zenith, 1° 13′ 14″·961.

By assigning to the amplitude found from each star a weight equal to the quotient of the square of the number of observations of the star by twice the sum of the squares of the errors at both stations, the group to the north of the zenith gives 1° 13′ 14″·173, and the group to the south 1° 13′ 14″·953. The results of the two methods of computation may be regarded as identical; and the stars north of the zenith give the amplitude *less* by 0″·78 than the stars south of the zenith.

It is not easy to assign the cause of this discrepancy. Mr. Maclear inclines to ascribe it to the probable expansion of the tube at Klyp Fonteyn from the high temperature. While the observations were made the thermometer was sometimes as high as 93°, while at the Guardhouse the range was between 57° and 63°. Lacaille (*Mém. de l'Acad.* 1752) states that his southern group of stars gave the amplitude greater by 0″·8 than the northern. This near coincidence with Mr. Maclear's result is remarkable.

If the expansion of the tube be the cause of the discrepancy, the mean between the north and south groups is as correct as if no expansion took place. This mean is 1° 13′ 14″·56, with a probable error not exceeding 0″·03.

The axis of the sector on the corn-floor at Klyp Fonteyn was 216 feet (reduced to the meridian) *south* of the centre of the foundation discovered on the 6th of April, and supposed to be Lacaille's sector station. The axis of the sector in the Guardhouse was 45 feet on the meridian *north* of Lacaille's sector station in Mrs. De Witt's yard. Now 261 feet = 2″·56; which added to 1° 13′ 14″·56 gives 1° 13′ 17″·12 for the amplitude of Lacaille's arc. Lacaille's value is 1° 13′ 17″·33.*

The author remarks in conclusion, "that although this work does not clear up the anomaly of Lacaille's arc, it redounds to the credit of that justly distinguished astronomer that, with his means, and in his day, his result from 16 stars is almost identical with that from 1133 observations on 40 stars, made with a powerful and celebrated instrument. Our field of inquiry is now limited to the terrestrial measure, which every friend to science must wish to see undertaken without delay, as a portion of a greater arc to extend so far as to neutralise local attractions, and leave no doubt upon the true curvature of this portion of the southern hemisphere."

* In the *Fundamenta Astronomiæ* (page 184), Lacaille states that the amplitude deduced from the observations corrected for errors subsequently discovered in the divisions of the sector, and recomputed, is 1° 13′ 17″·5.

II. The Longitude of Madras, computed from Moon-culminating Observations. By Edward Riddle, Esq.

Mr. Riddle having undertaken, at the request of Mr. Baily, to compute the longitude of Madras Observatory from a mass of corresponding moon-culminating observations made there and at Greenwich, Cambridge, and Edinburgh, in 1834, 5, 6, and 7 —observations which have not before been used for the purpose, has availed himself of the occasion to enter at considerable length into the practical details of this method of computing the longitude; and has given all the requisite formulæ, with examples of their application from the observations under discussion.

The recent improvements in the *Nautical Almanac* have greatly simplified the formulæ for computing the longitude by this method; the moon's place being given for equal intervals of time instead of unequal intervals, as they were in the *Ephemeris* when the method was first proposed.

Some of the results are strikingly accordant; and others, under circumstances apparently favourable, differ more from the general mean than could have been expected.

The Madras observations are made under disadvantages peculiar to that observatory. Mr. Riddle remarks that "the heat of the climate at Madras is unfavourable to the going of the clock; the oil thickens rapidly, and the clock in consequence requires frequent cleaning,—an operation which it is well that the superintendant is able to perform with his own hands. The Madras clock, however, is not of first-rate quality; and, in its best state, Mr. Taylor does not generally rely on its rate as constant for more than a small portion of a day."

Anomalies are occasionally found among the observations at the other observatories; some of these are noticed and discussed in the paper.

Mr. Taylor institutes a comparison between the moon's place as observed at Madras, and as interpolated from the *Nautical Almanac*; and, in adverting to this comparison, Mr. Riddle notices that the Madras observed places are those of the moon's centre when her bright limb is on the meridian.

In his concluding general remarks, Mr. Riddle notices with expressions of regret the small number of corresponding observations of the moon's second limb which the extensive series presents, and expresses an earnest hope that "in future the inconvenient hour of the night at which the second limb can be observed on the meridian will not prevent those who, as a matter of duty, observe the transits of the moon, from taking also, when practicable, a star or two on each side of her."

The general result of the computations is that the longitude of Madras from Greenwich is,

h m s			
5 20 54·9	by 54	observations at	Greenwich and Madras
53·9	.. 56	......	Cambridge and Madras
58·0	.. 65	......	Edinburgh and Madras.

III. Ephemeris and Elements of the third Comet discovered by Galle. By Mr. Rumker, Superintendent of the Observatory at Hamburg. Communicated by Dr. Lee.

Ephemeris for $15^h\ 20^m$ mean Greenwich Time.

	AR. of Comet.	Declination of Comet.	Distance from the Earth.
	h m s	° ′ ″	
May 24,	2 50 59	5 37 51 S.	1·95946
June 1,	3 20 49	9 14 6 S.	1·99319
11,	3 40 45	13 28 45	2·03446
21,	4 6 17	17 41 28	2·07933
July 1,	4 30 44·5	21 53 5	2·12956
11,	4 54 1·7	26 3 31	2·18643

The comet will probably be visible at the Cape of Good Hope.

ELEMENTS.

Passage, April 2·4954388, M. T. Greenwich.
Log. perihelion distance 9·8740948
Longitude of perihelion 324° 12′ 27″·4 } M. Eq. Jan. 1, 1840.
Longitude of ☊ .... 186 2 44·6 }
Inclination .... 79 51 52·2
Motion direct.

The following approximate elements of the same comet have been received from Professor Schumacher:—

Passage, April 3·109, M. T. at Altona.
Log. perihelion distance 9·87072
Longitude of perihelion 325° 57′·0
Longitude of ☊ ...... 186 21·5
Inclination .... 79 48·4
Motion direct.

It has been remarked that the above elements resemble those of a comet observed in China in 1097, and computed by Burkhard; and of one which appeared in 1468, of which observations are recorded by Pingré. Supposing the three appearances to have been of the same comet, the periodic time is thus about 371 years.

IV. On the present state of our knowledge of the Parallax of the Fixed Stars. By the Rev. R. Main.

This paper was in part read.

London:—Printed by Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *May* 8, 1840. No. 7.

Francis Baily, Esq. Vice-President, in the Chair.

The following gentlemen were balloted for, and duly elected Fellows of the Society:—

William Rutherford, Esq. of the Royal Military Academy, Woolwich; and Mr. George Huggins, Optician, St. Paul's Churchyard.

Among the presents announced at this meeting, was a 7-feet Newtonian Reflecting Telescope, the work of the late Sir William Herschel, and presented by him to his sister, Miss Caroline Herschel; in whose name, and that of the President, it was now presented to the Society.

The reading of Mr. Main's paper on the Present State of our Knowledge of the Parallax of the Fixed Stars, was resumed and concluded.

This memoir was read to the Council of the Society at their meeting in January of the present year; the object of it being a review of the parallax of 61 *Cygni*, recently obtained by Professor Bessel. In presenting it as a memoir, to be read before the Society, the author determined to allow it to remain in its original form of a report addressed to the Council; feeling that, if it were given in any other shape, his discussion of the results of eminent contemporaneous astronomers on the subject of annual parallax might seem presumptuous. He recommends to the notice of astronomers a very complete historical summary of astronomy, as connected with this subject, by Fockins (which work was printed

at Leyden in 1835), entitled *Commentatio Astronomica de Annuâ Stellarum Parallaxi*, which, he remarks, very materially assisted him in the prosecution of the historical part of his work. The author proposes the four following divisions of his report:

1. Abstracts of theoretical papers which have appeared on the subject of annual parallax.
2. A statement of the results of observations which have been made since the time of Bradley for the purpose of detecting parallax.
3. A review of the results of contemporaneous astronomers on the subject.
4. A discussion of Bessel's observations and results.

Under the first division, he gives abstracts of the following papers:

A Memoir by Clairaut.—Mémoires de l'Académie, 1739, p. 358.

A Memoir by Sir W. Herschel.—Phil. Trans. 1782, lxxii. P. I. p. 82.

A Memoir by Schubert.—Berliner Astronomisches Jahrbuch, 1796, p. 113.

A Memoir by Sir J. Herschel on the Detection of Parallax by the Variation of the Angle of Position of Double Stars.—Phil. Trans. 1826, P. II. p. 266; and 1827, P. I. p. 126.

A Memoir by Struve, forming one of the introductory chapters to his large work on double stars.

The discussion by Bessel of Bradley's Observations of Right Ascensions of Stars differing by nearly 12^h.—Fundamenta Astronomiæ, Introduction.

Under the second head he gives, briefly, the results obtained by Piazzi, Calendrelli, Brinkley, Pond, Bessel, and Struve, referring to the following works:

For Piazzi and Calendrelli:—

Memoirs of the Italian Society, Vol. XII.

Calendrelli's Opusculi Astronomichi, Vol. for 1806.

Zach's Monthly Correspondence, Vols. XVIII. and XIX.

For Brinkley and Pond:—

Phil. Trans., Vols. for 1810; 1817; 1818, P. II.; 1821, P. II.; 1823, P. I.; 1824, P. I. and II.

Memoirs of the Astronomical Society, Vol. I. P. II.

Transactions of the Royal Irish Academy for 1815, Vol. XII.

The Introduction to the Konigsberg Observations for the year 1816.

The Dorpat Observations, Vols. I., II., and III.

Under the third head, the following memoirs on the subject are considered:—

On the Parallax of α *Aquilæ*, by Mr. Henry Taylor, Astronomer to the East India Company at Madras.

On the Parallax of α *Lyræ*, by the Astronomer Royal.—Mem. Ast. Society, Vol. X.

On the Parallax of α^1 and α^2 *Centauri*, by Professor Henderson. Mem. Ast. Society, Vol. XI.

On the Parallax of Sirius, by Professor Henderson. Not yet printed in the Society's *Memoirs*.

Of these memoirs, it is sufficient to say that Mr. Henderson's parallax of α *Centauri* is discussed at some length; and the result is, that there seems a strong probability of a sensible parallax in this remarkable star, which is strongly recommended to the attention of southern astronomers.

Lastly, in discussing Bessel's parallax of 61 *Cygni*, the author proceeds as follows: *—

He divides the two sets of observations of the distance of 61 *Cygni* from the comparison stars (*a*) and (*b*), into separate groups, monthly and half-monthly, and takes the mean of the measures in each group as corresponding to the mean of the days in that group. These means are compared with Bessel's mean distances, which he has derived from the solution of his equations, and the differences tabulated. The effect of Bessel's assumed parallax is then computed for each mean day, and the resulting numbers placed in an adjacent column. The agreement between the computed effects of parallax and the above-mentioned differences is remarkably close, especially for the measures of distance of the star (*a*), in which the solution of the equations shews the error of the assumed proper motion to be very small indeed.

The following are the tabulated monthly groups:—

Results from the Measures of Distance of 61 *Cygni from Star* (*a*).

Mean day.	Mean of each group.	Bessel's mean distance.	Difference.	Effect of a parallax of 0″ 369
1837.				
August 23	461′·806	461′·609	+0″·197	+0″·212
September 14	·709	...	+ ·100	+ ·100
October 12	·649	...	+ ·040	— ·057
November 22	·395	...	— ·214	— ·258
December 21	·287	...	— ·322	— ·317
1838.		...		
January ...14	·233	...	— ·376	— ·318
February ... 5	·386	...	— ·223	— ·264
May 14	·854	...	+ ·245	+ ·238
June 19	·969	...	+ ·360	+ ·332
July 13	·825	...	+ ·216	+ ·332
August 19	·760	...	+ ·151	+ ·227
September 19	·649	...	+0 040	+0·073

* Bessel's letter is contained in Vol. IV. No. 17, of the *Notices* of the Astronomical Society.

Results from the Measures of Star (b).

Mean day.	Mean of each group.	Bessel's mean distance.	Difference.	Effect of a parallax of 0''·261
1837.	''	''	''	''
August.....22	706·504	706·291	+0·213	+0·133
September 15	·479	...	+ ·188	+ ·196
October....12	·419	...	+ ·128	+ ·227
November 22	·186	...	− ·105	+ ·187
December 20	·283	...	− ·008	+ ·100
1838.		...		
January....11	·177	..	− ·114	+ ·014
February.. 9	·084	...	− ·207	− ·100
March......12	705·900	..	− ·391	− ·196
May........13	706·206	...	− ·085	− ·209
June........20	·414	...	+ ·123	− ·102
July........21	·446	...	+ ·155	+ ·016
August.....22	·588	...	+ ·297	+ ·133
September 5	·498	...	+ ·207	+ ·175
22	·712	..	+0·421	+0·212

That the indication of a parallax in the agreement between the fourth and fifth columns of the preceding tables may be rendered more evident, a graphical projection of them is added. The times being set off by proportional spaces on the line of the abscissæ, inclined lines are drawn through the origin (Jan. 1, 1838), whose ordinates represent the effects of Bessel's corrections of the proper motion with contrary signs. From these lines the above differences are set off in the direction of ordinates to the abscissa. The curve, therefore, which passes through the extremities of these ordinates, represents the periodical effect of parallax; and accompanying curves being given, exhibiting the true effect of Bessel's assumed value of the constant, the agreement between the two is shewn to be most complete. In the case of measures of distance from star (*a*), the maximum and minimum and vanishing ordinates of the curve are shewn with almost as much regularity by the observed differences as by the assumed parallax; and in the second case, though the agreement is not so close, yet the general law of the curve of sines is well preserved.

In arguing on the evidence afforded by the foregoing tables and graphical illustrations, the author concludes, not only that a real parallax has been detected, but that its amount is very approximately given in Bessel's investigation, who is enabled, by repeating the same process, to diminish at pleasure the residual errors of the determination. This feature separates completely this from all former attempts, in some few of which an amount, rather greater than the limiting probable errors, would seem to announce a parallax, of which the evidence is yet so slight as to leave the mind quite unsatisfied of its existence; while the uncertainty of its

amount (supposing its existence to be proved) prevents its application to ulterior objects in sidereal astronomy.

To the memoir are annexed two appendices, the first of which contains the investigation of formulæ for computing the co-efficients of the constant of parallax in the two cases; and also for finding the variation in the angle of position of two stars very near each other, one of which is effected by parallax.

The second contains a translation of the most important parts of Bessel's description of his Heliometer, from the *Astronomische Nachrichten*, Vol. VIII. No. 189.

An extract was read from a letter from Professor Bessel to the President, stating that the observations on 61 *Cygni* had been continued through the last year to the end of March 1840; and that the most probable values of the parallax resulting from the measured distances of the double star from each of the two stars of comparison, (a) and (b), are as follows :—

188 obs. $61-a$......$0''\cdot3584-0''\cdot0756k$; weight, 64·66
214 ... $61-b$......$0\cdot3289-0\cdot0276k$; ... 78·89

(k being a small indeterminate correction depending on the effects of temperature on the micrometer-screw).

The sums of the squares of the errors cannot, on the supposition of a vanishing yearly parallax, be made less than —

$$61-a\ldots12\cdot7282-3\cdot2445k+0\cdot6330k^2$$
$$61-b\ldots15\cdot6507-1\cdot6094k+1\cdot7029k^2\,;$$

but on assuming the annual parallax to have the value before assigned to it, they become—

$$61-a\ldots4\cdot4245+0\cdot2579k\quad0\cdot2637k^2=4\cdot3614+0\cdot2637\,(k+0\cdot489)^2$$
$$61-b\ldots7\cdot1171-0\cdot1768k+1\cdot6426k^2=7\cdot1123+1\cdot6426\,(k-0\cdot054)^2$$

On deducing the value of k from the observations, those of the first star give, therefore, $k=-0\cdot489$; and those of the second, $k=0\cdot054$. M. Bessel shews, that the last of these values is more deserving of confidence than the first; nevertheless, the true value of the correction is still very doubtful, and it is accordingly left indeterminate.

Assuming the relative parallaxes of (a) and (b) to be equal, and having due regard to the mean error of both results, the most probable value of the annual parallax is found

$0''\cdot3483-0''\cdot0533k$
and its mean error $\pm0\cdot0141$

This result is greater by $0''\cdot0347$ than was found from the first series of observations.—See *Monthly Notices*, Vol. IV. No. 17.

Assuming $k = 0$, it results from this determination, that distance of the star 61 *Cygni* from the sun is 592,200 times the mean radius of the earth's orbit—a distance which light would require $9\frac{1}{4}$ years to pass through.

The following was omitted in the last *Monthly Notice* :—

Mr. Baily announced that his majesty, Christian VIII., King of Denmark, had been pleased to continue the offer of the gold medal founded by his predecessor for the first discovery of a telescopic comet, subject to the conditions and regulations inserted in the *Monthly Notice* for November 1835 (Vol. III. No. 17).

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ROYAL ASTRONOMICAL SOCIETY.

VOL. V. *June* 12, 1840. No. 8.

Francis Baily, Esq. Vice-President, in the Chair.

Mr. John Carter, of Cornhill, was elected a Fellow.

The following papers were read:—

I. Continuation of the Investigation for the correction of the Elements of the Orbit of Venus. By Mr. Glashier, of the Royal Observatory, Greenwich.

This paper is a continuation of Mr. Main's investigations, communicated to the Society in June 1837 and April 1838, and printed in Vols. X. and XI. of the *Memoirs*. Mr. Main corrected the orbit from Mr. Airy's observations of 1833–36. Mr. Glashier recomputed the observations of 1836 (for errors of assumed semidiameter), and computed the observations of 1837 and 1838, and gives corrections of the elements of the orbit for all the years: then, substituting the observations, he deduces the residual errors (as compared with Lindenau's tables), which he finds not so small as might have been expected from so fine a series of observations. The author, however, feels confident that the results are correctly derived, from the pains taken to ensure accuracy: and the whole is given in a detailed shape, in order that any suspected error may be the more readily detected.

II. An Account of some Experiments made with an Invariable Pendulum, at the Cape of Good Hope. By T. Maclear, Esq.

When Mr. Maclear was appointed Astronomer at the Cape of Good Hope, he was desirous of repeating the experiments there with an invariable pendulum; but it was some time before he had sufficient leisure to prosecute this measure. The pendulum had been previously swung in London by Mr. Baily, and also after its return to this country. The method of proceeding in such cases is so similar, and has been so often described, that it is unnecessary to enter further on that part of the subject. The result of these experiments shews that, on the assumption that the pendulum made 86400 vibrations in London, in a mean solar day, at the

temperature of 62° *in vacuo*, and at the mean level of the sea, it made only 86332·92 vibrations, under the same circumstances, at the Cape of Good Hope; which is almost identical with the experiments of Mr. Fallows, and differing very little from the experiments of Captains Foster and Freycinet. A new pendulum, consisting of a thick brass bar, without any bob, and furnished with *four* knife edges, is about to be forwarded to Mr. Maclear, which he proposes to swing at the principal stations of the triangulation that is now carrying on in that colony.

III. An Account of some Experiments made with three Invariable Pendulums, by Lieut. Murphy, R.E., during the late Expedition down the Euphrates. By Mr. Baily.

When Colonel Chesney undertook the expedition down the Euphrates, three invariable pendulums were placed under his care, for the purpose of their being swung at positions more *inland* than had been hitherto practised. Two of these pendulums (iron and copper) belonged to this Society, and the other (brass) to the Admiralty; and they are the same that were taken out by the late lamented Captain Foster. They had been previously swung in this country, before the expedition just mentioned, by Mr. Baily, and also subsequent thereto. Only two places presented favourable opportunities for swinging these pendulums during the expedition: the first, at Port William, near Bir, on the Euphrates; and the other, at Bussora. The experiments were made by Lieutenant Murphy, and were conducted with his usual caution and ability: the details are recorded in printed skeleton forms, with which he was furnished previous to his departure; but none of the computations were made till after Lieutenant Murphy's decease. The reductions have since been made by Mr. Baily, on the same data as those already mentioned in the seventh volume of the *Memoirs* of this Society. On the assumption that each of these pendulums made 86400 vibrations in a mean solar day in London, at the temperature of 62° *in vacuo*, and at the mean level of the sea, it is found that they respectively made the following number of vibrations at the two stations abovementioned, viz.:

Pendulum.	Port William.	Bussora.
Brass	86340·66	86318·98
Copper	86341·30	86317·96
Iron	86338·96	86317·66
Mean =	86340·31	86318·20

IV. The Elements of the Annular Eclipse of the Sun that will happen on October 8th, 1847. By Mr. George Innes.

These elements have been deduced from Carlini's "Solar Tables" (1832), and Burckhardt's "Lunar Tables" (1812). The time of apparent conjunction in right ascension at Greenwich will be Oct. $8^d\ 19^h\ 28^m\ 36^s{\cdot}593$; at which time the sun's apparent semidiameter will be $16'\ 2''{\cdot}529$, and the moon's augmented semidiameter, $14'\ 43''{\cdot}968$:

Greenwich, mean time.	Sun's true longitude.	True right ascension.	Declination south.	Equation of time.	Semi-diameter.
18 hours	195° 19′ 31″·390	194° 6′ 41″·844	6° 2′ 20″·693	12′ 29″·567	16′ 2″·512
19 do.	195 21 59·783	194 8 59·511	6 3 17·975	12 30·245	16 2·524
20 do.	195 24 28·183	194 11 17·180	6 4 15·247	12 30·930	16 2·536
21 do.	195 26 56·616	194 13 34·914	6 5 12·525	12 31·597	16 2·547
$21^h\ 6^m\ 37^s{\cdot}755$	195 27 13·036	194 13 50·125	6 5 18·861	12 31·670	16 2·549
22 hours	195 29 25·054	194 15 52·614	6 6 9·797	12 32·271	16 2·560

Greenwich, mean time.	Moon's true longitude.	Latitude north.	Equatorial horizontal parallax.	Horizontal semidiameter.	True AR in arc.	Declination south.
18 hours.......	193° 55′ 30″·843	22′ 49″·441	53′ 54″·139	14′ 41″·300	192° 57′ 44″·323	5° 8′ 47″·322
19 do.	194 24 59·768	25 32·833	53 54·197	14 41·316	193 26 5·329	5 17 42·208
20 do.	194 54 28·771	28 16·446	53 54·214	14 41·320	193 54 27·367	5 26 35·565
21 do.	195 23 57·790	30 59·751	53 54·325	14 41·351	194 22 50·127	5 35 27·710
$21^h\ 6^m\ 37^s{\cdot}755$	195 27 13·036	31 17·610	53 54·326	14 41·351	194 25 55·127	5 36 34·132
22 hours.......	195 53 26·993	33 42·861	53 54·409	14 41·428	194 51 13·894	5 44 18·646

Obliquity of the Ecliptic	23° 27′ 24″·514
Sun's semi-diameter	16 2·529
—— horizontal parallax	8·5925
—— latitude	+0·179

V. On the Comparison of the Neapolitan Standard Yard with the Standard Yard of this Society. By Mr. Simms.

The Neapolitan Government having directed Mr. Simms to construct a new standard scale, on a principle similar to that which was made for this Society, the object of the present communica-

tion was to place on record the results of the comparisons that were made with the centre yards of each scale. It appears, that 215 comparisons were made on nine several days by three different persons; and the mean of the whole shewed that the centre yard of the Neapolitan scale was longer than the standard yard of the Society's scale, by ·002680 of an inch: the greatest differences from the mean being +·000520 and —·000365 of an inch. This new scale is marked No. 6; it being the sixth scale of this construction that has been made.

VI. On the difference of Longitude between the Observatories of Madras and the Cape of Good Hope, deduced from Moon-culminating stars. By T. Maclear, Esq.

The observations extend from February 19, 1834, to October 10, 1837, both inclusive, and contain all the corresponding observations within that period, except two which appear to be erroneous; and amount to seventy in number. Of these, only three were of the second limb. The result of the whole shews that the difference of longitude is $4^{h}\ 7^{m}\ 1^{s}{\cdot}56$, with a probable error of $\pm\ 0^{s}{\cdot}53$.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *November* 13, 1840. No. 9.

Francis Baily, Esq., Vice-President, in the Chair.

The Right Honourable the Earl Fitzwilliam, &c. &c., and Augustus Percival Greene, Esq. R.N., were elected Fellows of the Society.

The following communications were read:—

I. A Letter from Mr. Dawes on the subject of a new Binary Star recently observed.

"I beg to call the attention of the Society to the star registered by Sir William Herschel as the 16th of his third class of double stars. Its Æ is $20^h\ 23^m.6$, and N.P.D. 79° 17′. This star was measured by Herschel and South in 1822 with the five-foot achromatic. It was again observed by Struve on two nights in 1829, and also on two in 1832; and though powers of 320 and 480 were employed in the measurements, nothing remarkable was noticed by him in either of the stars. On turning upon it Mr. Bishop's equatorially-mounted achromatic telescope, having an aperture of seven inches, and focus of nearly eleven feet, armed with a power of 320, though the character of the night (October 27) was very indifferent, I was immediately struck with the elongated appearance of the smaller of the two stars; and having applied higher powers, I procured measures of the direction of the elongation. I have since obtained two other sets with power 420, with which, in best moments, the elongated disk was slightly notched. The results of the three nights' observations are:—

October 27,	Position = 208°40′	Weight = 12
— 31,	— = 208 25	— = 11
November 4,	— = 208 44	— = 18
	Mean ... = 208 38	

The estimated central distance = 0″·6 or 0″·7.

"It is extremely improbable that so acute an observer as Struve should have failed to recognise an appearance which is now so obvious and measurable with a much smaller instrument, if the star had then presented the same aspect as at present — the Dorpat refractor being capable of *distinctly separating* stars of the 8th or 9th magnitude whose central distance does not exceed 0″·4. Most likely, therefore, this star constitutes a new binary system; and it is highly desirable that, during the remainder of its present apparition, observations should be obtained by different individuals possessing instruments of sufficient optical power. To the notice of such, I beg earnestly to commend it.

"I may embrace this opportunity of announcing that the observations made during the last sixteen months at Mr. Bishop's Observatory afford satisfactory proof of binary character in several instances where it had been only suspected to exist, and of a very large amount of orbital motion in some binary systems previously known as such. The close pair of ι *Equulei* are decidedly more distant than when observed by Struve in 1835 and 1836. Within the last four years, 4 *Aquarii* has advanced 20° in its orbit; in which interval the star H i. 39 (Σ 3062) has changed its position to the extent of about 40°, and η *Coronæ* nearly 50°, with a central distance of scarcely 0″·5. The alteration is also striking in ζ *Herculis* (now measurable with a five-foot achromatic) and in Σ 2107; while τ *Ophiuchi*, which for years defied the power of the Dorpat telescope even to elongate it, has now opened out to the extent of nearly a second between the centres of its component stars. Supposing Sir W. Herschel's measure of the close pair of ζ *Cancri*, taken in 1781, to be exact, that remarkable binary system will now have *completed* a whole revolution since that date, that is, in 59 years.

"W. R. DAWES."

"Mr. Bishop's *Observatory*, *Regent's Park*,
Nov. 12, 1840."

II. A Supplemental Catalogue of the Right Ascensions of Fifty-five Stars contained in the Royal Astronomical Society's Catalogue. By the Honourable John Wrottesley.

Of the stars contained in this catalogue, seventeen had been wholly, or in part, observed at the time Mr. Wrottesley's former catalogue of 1318 stars,* for which the gold medal of the Society was awarded in 1839, was in course of observation, but are not included in that catalogue. The remaining thirty-eight are selected from the first list of stars accompanying Mr. Baily's "Address to Astronomical Observers," May 1837; and such were chosen as had not been previously observed at the author's observatory, or

* See the *Monthly Notice* for November 1836.

had not been observed at all since Piazzi's time, or presented discrepancies the clearing up of which seemed most likely to prove interesting. The observations of these thirty-eight were began in May 1837, and carried on until August in the same year, by Mr. Hartnup; they were resumed by Mr. Wrottesley himself in December 1839, and concluded in August 1840. In every case the observations were continued until six or more of each star had been procured. In observing this catalogue all possible care was exercised that the mean places of the stars comprised in it should be entirely unaffected by any errors arising from imperfect instrumental adjustment. With respect to the standard stars employed, in 1837 only a few were used, sometimes only one, but care was taken that it should be situated very near the parallel of declination of the catalogue star. In 1840 all the Greenwich stars were used indifferently, that pass the meridian to the south of the zenith, and are comprised in the list of those with which the former catalogue was compared; and usually from five to six, and sometimes as many as from eight to ten, were observed on the same day, and the mean clock-error resulting therefrom used in deducing the catalogue stars. Bessel's right ascensions of these standard stars, and Mr. Wrottesley's own place of Fomalhaut, have been invariably used in reducing the catalogue stars: so that this catalogue, as well as the preceding, is founded on Bessel's mean places of the Greenwich stars. In an introduction to the catalogue, the author has explained in detail the different methods which he employed for determining the errors of level, collimation, and azimuth, and the other corrections applied in the reductions. The performance of the transit-clock during the progress of the observations was satisfactory; for though the rate was at times considerable in amount, it was always uniform. As a test of the extent to which the catalogue may be relied on for accuracy, Mr. Wrottesley states, that out of the forty-three stars contained in it, which have been observed by Mr. Airy, in no one case does the difference of the results, for a star more than 25° from the pole, exceed $0^s.17$.

III. Postscript to Mr. Baily's Report on Mr. Maclear's Pendulum Experiments.* By Mr. Baily.

The author states that the Admiralty having left to his decision the form and construction of a new pendulum, which they had resolved on sending out to the Cape, for the purpose of being swung by Mr. Maclear at the several stations of the trigonometrical survey now in progress in that colony, he had not hesitated in adopting the *bar*-pendulum, as by far the best and most convenient for a travelling instrument. The pendulum which has been accordingly constructed is a brass bar, sixty inches long, two inches wide, and about half an inch thick. It was formed of several thin plates which were pressed together by a rolling machine, and is, conse-

* Read at the last meeting of the Society in June.

quently, very compact and hard. Its specific gravity was 8·60, and its rate of expansion for one degree of Fahrenheit's thermometer, ·00001034. It is furnished with four knife-edges, thereby affording the advantages of four distinct pendulums on one and the same bar, and which thus serve as a check on each other. As the construction of the pendulum did not allow of much filing away at the ends without cutting into the knee-pieces, the vibrations on the several knife-edges were rendered nearly isochronous (for absolute isochronism can hardly be obtained), by fastening a circular piece of brass, weighing 3000 grains, about an inch and a half from the centre of the bar; the weight and position having been determined by repeated preliminary experiments. After every thing was finished, seven sets of experiments were made on each knife-edge, the mean results of which were respectively as follows: knife-edge A, 85906·322 vibrations; B, 85905·725; C, 85904·107; D, 85903·427; in a mean solar day. The computations and corrections were made in the usual manner, with the exception of the correction for the height of the barometer, which can only be determined accurately by swinging the pendulum *in vacuo*. For this there was not time before the pendulum was sent off, and the correction was assumed to be the *double* of that which is given by the formula which was usually employed prior to the experiments of M. Bessel. The agate planes, which were made expressly for this pendulum, are attached to a solid frame of brass, three-quarters of an inch thick, and having three foot-screws for the purpose of levelling the planes.

IV. Observations of the Second Comet of 1840, made at the Observatory at Hamburgh. By Mr. Rumker.

The observations give the apparent right ascension and declination from January 29 to March 24, 1840.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *December* 11, 1840. No. 10.

John Lee, Esq. LL.D., Vice-President, in the Chair.

George Turnbull, Esq., Civil Engineer, of Middlesburg-on-Tees, was elected a Fellow of the Society.

The following communications were read:—

I. On a large Achromatic Object-Glass of a Telescope worked by Mr. Dollond, the flint glass of which was prepared by the late Dr. Ritchie. By the Rev. Samuel King, M.A. F.R.A.S.

In a paper by Mr. Simms, "On the Optical-Glass prepared by the late Dr. Ritchie," which was read to the Society on the 14th of June, 1839, and is now printed in Vol. XI. of the *Memoirs*, reference was made to an object-glass of 7⅜ inches aperture, the flint glass of which was worked by Mr. Dollond out of a disc prepared by Dr. Ritchie; and it was intimated that Mr. King, who had the object-glass at that time under trial, would probably report to the Society upon its performance. Mr. King now states that the result of numerous observations on a variety of objects leads him to consider this glass as one of much excellence, though not faultless. There is scarcely any spherical aberration, and the light is very white and free from colour; but when the central portion is covered up, there is a good deal of irradiation, indicating a want of homogeneity near the edge of the lens, where the glass is very thick. For many objects, however, especially very faint nebulæ, the whole aperture may be used with great advantage; but for most purposes a contraction to six inches, or a little less, causes it to perform much better, and enables it to carry high powers with much distinctness. The powers tried with it vary from 40 to 700, and no doubt it would satisfactorily bear a considerable increase in this respect. It is not saying much of such an object-glass, that the small stars accompanying *Polaris*, *α Lyræ*, and *Rigel*, are readily seen; but it shews also with the greatest distinctness and completely separates the close stars of *ζ Cancri*,

ι and ζ *Boötis*, ξ *Ursæ*, &c., and also δ *Cygni*, which affords one of the best tests of distinctness and perfection in a telescope. As a planetary glass, Mr. King remarks that he cannot speak of it so decidedly, not having had a fair opportunity of trying it. *Jupiter* and *Saturn* have both for a long time been at very low altitudes, and, with respect to his observatory, the entire range of each is through the smoke of London. But with the moon it performs magnificently, penetrating, as it would seem, into her very structure when high magnifying powers are used. Upon the whole, he is of opinion that this object-glass will bear comparison with most others of the same size worked from the foreign material.

The object-glass is fitted into a brass cell, admitting of accurate adjustment by means of three screws with rods reaching to the eye end. The telescope, twelve feet in length, is mounted upon the rotative roof of a small observatory, in a manner which combines great ease in observing with freedom and steadiness of motion.

II. Description of a Method of dividing one Circle, B, by copying from another, A, previously divided. By Lieut.-Col. Everest, Director of the Trigonometrical Survey of India.

In Col. Everest's *Account of the Measurement of an Arc of the Meridian in India*, published in 1830, it was stated that, with a view to avoid the effects of the errors of catalogues and the periodical irregularities to which all stars are more or less liable, it had been resolved to substitute two astronomical instruments, each with a vertical circle of three feet and an azimuth circle of two feet diameter, for the zenith sector of Ramsden, formerly used in determining the amplitudes by his predecessor and himself; the intention being that all celestial amplitudes should be determined by observations taken simultaneously at both extremities of the arc. Accordingly, two new altitude and azimuth instruments were ordered in London and forwarded to India, where they arrived in 1832; but an opportunity of giving them a fair trial did not occur until 1837, when a small observatory was constructed at Kaliana, the northern limit of one of the sections of the great arc of the meridian. The instruments proved, on trial, to be of too feeble construction for the purpose for which they were intended, and Colonel Everest found that, in order to render them serviceable, it would be necessary to construct for each a pair of stronger columns, new outriggers and friction rollers, a new axis for the azimuth motion, a new table for supporting the columns, and a new azimuth circle; in short, to construct two entirely new instruments with the exception of the vertical circles. To any other than a professed artist, an operation of this kind, even under the most favourable circumstances, would prove an arduous and formidable undertaking; but, in a half-barbarous country like Upper India, where no artificer was to be found who had ever tried his hand at dividing, or even seen a cutting-tool, it could not be otherwise than extremely embarrassing. Before Col. Everest un-

dertook the alterations, he had obtained, indeed, the assistance of a mathematical instrument-maker in the service of the Company, at Calcutta; but, owing to some disagreement, this person left him before the most difficult part of the operation, that of dividing the new circles, was ready to be commenced, and he was thereby thrown entirely upon his own resources.

Colonel Everest resolved to construct the new tables and azimuth circles of cast-iron, which, though objectionable on account of its liability to rust, has the advantage of being lighter and less yielding than brass. The circles were inlaid with gold to receive the graduations; and at every tenth degree a circular disc of silver was let in, so that the large figures might stand prominent. Each was supplied with a strong edge-bar below, which put all yielding out of the question, and moreover was of great service in the process of graduation, by affording a support on which to affix the reading microscopes. With respect to the mode of dividing the circles, Col. Everest states that, being aware of the dilatoriness and difficulty of any method he had seen detailed, he devised a plan of his own, whereby the divisions of the original circles should be copied on the new. Without the aid of drawings, it is scarcely possible to impart an idea of the mechanical means which were employed for carrying this plan into effect. The two circles, the new one which was to be graduated, and the old one from which the graduation was to be copied, were placed the one directly over the other, and strongly secured by screws; and the principle of the method consists in keeping the circle to be graduated, which is placed below, steadily fixed on a substantial pillar of masonry, and making the circle to be copied from revolve above it, carrying with it the cutting-tool and clamp: and the apparatus being thus arranged, it is obviously only necessary to apply sufficient means of reading off the angular spaces passed over by the latter in its rotation, in order to transfer its graduation to the former. The mode of operation was assimilated to that which is followed in taking angles. A skilful person was appointed to each of the four microscopes; the different readings were made precisely equal to the arcs to be intercepted; and when the one mean reading subtracted from the other shewed this value, the order was given to cut.

The author describes in minute detail the different parts of the operation, which appears to have been executed successfully, and he concludes his paper with an investigation of the mathematical relations of the method, and particularly the effect of eccentricity of the upper circle, and of any error in fixing the cutting-tool, whereby its point would be made to move obliquely to the radius.

III. Transits observed at Washington (United States), from January 1 to July 1, 1840; and Occultations observed at the same place, since June 1839. By J. Melville Gillies, Esq.

The transit is one of the 6-feet instruments made by Troughton for Mr. Hassler, in 1815, and mounted on substantial granite pillars.

The usual methods were adopted for determining its errors of level and collimation, and the observations are free from all such. The deviation in azimuth was determined from the observed and true differences of right ascension of high and low stars, and registered in its proper column; but the proportional part due to each observation was not in any case applied. The observations were registered by a chronometer regulated to sidereal time, and its rate determined by a mean of successive transits of the same star. The true sidereal time and right ascensions were taken from the list of moon-culminating stars, the list of 100 stars, and the list of stars liable to occultation given in the *Nautical Almanac;* and the remainder computed by means of the Royal Astronomical Society's constants. The moon's right ascension was determined by applying the mean error of the chronometer to the observed right ascension.

IV. Places of Bremicker's Comet, as determined with the Equatoreal Telescope at Mr. Bishop's Observatory. By the Rev. W. R. Dawes.

Date.	Greenwich Mean Time.	R.A.	N.P.D.	Declination.	Remarks.
1840.	h m s	h m s	° ′ ″	° ′ ″	
Nov. 14	9 4 19	.....	30 39 6	= +59 20 24	Rather doubtful.
	9 29 9	20 31 13	......	....	Ditto.
16	9 55 53	20 46 37	31 13 11	= +58 46 49	Tolerably good.
19	8 38 13	21 9 18	32 15 3	= +57 44 57	Good.
	9 37 59	21 9 33	32 16 11	= +57 43 49	Ditto.
	9 50 2	21 9 39	32 16 39	= +57 43 21	Ditto.
21	9 9 39	21 24 55	33 7 2	= +56 52 58	Very good.
24	8 51 1	21 48 4	34 37 47	= +55 22 13	Excellent.
26	6 54 40	22 2 43	35 45 10	= +54 14 50	Very good.
27	5 43 15	22 10 3	36 21 10	= +53 38 50	Comet excessively faint, seen through smoke and fog. Doubtful observation *on meridian*.
Dec. 2	7 31 58	22 46 39	40 0 11	= +49 59 49	Very good.
3	6 33 59	22 53 14	40 46 8	= +49 13 52	Excellent.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *January* 8, 1841. No. 11.

Francis Baily, Esq., Vice-President, in the Chair.

John Jesse, Esq. of Ardwick, Manchester; and John Francis Egerton, Esq., of the Hon. E. I. C. Horse Artillery, were elected Fellows of the Society.

P. Francesco De-Vico, Director of the Observatory of the Roman College at Rome, was elected an Associate of the Society.

The following communication was read:—

Remarks on the Present State of our Knowledge relative to Shooting-Stars, and on the Determination of Differences of Longitude from Observations of those Meteors. By Mr. Galloway.

After adverting to some of the earlier opinions which have been entertained on the nature of fire-balls, shooting-stars, and other igneous meteors, the author remarks that no very definite theory was formed respecting them till towards the end of the last century; for although the cosmical origin of the more remarkable bolides and fire-balls had been suspected, the shooting-stars were generally regarded as atmospherical phenomena, which were ascribed by some to electricity, and by others to the inflammation of hydrogen gas accumulated in the higher regions of the atmosphere. In 1794, Chladni published his celebrated work, in which he gave a catalogue of all the recorded observations of fire-balls; and, from a comparison of the different descriptions, inferred that these meteors have not their origin in our atmosphere, but are cosmical masses moving through the planetary spaces with velocities equal to those of the planets, which, when they encounter the earth's atmosphere, are inflamed by the resistance and friction, and become luminous, sometimes bursting into pieces, and scattering masses of stone and iron on the ground. This opinion was at first greatly ridiculed; but the repeated and even not unfrequent fall of meteoric stones, and the discovery by Howard that all of them present an almost perfect similarity of constitution, widely different from that of any substance found on the earth, at length forced conviction even on the most sceptical. From the close re-

semblance between fire-balls and shooting-stars, and, indeed, the impossibility in many cases of distinguishing the one class of meteors from the other, Chladni was led also to ascribe a cosmical origin to the latter phenomena. At this period, however, there were no observations from which precise or certain conclusions could be formed respecting the altitudes, velocities, or paths described by the shooting-stars—the elements by which the question of their existence within or beyond the atmosphere could be solved. In the year 1798, the first series of observations for determining these points was undertaken in Germany by Brandes and Benzenberg. Having selected a base-line of about nine English miles in length, and stationed themselves at its extremities, they began to observe on nights previously agreed on; and when a meteor was seen, they immediately traced its apparent path on a celestial map, noting carefully the exact times of its appearance and extinction, with any other circumstances likely to assist in identifying it. The meteors observed simultaneously at both stations were in this manner recognised with considerable certainty; and the comparison of their paths on the two maps afforded data for the determination of their parallaxes and altitudes. The results were as follows:—Between the 11th of September and the 4th of November, 1798, only twenty-two corresponding observations were obtained from which the altitudes could be computed. The altitude of the lowest was about 6 English miles; there were seven under 45 miles; nine between 45 and 90 miles; six above 90 miles; and one had an altitude of about 140 miles. There were only two observations from which the velocity could be deduced: the first gave 25 miles, and the second from 17 to 21 miles in a second. The most remarkable result was, that at least *one* of the meteors moved upwards, or away from the earth. By these observations, the perfect similarity between fire-balls and shooting-stars, in respect of velocity and altitude, was completely established.

Another attempt, on a more extensive scale, to determine the altitudes and velocities of shooting-stars by means of simultaneous observations, was made by Brandes in 1823, assisted by a number of associates resident in Breslaw and the neighbouring towns. The observations were continued from April to October, and during this interval about 1800 shooting-stars were observed at the different places, out of which number ninety-eight were found which had been observed simultaneously at more than one station. The altitudes of four of these were computed to be under 15 English miles; of fifteen between 15 and 30 miles; of twenty-two between 30 and 45 miles; of thirty-five between 45 and 70 miles; of thirteen between 70 and 90 miles; and of eleven above 90 miles. Two of these last had an altitude of about 140 miles; one of 220 miles; one of 280; and there was one whose height was computed to exceed 460 miles. Thirty-six orbits were obtained; in twenty-six of which the motion was downwards, in one horizontal, and in the remaining nine more or less upwards. In three cases only the observations were so complete as to furnish data for de-

termining the velocity; the results were respectively 23, 28, and 37 English miles in a second, the last being nearly double the velocity of the earth in its orbit. The trajectories were frequently not straight lines, but incurvated, sometimes horizontally, and sometimes vertically, and sometimes they were of a serpentine form. The predominating direction of the motion was from north-east to south-west, contrary to the motion of the earth in its orbit,—a circumstance which has been generally remarked, and which is important in respect of the physical theory of the meteors.

A similar set of observations was made in Belgium in 1824, under the direction of M. Quetelet, the results of which are published in the *Annuaire de Bruxelles* for 1837. M. Quetelet was chiefly solicitous to determine the velocity of the meteors. He obtained six corresponding observations from which this element could be deduced, and the results varied from 10 to 25 English miles in a second. The mean of the six results gave a velocity of nearly 17 miles per second, a little less than that of the earth in its orbit.

The last set of corresponding observations referred to in the paper was made in Switzerland on the 10th of August, 1838; a circumstantial account of which is given by M. Wartmann in Quetelet's *Correspondance Mathématique*, for July 1839. M. Wartmann and five other observers, provided with celestial charts, stationed themselves at the Observatory of Geneva; and the corresponding observations were made by M. Reynier and an assistant at Planchettes, a village about sixty miles to the north-east of that city. In the space of seven and a half hours, the number of meteors observed by the six observers at Geneva was 381; and during five and a half hours, the number observed at Planchettes by two observers was 104. All the circumstances of the phenomena—the place of the apparition and disappearance of each meteor, the time it continued visible, its brightness relatively to the fixed stars, whether accompanied with a train, &c., were carefully noted. The trajectories were then projected on a large planisphere. The extent of the trajectories described by the meteors was very different, varying from 8° to 70° of angular space, and the velocities appeared also to differ considerably; but the average velocity concluded by M. Wartmann was 25° per second. It was found, from the comparison of the simultaneous observations, that the average height above the ground was about 550 miles; and hence the relative velocity was computed to be about 240 miles in a second. But as the greater number moved in a direction opposite to that of the earth in its orbit, the relative velocity must be diminished by the earth's velocity (about 19 miles in a second). This still leaves upwards of 220 miles per second for the absolute velocity of the meteor, which is more than eleven times the orbital velocity of the earth, seven and a half times that of the planet *Mercury*, and probably greater than that of the comets at their peribelia.

From the above results, it is obvious that the heights and velocities of the shooting-stars are exceedingly various and uncertain; but if the observations are in any respect worthy of confidence,

they prove that many of these meteors (according to Wartmann's observations, by far the greater number) are, during the time of their visibility, far beyond the limits to which atmosphere is supposed to extend, and that their velocities greatly exceed that which is due to bodies moving at the same distance from the sun under the influence of solar gravitation.

It is perhaps impossible to form any correct estimate of the absolute magnitudes of the meteors. Their apparent magnitudes differ greatly; the greater number resembling stars of the third or fourth magnitude, while many are equal to stars of the first, and some even surpass *Jupiter* and *Venus* in brilliancy. It is remarkable that the largest are those which have the greatest altitudes, and only the smaller ones appear to come within 20 or even 40 miles of the earth.

With respect to the casual observations of the phenomena, the accounts of which are very numerous, the most interesting conclusion which has been inferred from them is the periodical recurrence of shooting-stars in unusual numbers at certain epochs of the year. Of these epochs, the most remarkable is that of November, on account of the prodigious number of meteors which have been seen in some years at that time. The principal displays were in 1799, 1832, 1833, and 1834. On the 11th of November, 1799, thousands were observed within a few hours by Humboldt and Bonpland at Cumana; and on the same night by different persons over the whole continent of America, from the borders of Brazil to Labrador, and also in Greenland and Germany. On November 12th, 1832, they were seen over the whole of the north of Europe; and on November 12th, 1833, the stupendous exhibition took place in North America which has been so often described. From the accounts of this phenomenon collected by Prof. Olmsted, M. Arago computed that the number of meteors on this night amounted to 240,000. In 1834, a similar phenomenon recurred on the night of November 13th, but on this occasion the meteors were of a smaller size. In 1835, 1836, and 1838, shooting-stars were observed on the night of November 13th, in different parts of the world; but though diligently looked for on the same night in the last few years, they do not appear to have been more numerous than on other nights about the same season,—a circumstance which has shaken the faith of many in their periodicity.

The second great meteoric epoch is the 10th of August, first pointed out by M. Quetelet; and although no displays similar to those of the November period have been witnessed on this night, there are more instances of the recurrence of the phenomena. In the last three years shooting-stars have been observed in great numbers, both on the 9th and 10th; but they appear in general to be unusually abundant during the two first weeks of August. The other periods which have been indicated are the 18th of October, the 23d or 24th of April, the 6th and 7th of December, from the 15th to the 20th of June, and the 2d of January; and it is not improbable that further observations will add to the number.

The different theories which have been given to explain the origin and phenomena of the shooting-stars are next stated. The following are the principal :—

1. That the shooting-stars and fire-balls are substances projected from volcanoes in the moon. It is known that a body projected vertically from the moon with a velocity of about 8500 feet in a second would not fall back upon the lunar surface, but would recede from it indefinitely; and in order to reach the earth the projectile would only require, under the most favourable circumstances, to have a velocity of about 8300 feet. Such a velocity, which is only about four or five times greater than that of a cannon-ball, is quite conceivable; but the extraordinary exhibitions of 1799 and 1833, to say nothing of their supposed periodicity, is utterly irreconcilable with the theory of a lunar origin. Benzenberg, however, adopts this theory, and supposes the shooting stars to be small masses of stone, from one to five feet in diameter, which are projected from lunar volcanoes, and circulate about the earth or about the sun when their projectile velocity exceeds a certain limit.

2. Dr. Olbers, and some other astronomers, have supposed the shooting-stars to be the *débris*, or fragments of a large planet, burst into pieces by some internal explosion, of which *Ceres*, *Pallas*, *Juno*, and *Vesta*, are the principal remaining portions. The smaller fragments continue to circulate about the sun in orbits of great eccentricity, and when they approach the region of space through which the earth is moving, they enter the atmosphere with great velocity, and by reason of the resistance and friction are rendered incandescent, and emit a vivid light so long as they remain within it.

3. It has been suggested by Biot that the extraordinary displays observed in November may be explained by supposing the meteors to have their origin in the zodiacal light. The extent of this lens-shaped nebulosity is not well ascertained; but as the plane of its principal section is not parallel to the ecliptic, if the earth passes through it at one season, it must be remote from it at another. But shooting-stars are observed at all times of the year; and the November meteors differ from those of other seasons in no respect excepting in their greater multitude.

4. The hypothesis first suggested by Chladni is that which appears to have met with most favour, having been adopted by Arago and other eminent astronomers of the present day to explain the November phenomena. It consists in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November. The principal difficulties attending this theory are the following :— First, that bodies moving in groups in the circumstances supposed must necessarily move in the same direction, and consequently, when they become visible from the earth, would all appear to

emanate from one point and move towards the opposite. Now although the observations seem to shew that the predominating direction is from north-east to south-west, yet shooting-stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions. Secondly, their average velocity (especially as determined by Wartmann) greatly exceeds that which any body circulating about the sun can have at the distance of the earth. Thirdly, from their appearance and the luminous train which they generally leave behind them, and which often remains visible for several seconds, sometimes for whole minutes, and also from their being situated within the earth's shadow, and at heights far exceeding those at which the atmosphere can be supposed capable of supporting combustion, it is manifest that their light is not reflected from the sun; they must therefore be self-luminous, which is contrary to every analogy of the solar system. Fourthly, if masses of solid matter approached so near the earth as many of the shooting-stars do, some of them would inevitably be attracted to it; but of the thousands of shooting-stars which have been observed, there is no authenticated instance of any one having actually reached the earth. Fifthly, instead of the meteors being attracted to the earth, some of them are observed actually to rise upwards, and to describe orbits which are convex towards the earth; a circumstance of which, on the present hypothesis, it seems difficult to give any rational explanation.

5. The most recent hypothesis is that of Capocci of Naples, who regards the aurora borealis, shooting-stars, aerolites, and comets, as having all the same origin, and as resulting from the aggregation of cosmical atoms, brought into union by magnetic attraction. He supposes that in the planetary spaces there exist bands or zones of nebulous particles, more or less fine, and endued with magnetic forces, which the earth traverses in its annual revolution; that the smallest and most impalpable of these particles are occasionally precipitated on the magnetic poles of our globe, and form polar auroras; that the particles a degree larger, in which the force of gravitation begins to be manifested, are attracted by the earth and appear as shooting-stars; that the particles in a more advanced state of concretion give rise in like manner to the phenomena of fire-balls, aerolites, &c.; that the comets, which are known to have very small masses, are nothing else than the largest of the aerolites, or rather *uranolites*, which in course of time collect a sufficient quantity of matter to be visible from the earth. This theory of Capocci differs from Chladni's only by the introduction of magnetic forces among the particles, and it is obvious that all the objections to the former theory apply with equal force to this. It may be remarked, however, that some physical connexion between the phenomena of shooting-stars and aurora had been already suspected, and the observations adduced by M. Quetelet afford reason to suppose that the latter phenomenon is also periodical.

From the difficulties attending every hypothesis which has

hitherto been proposed, it may be inferred how very little real knowledge has yet been obtained respecting the nature of the shooting-stars. It is certain that they appear at great altitudes above the earth, and that they move with prodigious velocity; but every thing else respecting them is involved in profound mystery. From the whole of the facts M. Wartmann thinks that the most rational conclusion we can adopt is, that the meteors probably owe their origin to the disengagement of electricity, or of some analogous matter, which takes place in the celestial regions on every occasion in which the conditions necessary for the production of the phenomena are renewed.

The concluding part of the paper contains an account of the different attempts which have been made to deduce differences of longitude from the observation of shooting-stars. That meteors which appear and are extinguished so suddenly, and which by reason of their great altitude and brilliancy are visible over considerable portions of the earth's surface, would afford excellent natural signals, provided they could be identified with certainty, was an obvious thought; but so long as they were regarded merely as casual phenomena, it could scarcely be hoped that they would be of much use, in this respect, to practical astronomy. As soon, however, as their periodicity became probable, the observation of the phenomena acquired a new interest. In observing the meteors for this purpose, it is assumed that they appear instantaneously to observers stationed at a distance from each other, and that the meteors seen by different observers so placed are identically the same. These points are not altogether free from uncertainty; but the results of the trials that have been already made may be regarded as favourable, and as shewing that among the other methods of determining astronomical positions, the observation of shooting-stars is not to be disregarded. At the November meeting of this Society, in 1839, an account was given of Professor Schumacher's observations at Altona on the night of the 10th of August, 1838. On the same night, corresponding observations were made at several observatories in Germany; but those at Breslaw appear to have been the most successful. From twelve coincident observations at Altona and Breslaw, Professor Boguslawski computed the difference of longitude of the two places to be $28^{m}\ 22^{s}{\cdot}07$, which differs less than a second from that which had been previously adopted. In Silliman's *American Journal* for October 1840, an account is given of simultaneous observations made on the 25th of November, 1835, at Philadelphia, and at the College of New Jersey, at Princeton. Seven coincidences were observed, and the mean result gave a longitude differing only $1^{s}{\cdot}2$ from the mean of other determinations; the whole difference being two minutes. This appears to have been the first actual determination of a difference of longitude by meteoric observations. In the corresponding observations of Wartmann and Reynier at Geneva and Planchettes, the differences of longitude deduced from three of the meteors, which were attended with peculiarities so remarkable as to leave no doubt of their identity,

were respectively 2^m, $2^m\ 3^s$, $2^m\ 5^s$, whence it would seem that a single observation may be in error to the amount of several seconds of time. In the *Bibliothèque Universelle de Genève* for August 1840, there is given an account of the determination by this method of the difference of longitude between Rome and Naples. The corresponding observations were begun in November 1838, and were continued at intervals under the direction of Father Vico at Rome, and of Capocci and Nobili at Naples. The apparent paths of the meteors were traced on a celestial globe, and the times of appearance and extinction compared with clocks regulated by astronomical observations. The observed times of the extinction of the phenomena presented a very satisfactory agreement, inasmuch as it is stated that there was in general a difference of only a few tenths of a second of time between the partial results for a difference of longitude amounting to $7^m\ 5^s{\cdot}7$.

The merit of first suggesting the use of shooting-stars and fire-balls as signals for the determination of longitudes is claimed by Dr. Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr. Baily, however, has pointed out a paper published by Dr. Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phenomena. The paper, which is printed on a single sheet, is entitled "A Plan for observing the Meteors called Fire-balls, by Nevil Maskelyne, D.D., F.R.S., and Astronomer-Royal," and is dated Greenwich, November 6th, 1783. After recounting some observations, from which he infers that such meteors appear more frequently than is commonly imagined, and stating the particulars to be attended to in observing them, he adds:—

"It would be well if those persons who happen to see a meteor would put down the time by their watch when it first appeared, or was at its greatest altitude, or burst, or disappeared, and again when they hear the sound; and as common watches are liable to vary much in a few hours, that they would, as soon after as may be, find the error of their watch by a good regulator; for, if *the exact time could be had at different places*, the absolute velocity of the meteor, *the velocity of the sound propagated to us from the higher regions of the atmosphere, and the longitudes of places, might be determined.*"

Erratum in Last Monthly Notice.

Page 68, line 12 from foot, *for* 59° 20′ 24″, *read* 59° 20′ 54″.

London:—Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *February* 12, 1841. No. 12.

Report of the Council of the Society to the Twenty-first General Annual Meeting, held this day.

The recurrence of this annual meeting of the Society affords the Council its usual opportunity of referring to the principal events of the past session. These, although few in number, are full of interest to those who prosecute the varied branches of astronomical science. For, since our last anniversary, a comet has been discovered at Berlin, by that indefatigable astronomer, M. Galle, which appears to be the same that was observed in the year 1097, and again in the year 1468; thus performing its revolution in about 371 years. And, more recently, another comet has been discovered also at Berlin, by M. Bremicker; which is now visible to those who are possessed of a powerful telescope, but which, however, from the elements at present deduced, has never been previously seen by any human being. The Council cannot but congratulate the Society that this branch of the science appears now more likely to be prosecuted than it has recently been, and thus lead to discoveries that may eventually have an important bearing on our knowledge of the laws and physical constitution of the universe. The subject of parallax also has engaged more than ordinary attention within the last year, from the interest excited by M. Bessel's valuable observations on the double star 61 *Cygni*, which now appear to be brought to a close, and which the Council have considered of sufficient importance to entitle him to the award of the medal of this Society, as will be fully explained by the President in his address at the close of this meeting.

The Council have the satisfaction of stating the flourishing condition of the finances of the Society. It appears from the Report of the Auditors that, after all the claims on the Society have been discharged, including the cost of the volume of *Memoirs* recently published, and after the investment of the compositions from members who have compounded for their annual payments (a system which has always been rigidly adhered to), there is still a balance in the hands of the Treasurer of nearly 450*l*. This state-

ment, however, will more clearly appear from the following abstract from the Report of the Auditors above alluded to.

RECEIPTS.

	£.	s.	d.
Balance of last year's Account	382	15	11
1 year's dividend on £500 Consols	15	0	0
½ year's ditto on £1894 6s. 9d. New 3½ per Cents	33	3	0
½ year's ditto on £1915 9s. 5d. ditto	33	10	4
Sale of Memoirs, by J. Weale, from Dec. 1838 to March 1840	80	0	2
Ditto by Harrison and Co. from Jan. to Nov. 1840	18	14	6
Ditto by J. Hartnup, from Nov. 1840 to Jan. 1841	32	2	0
On account of arrears of contributions	88	4	0
76 annual contributions (1840–1841)	159	12	0
6 compositions	117	12	0
13 admission fees	27	6	0
9 first year's contributions	13	13	0
	£1001	12	11

EXPENDITURE.

				£.	s.	d.
Purchase of £105 9s. 11d. New 3½ per Cents				105	0	0
J. Basire, for printing plates 1–6 of Vol. XI.				12	7	6
W. Wyon, for 3 gold medals and cases				35	0	6
C. Tuckett, for book-binding				18	1	3
J. W. Rumfitt, for ditto				2	9	0
Harrison and Co. for stationery				7	8	6
Ditto, for commission on collecting				11	1	0
Moyes and Barclay, for printing Vol. XI.				168	19	3
Ditto, for printing Monthly Notices, Vol. V. Nos. 1–8, &c.				29	17	6
H. D. Smith, for floor-cloth for the ante-room				3	16	0
J. Hartnup, for commission on collecting				4	8	9
1 year's salary to the assistant-secretary				80	0	0
Coals, candles, &c.				11	13	0
Tea, sugar, cakes, &c. for the evening meetings				13	13	0
Postage of letters				9	4	7
Charges on books, and carriage of parcels				5	10	10
Porter's and charwoman's work, &c.				8	18	2
Sundry disbursements by the treasurer				7	13	7
Taxes: Window duty	5	4	9			
Poor's rate	5	2	0			
Land tax	3	2	6			
Sewer's rate	0	12	9			
Church and rector's rate	3	12	3			
				17	14	3
Balance in the hands of the treasurer (Jan. 29, 1841)				448	16	3
				£1001	12	11

The assets and present property of the Society are as follow:—

				£.	s.	d.
Balance in the hands of the Treasurer				448	16	3
Arrears 29th January, 1841.						
1 contribution of five years' standing	£10	10	0			
1 ——— of four ditto	8	8	0			
3 ——— of three ditto	18	18	0			
5 ——— of two ditto	21	0	0			
37 ——— of one ditto	77	14	0			
				136	10	0

1936*l.* 16*s.* 5*d.* 3½ per Cent Annuities } valued at £2400 0 0
500*l.* 3 per Cent Consols .... }
In the hands of J. Weale, on account of Memoirs sold by him 9 18 3

1 Gold Medal unappropriated.
Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

Amongst the instruments here alluded to, the Council have the satisfaction of bringing before the notice of the members, the valuable joint present, from Sir John Herschel and his aunt Miss Caroline Herschel, of a seven-feet reflecting telescope made by the late Sir William Herschel, and used by her in many of her observations. This token of respect and attention will be duly estimated by the Society, who will doubtless preserve this interesting memorial of science with more than ordinary care. With respect to the other instruments belonging to the Society, they are all disposed of, in the manner stated in the last Report, with the exception of the standard scale, which has been returned by Mr. Simms, into Mr. Baily's custody, to await the further decision of the Council.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year, viz.

	Compounded.	Annual Contributors.	Non-resident.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1840	85	133	89	5	312	38	350
Since elected	3	8	...	1	12	1	13
Deceased	...	−5	...	...	−5	−3	−8
Resigned	...	−1	...	...	−1	...	−1
Removals	3	−2	−1	...	...	...	...
Expelled	...	−6	...	...	−6	...	−6
February 1841	91	127	88	6	312	36	348

As connected with the finances of the Society, it may be proper here to state that a new arrangement has been made with respect to the collection of the annual contributions of those members who have not compounded. It has been considered most convenient to the members that such contributions should be received at the apartments of the Society; and Mr. Hartnup, the assistant-secretary, has, consequently, been empowered by the Council to receive the annual contributions, and to apply to such members as may be in arrears.

The eleventh volume of the *Memoirs* of the Society has been

published since the last anniversary; and the Council trust that it will be found of equal interest to any that have preceded it. And here it may be also proper to announce that the sale of the several volumes of the *Memoirs* is now entrusted to Mr. Hartnup, who will supply the same to the respective members that may apply for them, at the usual reduced price.

Amongst the losses by death, during the past year, the Council have to regret the decease of Capt. Drummond. Dr. Gregory, Prof. Leybourn, and Mr. Best, on the home list; and MM. Olbers, Poisson, and Littrow, on the foreign list.

Capt. Thomas Drummond was born at Edinburgh in October 1797, and entered the corps of Royal Engineers in July 1815. In this department his talent for mechanical combinations became conspicuous, which, together with his close attention to the study of chemistry, rendered his services of considerable value. In 1819, he took part in the trigonometrical survey which was then carrying on in Great Britain, under the superintendence of Col. Colby: and whilst in this employment, he suggested the happy idea of applying the brilliant lamp, which goes under his name, to rendering visible the distant stations. An account of this valuable invention is printed in the *Philosophical Transactions for* 1820.

Capt. Drummond was an early member of this Society, and took great interest in promoting its welfare. During the comparisons of the standard scale of the Society with the parliamentary standard, he frequently attended the meetings of the Committee, and aided them with his opinion and advice on several important points. His previous knowledge and experience on such subjects had been well matured by the laborious and extensive comparisons of the new standard bars, used in the trigonometrical survey, that had just before been carried on at the Tower, principally under his management; and where he had alternately to encounter the heat of an oven and the cold of an ice-house: no method being left untried for attaining the greatest degree of accuracy, both in the measures themselves, and in determining the rate of expansion.

It is supposed that the first shock, which Capt. Drummond's constitution received, was in laying down the base-line at Loch Foyle, in Ireland; where he was oftentimes exposed to the inclemencies of the weather, and himself frequently standing in deep water, earnestly intent on the operations that were then carrying on, as a foundation for the future survey of the country. In fact, he entered with so much ardour and zeal into whatever he undertook, that he might be considered the life and soul of every enterprise in which he was engaged. But he was withdrawn from his geodesical pursuits by the Lord Chancellor (Brougham), who placed him at the head of the Boundary Commission that was established as a preliminary to the Reform Bill. In this new employment Capt. Drummond exerted himself with his usual zeal and ability, which led to a more intimate connexion with the ministers then in power: and he was, in April 1832, appointed private-secretary to

Lord Althorp, then Chancellor of the Exchequer. In this situation he aided the cause of this Society, and was the principal means of our obtaining the grant of the present apartments which we now occupy. In July 1835, he was appointed under-secretary to the Earl of Mulgrave, Lord Lieutenant of Ireland. The arduous duties of his office, united with other employments which he undertook, more especially as first commissioner of the Irish Railway Commission, in which his usual good sense and indefatigable exertions were manifest, proved too much for his physical strength. His constitution gradually gave way, and it was soon apparent that the scene was about to close upon him for ever. At length, " in the plenitude of mental power and the maturity of knowledge, beloved in private and esteemed in public life," he expired on the 15th of April last, and was followed to the grave by thousands who revered his memory and mourned his loss.

Dr. Olinthus Gregory was one of the earliest members of the Society, and for some time held the office of secretary. Though educated in the old English school of mathematics, his acquaintance with the continental methods was much more extensive than would have appeared from his writings, which were almost altogether intended for those who had studied the older English writers. He knew generally what was going on abroad, particularly in the extensions of geometry made by the school of Monge. As editor of the well-known Ladies' and Gentleman's Diaries, he was brought into communication with young students who were desirous of distinguishing themselves in the exact sciences. The protection and encouragement which he afforded to those who were pursuing the path which he himself had trodden, will be gratefully remembered by many; and the period of his superintendence of those useful works will be remembered as that in which every contributor of merit found a friend in the editor. His manners were altogether in accordance with what might have been expected from the preceding account; all he did and said was dictated by benevolence of feeling.

Dr. Gregory's occupations were numerous and engrossing, and his power of application was very great. About thirteen years ago, he was attacked with severe illness, from which it was hardly expected that he would finally recover. Although more or less of an invalid from that time till his death, he continued his numerous avocations with all but, if not altogether, his accustomed energy. On the removal of the tax on almanacs, he was the first to see that the publications which he superintended might be made still more useful in diffusing the spirit of scientific inquiry; and, from that time till his death, original treatises, or useful reprints, were made to form part of them.

The writing by which he is best known to the public at large is his letters on the evidences of Christianity, a work of large circulation. His treatise on mechanics had a considerable celebrity, and was translated into German; it is not improbable that some here present may have learned their first ideas of that science from it.

His edition of Dr. Hutton's Course of Mathematics was considerably augmented by himself, and is the best of them all. As an accurate observer, he is known by his experiments on the velocity of sound, which agree almost exactly with those of the first French and three Dutch observers, who were almost simultaneously employed on the same subject.

We have frequently to record losses which bear more directly on practical astronomy, but it is not often that we have to regret the termination of a more energetically useful career, and never of a more zealous one. The principal works known to have been written or edited by him are as follow:

1793. Lessons, Astronomical and Philosophical, 1 vol.
1801. Treatise on Astronomy, 1 vol.
1802. The Gent's Diary, under his editorship.
1806. Treatise on Mechanics, 3 vols.
1807. Translation of Haüy's Natural Philosophy, 2 vols.
1808. Pantalogia, of which he was the general editor, and the contributor of about one half, 12 vols.
1810. Third volume of Dr. Hutton's Course of Mathematics, of which he wrote about one half; and he afterwards edited an edition of the whole course.
—— Letters on the Evidences, &c. of Christianity, 2 vols.
1815. Tracts on the Trigonometrical Survey.
1816. Plain and Spherical Trigonometry, 1 vol.
—— Dissertation on Weights and Measures.
1817. An account of his Pendulum Experiments and Astronomical Observations made at Shetland, in the Philosophical Magazine.
1818. Appointed editor of the Ladies' Diary and general superintendent of the Stationers' Company's Almanacs.
1825. Mathematics for practical Men, 1 vol.
1839. Address to the Cadets of the Royal Military Academy on resigning the Professor's Chair.
1840. Hints to Mathematical Teachers, 1 vol.
—— Tables to be used with the Nautical Almanac.

He was also, at one period of his life, a large contributor to the leading reviews.

Thomas Leybourn, Esq. Fellow of the Royal Society, late senior Professor of Mathematics in the Royal Military College, was one of the original members of this Society, having been admitted by the founders in 1820. Mr. Leybourn was born in 1769, at Bishop-Middleham, in the county of Durham. Although he had not the advantages of a regular education, he appears to have devoted himself to mathematical studies from an early period of life; for, soon after 1790, he was a correspondent of Dr. Hutton in the *Ladies' Diary;* and, in 1795, he became known to the public as the editor of the *Mathematical Repository*—a publication which made its appearance at irregular intervals, and of which the object was to afford a channel for giving publicity to lucubrations

of greater length, and to the solutions of problems of a higher order of difficulty, than could be admitted into the Diaries. At that time he was employed as land-agent on the estate of a nobleman in Wales. In 1802 he was appointed, on the recommendation of Dr. Hutton, one of the mathematical instructors in the Military College then recently established at Great Marlow—an office which he continued to fill with much ability and advantage to the institution, until within a few months before his death, which took place in March 1840.

The first series of the *Mathematical Repository* terminated in 1802, when the work had extended to five duodecimo volumes. Another series was begun in 1803, which was continued, though with some long intermissions, until 1835, when it had extended to six volumes 8vo. To this publication some of the most distinguished mathematicians of the country were occasional contributors; and although, as in all other periodicals, the contributions are of very unequal merit, the work is on the whole valuable to the student of geometry, particularly the earlier volumes of it, which contain many very beautiful specimens of the application of the ancient geometrical analysis. Besides the *Mathematical Repository*, Mr. Leybourn was editor of the "Questions Proposed in the *Ladies' Diary*," published in 1817, in four vols. 8vo.; and of the *Gentleman's Diary* from 1824 till his death. To these several works he contributed little of his own; but his merit in bringing them forward, and the liberality he shewed in defraying the expense of their publication (for, with the exception of the *Gentleman's Diary*, they were all attended with a considerable pecuniary sacrifice), will secure for his name the respect of those who wish well to the propagation of useful knowledge.

Mr. Leybourn's distinguishing merit, however, consisted in the ability and zeal with which, during the long period of thirty-seven years, he discharged his laborious public duties in the Military College. As an instructor he was highly respected and esteemed by the governors of the institution, and much beloved by his pupils, in whose progress and advancement he took the warmest interest. He was a man of a kind and benevolent disposition; ready at all times to extend a helping hand to those who stood in need of assistance, more especially if they came recommended to him as possessing a taste for the mathematics.

Richard Best, Esq. was one of the earliest members of this Society, and, while his health permitted, he was a regular attendant at the monthly meetings. He had a sort of hereditary connexion with our science through his mother, who was a niece of Dr. Bradley, being the daughter of his sister Rebekah, the wife of Mr. John Dallaway; and he possessed some books and instruments which had once been the property of that distinguished astronomer. The popular branches of astronomy were to him a source of amusement; he was a diligent observer of telescopic phenomena, and he occasionally furnished an account of them to the periodicals of the day. He was greatly respected in his own neighbourhood, having

long been treasurer to most of the local charities, and a liberal supporter of them all. He was a native of Greenwich, where he resided till within a few months of his death; and, for the last thirty-five years, lived in a house which was built by his grandfather, and occupied by him for the first time on the day his son, Mr. Best's father, came of age.

Mr. Best retired to Henly-on-Thames, where he died on the morning of May 19, 1840, the day on which he would have completed his seventy-third year, regretted by all who had the pleasure of his acquaintance.

The important discovery of two new planets in our system (*Pallas* and *Vesta*) has rendered the name of Olbers familiar to every lover of astronomy. The circumstances that led to this discovery were as singular as they were fortunate; and shew the happy results that may arise from a zealous association of individuals in the steady pursuit of some definite object. The detail of those circumstances has been so recently given in the obituary of this distinguished astronomer and mathematician, read at the last anniversary meeting of the Royal Society, that it appears almost needless to repeat them in this place. Yet it may here be stated, that the discovery of *Pallas* at nearly the same distance from the sun as *Ceres* (which had been discovered in the preceding year) led Olbers to conjecture that they were fragments of a larger planet, which might have been scattered by some great catastrophe, and that, probably, some other portions of the original mass might be found in nearly the same orbit. His diligence was rewarded by the discovery of *Vesta*, about five years afterwards; and nearly in the position in which he expected it would be found. The instrument with which he made these discoveries was a very small telescope, and his observatory was a room in the upper part of his house; thus shewing to the world with what slender means the most important results may be obtained. But Olbers was not merely a practical astronomer; his treatise on the best mode of determining the orbit of a comet, and his improvements and investigations of various astronomical formulæ, exhibit him as a mathematician of considerable talent. One of the latest papers relative to astronomy, on which he was employed prior to his death, was on a reform of the constellations, both as to their nomenclature and the arrangement of the stars which limit their boundaries: to which subject he was excited by a passage in the Report of the Council at our last anniversary, wherein it was stated that a revision of this kind was about to be made under the superintendence of a committee appointed by the British Association. The views of Olbers are in perfect coincidence with the object proposed; and in the paper above alluded to, and which has been transmitted to the President, he laments the confusion that has been introduced by his predecessors, and suggests some useful hints for a remedy.

Simeon Denis Poisson (born June 21, 1781, died April 25, 1840) was placed, by common consent, at the head of European

analysts on the death of Laplace. He was of humble birth, and was admitted in 1793 a student of the Ecole Polytechnique, then newly established. It is stated, by the historian of this school, that, at the age of eighteen, he submitted to his professor some ameliorations in the method of demonstrating the binomial theorem; which that teacher, who was no other than Lagrange, read publicly at his next lecture, and which he declared his intention of adopting in future.

The life of Poisson was one of quiet and uninterrupted study. He never held any situation connected with politics, nor was in any way, during thirty years, prevented from pursuing his one great object, the application of the most abstruse and newest developements of the integral calculus to problems of physics. The number of his memoirs is enormous; to which must be added his elementary treatise on mechanics (which stands at the head of all elementary writings on the application of pure analysis to the properties of matter), his treatises on capillary attraction, on heat, and on the theory of probabilities.

It is well known that the energies of Euler, Clairaut, D'Alembert, and the younger Bernoullis, had organised the application of mathematics in a manner which made the subsequent triumphs of Lagrange and Laplace seem almost beyond expectation. The power of the pure mathematics seemed to flag, when Fourier first came forward with his applications of definite integrals and periodic series to questions of physics, which seemed to be unconquerable, and of which the difficulties seemed to be altogether inexpressible, by ordinary analysis. A new school of mathematicians was rapidly formed, in whose hands the mode of expression by definite integrals added one more to the instances in which the happy enunciation of questions was all but their solution. Poisson was one of the first of this school in point of time, and by far the greatest in power. Throughout the major part of his writings we trace the same capability of explaining the most abstruse points with fluent clearness and rigid accuracy, combined with that of conquering the physical difficulties of his problem by the most happy art of adaptation.

Many of his memoirs are on the great questions of physical astronomy, and it is here that he shews that he was not the accident of a fortunate epoch, but that he could handle the instruments of his two great predecessors with skill resembling their own. Perhaps his greatest achievement in this line is the extension of our knowledge respecting the stability of the solar system, as far as it may be affected by perturbations of the mean orbital motions, or of the axical rotations. This question does not, as many imagine, owe all its interest either to the predictive power which is sought, or to the grandeur of the problem considered as the path to such a power. It is to be remembered that the connecting constants between the oldest and most recent astronomy are the lengths of the sidereal year and of the day; and that we cannot assume to

talk a common language with Hipparchus and Ptolemy, unless we have reason to know that these elements continue sensibly unaltered. In addition to the imperfect presumptions derived from observation (imperfect on account of the large liability to error of the older astronomers) Lagrange had shewn that the mean motions have no secular inequalities depending on the first power of the disturbing forces; or, so far as this first power was concerned, or any powers of the eccentricities or inclinations. Laplace had shewn that a certain secular equation, which should in theory be applied to the sidereal day, would always be too small to be of any importance. Poisson extended the conclusions of Lagrange to the second power of the disturbing forces, and, relatively, to any powers of the eccentricities and inclinations; or rather, we may say, that he shewed any secular equation of the mean motions to depend only on the fourth power of disturbing forces: for, in the course of the investigation, it appears that no such equation of any odd order can exist. As far as the fourth powers of eccentricities and inclinations, he actually shews the mutual destruction of an infinite number of non-periodic disturbing terms; the rest of the powers are completed by a general and different investigation. In the problem of the rotation of the earth, he generalises the investigation of Laplace, by taking into consideration the actual change of the axis *on the earth;* the former investigation considering only the change of the axis, supposed to be fixed in the earth, relatively to the stars. The result agrees with that of Laplace as to the non-existence of any sensible secular inequality.

Poisson belongs to a class of investigators of whom many are always wanted, but one is permanently indispensable.

The applications of the newest powers of mathematical language should always be made as near as may be to the time when they are first exhibited. But such peculiar combination of address and power, with a perfect knowledge of existing materials, is not of every-day occurrence; nor is it permitted to say, *uno avulso non deficit alter*.

His majesty the King of Denmark, who was elected an honorary member of this Society at the last anniversary, has been pleased to continue the offer of the gold medal founded by his royal predecessor, for the first discovery of a telescopic comet; subject to the conditions and regulations already inserted in the *Monthly Notices* of this Society for November 1835. One of these conditions (which is indispensable as far as it affects persons resident in Great Britain) is, that notice of such discovery must be sent by the *first* post to Mr. Baily.

The Council cannot conclude this Report without expressing a hope that the future efforts of the Society for the promotion and encouragement of astronomy, will be marked with the same success as has hitherto attended their career: and that they may continue to shew to the world the happy effects of unanimity and zeal in the pursuit of a favourite science.

Titles of Papers read before the Society, between February 1840 and February 1841.

1840.

Mar. 13. On the Regulator of the Clock-work for effecting uniform Movement of Equatoreals. By G. B. Airy, Esq., Astronomer-Royal.

Note on an Arabic Globe belonging to the Society. By R. W. Rothman, Esq. M.A., Foreign Secretary.

Elements of Galle's Second Comet; computed by M. Peterson, and communicated by Professor Schumacher.

April 10. Observations made at the Cape of Good Hope, in the year 1838, with Bradley's Zenith Sector, for the Verification of the Abbé de Lacaille's Arc of the Meridian. By Thos. Maclear, Esq. F.R.S. Communicated by the Lords Commissioners of the Admiralty.

The Longitude of Madras, computed from Moon-Culminating-stars. By E. Riddle, Esq.

Ephemeris and Elements of the third Comet discovered by Galle. By Mr. Rumker, Superintendent of the Observatory at Hamburg. Communicated by Dr. Lee.

May 8. On the Present State of our Knowledge of the Parallax of the Fixed Stars. By the Rev. R. Main.

June 12. Continuation of the Investigation for the Correction of the Elements of the Orbit of *Venus*. By Mr. Glaisher, of the Royal Observatory, Greenwich.

An Account of some Experiments made with an Invariable Pendulum, at the Cape of Good Hope. By T. Maclear, Esq.

An Account of some Experiments made with three Invariable Pendulums, by Lieut. Murphy, R.E., during the late Expedition down the Euphrates. By Mr. Baily.

The Elements of the Annular Eclipse of the Sun that will happen on October 8th, 1847. By Mr. George Innes.

On the Comparison of the Neapolitan Standard Yard with the Standard Yard of this Society. By Mr. Simms.

On the Difference of Longitude between the Observatories of Madras and the Cape of Good Hope, deduced from Moon-Culminating Stars. By T. Maclear, Esq.

Nov. 13. A Letter from Mr. Dawes on the subject of a new Binary Star recently observed.

A Supplemental Catalogue of the Right Ascensions of Fifty-five Stars contained in the Royal Astronomical Society's Catalogue. By the Hon. John Wrottesley.

Postscript to Mr. Baily's Report on Mr. Maclear's Pendulum Experiments. By Mr. Baily.

Observations of the Second Comet of 1840, made at the Observatory at Hamburg. By Mr. Rumker.

1840.
Dec. 11. On a large Achromatic Object-glass of a Telescope worked by Mr. Dollond, the flint-glass of which was prepared by the late Dr. Ritchie. By the Rev. Samuel King, M.A. F.R.A.S.

Description of a Method of Dividing one Circle, B, by copying from another, A, previously divided. By Lieut.-Col. Everest, Director of the Trigonometrical Survey of India.

Transits observed at Washington (United States) from January 1st to July 1st, 1840; and Occultations observed at the same place, since June 1839. By J. Melville Gilles, Esq.

Places of Bremicker's Comet, as determined with the Equatoreal Telescope at Mr. Bishop's Observatory. By the Rev. W. R. Dawes.

1841.
Jan. 8. Remarks on the Present State of our Knowledge relative to Shooting-Stars, and on the Determination of Differences of Longitude from Observations of those Meteors. By Mr. Galloway.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

American Philosophical Society.
Society of Arts.
Royal Asiatic Society.
Asiatic Society of Bengal.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences at Brussels.
L'Académie Royale des Sciences de l'Institut de France.
Royal Geographical Society.
Geological Society of London.
Royal Irish Academy.
Linnean Society of London.
Royal Society of London.
Numismatic Society.
Italian Society.
Editor of the Athenæum Journal.
Royal Observatory of Brussels.
L'Académie de Dijon.
The Observatory at Milan.
Her Majesty's Government.
Zoological Society of London.
Imperial Academy of Sciences at St. Petersburg.
The Hon. East India Company.
Administration des Mines de Russie.

J. Caldecott, Esq.	Sir J. W. Lubbock, Bart.
Rev. J. Challis.	O. F. Mossotti.
Professor Encke.	Professor Quetelet.
T. Forster, Esq.	Lieut. Raper.
W. D. Haggard, Esq.	G. Rennie, Esq.
J. O. Halliwell, Esq.	Rev. Dr. Robinson.
J. Hancock, Esq.	Professor Schumacher.
G. Innes, Esq.	Capt. Smyth.
Professor Hassler.	T. G. Taylor, Esq.
J. Herrapath, Esq.	R. Taylor, Esq.
Sir J. F. W. Herschel, Bart.	Colonel Visconti.
Miss Caroline Herschel.	F. G. W. Struve.
Dr. J. Lamont.	Lieut. Stratford.
Professor Littrow.	J. N. Vallot.
J. Lockhart, Esq.	T. G. Western, Esq.

The President (Sir J. F. W. Herschel, Bart.) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council has placed before you so ample a view of the state of the Society, of its labours during the last year, of the accessions to its members, and of the many and severe losses it has had to deplore, that little is left for me to add, except my congratulations on its continued and increasing prosperity. It would be inexpressibly gratifying to me if I could persuade myself that my own exertions in its chair had contributed, even in a small degree, to that prosperity; but, alas! I have felt only too sensibly how very feebly and inefficiently, especially during the last year, owing to a variety of causes, but chiefly to residence at a distance from London, I have been able to fill that most honourable office.

The immediate object of my now addressing you, gentlemen, is to declare the award by your Council of the gold medal of this Society to our eminent associate, Mr. Bessel, for his researches on the annual parallax of that remarkable double-star 61 *Cygni*,—researches which it is the opinion of your Council have gone so far to establish the existence and to measure the quantity of a periodical fluctuation, annual in its period and identical in its law with parallax, as to leave no reasonable ground for doubt as to the reality of such fluctuation, as something different from mere instrumental or observational error: an inequality, in short, which, if it be *not* parallax, is so inseparably mixed up with that effect as to leave us without any criterion by which to distinguish them. Now, in such a case, parallax stands to us in the nature of a *vera causa*, and the rules of philosophizing will not justify us in referring the observed effect to an unknown and, so far as we can see, an inconceivable cause, when this is at hand, ready to account for the whole effect.

I say, in the nature of a *vera causa*, since each particular star must of necessity have some parallax. Every *real, existing, material* body, must enjoy that indefeasible attribute of body, viz. *definite place*. Now place is defined by *direction* and *distance* from a fixed point. Every body, therefore, which does exist, exists at a certain definite distance from us and at no other, either more or less. The distance of every individual *body* in the universe from us is, therefore, necessarily admitted to be finite.

But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have *measured* it in any one case. To accomplish this, has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct apprehension, and so leading him on in hopeless, endless, and exhausting pursuit.

The pursuit, however, though eager and laborious, has been far from unproductive even in those stages where its immediate object has been baffled.

The fact of a periodical fluctuation of *some kind* in the apparent places of the stars was recognized by Flamsteed, and erroneously attributed to parallax. The nearer examination of this phenomenon with far more delicate instruments; infinitely greater refinement of method, and clearer views of the geometrical relations of the subject, rewarded Bradley with his grand discoveries of aberration and nutation, and enabled him to restrict the amount of possible parallax of the stars observed by him within extremely narrow limits.

Bradley failed to detect any appreciable parallax, though he considered 1″ as an amount which would not have escaped his notice. And since his time this quantity has been assumed as a kind of conventional limit, which it might be expected to attain but hardly to surpass. But this was rather because, in the best observations from Bradley's time forward, 1″ has been a tolerated error; a quantity for which observation and mechanism, joined to atmospheric fluctuations and uncertainties of reduction, could not be held rigidly accountable even in mean results; than from any reason in the nature of the case, or any distinct perception of its reality. If parallax were to be detected at all by observations of the absolute places of the stars, it could only emerge as a "residual phenomenon," after clearing away all the effects of the uranographical corrections as well as of refraction, when it would remain mixed up with whatever uncertainties might remain as to the coefficients of the former, with the casual irregularities of the latter, and with all the forms of instrumental and observational error. Now these have hitherto proved sufficient, even in the

observation of zenith stars, quite to overlay and conceal that minute quantity of which astronomers were in search.

It is not my intention, gentlemen, to enter minutely into the history of the attempts of various astronomers on this problem, whether by the discussion of observations of one star, or by the combination of those of pairs of stars opposite in right ascension; nor with the occasional gleams of apparent success which, however, have always proved illusory, which have attended these attempts. For such a history, and, indeed, for a complete and admirably drawn up monograph of the whole subject, I must refer to a paper lately read to this society by Mr. Main, and which is now in process of publication in the forthcoming volume of our *Memoirs*. In whatever reference I may have to make to the history of the subject, I must take this opportunity to acknowledge my obligations to the author of this paper, as well as for his exceedingly luminous exposition of the results of those more successful attempts on the problem by Henderson, Struve, and Bessel, which I shall now proceed more especially to consider.

It would be wrong, however, not to notice that the first indication of some degree of impression beginning to be made on the problem seems to be found in Struve's discussion of the differences of right ascension of circumpolar stars in 1819, 20, and 21. The only *positive* result, indeed, of these observations is, that in the case of twenty-seven stars examined, none has a parallax amounting to half a second. But *below* this, there certainly do seem to be indications in the nature of a real parallax, which might at least suffice to raise the sinking hopes of astronomers, and excite them to further efforts.

But the time arrived when the problem was to be attacked from a quarter offering far greater advantages, and exposed to few or none of those unmanageable sources of irregular error to which the determinations of absolute places are liable. I mean by the measurement of the distances of such double stars as consist of individuals so different in magnitude as to authorize a belief of their being placed at very different distances from the eye; or, as Struve expresses it, *optically* and not *physically* double. This, in fact, was the original notion which led to the micrometrical measurements of double stars; but not only was anything like a fair trial of the method precluded by the imperfections of all the micrometers in use until recently, but the interesting phenomena of another kind, which began to unfold themselves in the progress of those measurements, led attention off altogether from this their original application, which thus lay dormant and neglected, until the capital modern improvements, both in the optical and mechanical parts of refracting telescopes, and the great precision which it was found practicable, by their aid, to attain in these delicate measurements, revived the idea of giving this method, what it never before had, a fair trial. The principle on which the determination of parallax by means of micrometical observations of a double star turns, is extremely simple. If we conceive two stars very nearly in a line

with the eye, but of which one is vastly more remote than the other, each, by the effect of parallax, will appear to describe annually a small ellipse about the mean place as its centre. These two ellipses, however, though similar in form will differ in dimension; that described by the more remote star being comparatively much smaller: consequently, the apparent places being similarly situated in each, their apparent distance on the line joining these apparent places will both oscillate in angular position and fluctuate in length, thus giving rise to an annual relative alternate movement between the individuals both in position and distance, which is greater the greater the difference of the parallaxes.

Thus it is not the absolute parallax of either, but the difference of their parallaxes, which is effectively measured by this method; *i. e.* by repeating the measurements of their mutual distance at all times of the year. But, on the other hand, aberration, nutation, precession, and refraction, act equally on both stars, or so very nearly so as to leave only an exceedingly small fraction of these corrections bearing on the results. And when the stars are very unequal in magnitude, there is a presumption that the difference of their parallaxes is very nearly equal to the whole parallax of the nearer one.

The selection of a star for observation involves many considerations. In that pitched on by M. Bessel (61 *Cygni*), the *large star* so designated, is in fact a fine double star; nay, one that has been ascertained to be physically double. It is in every respect a highly remarkable star. The mutual distance of its individuals is great, being about 16½″. Now this being necessarily less than the axis of their mutual orbit, affords in itself a presumption that the star is a *near one*. And this presumption is increased by the unusually great proper motion of this binary system, which amounts to nearly 5″ per annum, and which has been made by Sir James South the subject of particular inquiry, and found to be *not* participated in by several small surrounding stars, *which, therefore*, are not physically connected with it. Moreover, the angular rotation of the two, one about the other, has been well ascertained.

Now, it fortunately happens, that of these small surrounding stars there are two very advantageously situated for micrometrical comparison with either of the individuals of the binary star, or with the middle point between them. The one of these (*a*), at a distance of 7′ 42″, is situated nearly at right angles to the direction of the double star; the other (*b*) at a distance of 11′ 46″, nearly *in* that direction. Considering (*a*) and (*b*) as fixed points then, and measuring at any instant of time their distances from (*c*), the middle point of the double star, the situation of (*c*) relative to (*a*) and (*b*) is ascertained; and if this be done at every instant, the relative *locus* of (*c*), or the curve described by it on the plane of the heaven with respect to the fixed base-line *a b*, will become known.

Now, on the hypothesis of parallax, that locus ought to be an ellipse of one certain calculable eccentricity and no other. And

its major and minor axes ought to hold with respect to the points, *a*, *b*, certain calculable positions and no other. Hence it follows that the distances *a c* and *b c* will each of them be subject to annual increase and diminution; and *that*, 1st, in a given and calculable ratio the one to the other; and, 2dly, so that the maxima and minima of the one distance (*a c*) shall be nearly contemporaneous with the *mean* values of the other distance *b c*, and *vice versâ*.

Thus we have, in the first place, several particulars independent of mere numerical magnitudes; and, in the second place, several distinct relations *à priori* determined, to which those numerical values must conform, if it be true that any observed fluctuations in these distances (*a b*) (*a c*) be really parallactic. So that if they be found in such conformity, and the above-mentioned maxima and minima do observe that interchangeable law above stated; and if, moreover, all due care be proved to have been taken to eliminate every instrumental source of annual fluctuation; there becomes accumulated a body of probability in favour of the resulting parallax, which cannot but impress every reasonable mind with a strong degree of belief and conviction.

Now, all these circumstances have been found by M. Bessel, in his discussion of the measures taken by him (which have been very carefully and rigorously examined by Mr. Main in the paper alluded to, as have also M. Bessel's formulæ and calculations, for in such matters nothing must remain unverified), to prevail in a very signal and satisfactory manner. Not one case of discordance, in so many independent particulars, have been found to subsist; and this, of itself, is high ground of probability. But we may go much farther. Mr. Main has projected graphically the deviations of the distances (*a c*) and (*b c*) from their mean quantities (after clearing them of the effects of proper motion and of the minute differences of aberration, &c.). Taking the time for an abscissa, and laying down the deviations in the distances so cleared as ordinates, two curves are obtained, the one for the star (*a*), the other for the star (*b*). Each of these curves ought alternately to lie for half a year above, and for half a year below, its axis.—*It does so.* Each of them ought to intersect its axis at those dates when the maximum and minimum of the other above and below the axis occurs. With only a slight degree of hesitation at one crossing—*it does so.* The points of intersection with the axis ought to occur at dates in like manner calculated *à priori*; and so they do within very negligible limits of error. And, lastly, the general forms, magnitudes, and flexures of the curves ought to be identical with those of curves similarly projected, by calculation on an assumed resulting parallactic coefficient. This is the final and severe test: Mr. Main has applied it, and the results have been placed before you:—*oculis subjecta fidelibus*. If all this does not carry conviction along with it, it seems difficult to say what ought to do so.

The only thing that can possibly be cavilled at is the shortness

of the period embraced by the observations: viz. from August 1837 to the end of March 1840. But this interval admits of five intersections of each curve with its axis; of two maxima and two minima in its excursions on either side; and of ample room for trying its agreement in general form with the true parallactic curves. Under such circumstances, it is quite out of the question to declare the whole phenomenon an accident or an illusion. *Something* has assuredly been discovered, and if that something be not parallax, we are altogether at fault, and know not what other cause to ascribe it to.

The instrument with which Bessel made these most remarkable observations is a heliometer of large dimensions, and with an exquisite object-glass by Fraunhofer. I well remember to have seen this object-glass at Munich before it was cut, and to have been not a little amazed at the boldness of the maker who would devote a glass, which at that time would have been considered in England almost invaluable, to so hazardous an operation. Little did I then imagine the noble purpose it was destined to accomplish. By the nature and construction of this instrument, especially when driven by clock-work, almost every conceivable error which can affect a micrometrical measure is destroyed, when properly used; and the precautions taken by M. Bessel in its use have been such as might be expected from his consummate skill. The only possible apparent opening for an annually fluctuating error seems to be in the correction for temperature of its scale. But this correction has been ascertained by M. Bessel by direct observation, in hot and cold seasons, and applied. Nor could this cause destroy the evidence arising from the simultaneous observation of the two companion stars, since a wrong correction for temperature would affect both their distances proportionally, leaving the apparent parallactic movement still unaccounted for.

The resulting parallax is an extremely minute quantity, only thirty-one hundredths of a second; which would place the star in question at a distance from us of nearly 670,000 times that of the sun!* Such is the universe in which we exist, and which we have at length found the means to subject to measurement, at least in one of its members, probably nearer to us than the rest.

It becomes necessary for me now to refer to two series of researches on this important subject, which have been held by your Council to merit very high and honourable mention; though neither of them, separately, for reasons which I shall state, would have been considered as carrying that weight of probability in favour of its conclusions, which would justify any immediate decision of the nature which they have come to in the case of M. Bessel's. I allude to M. Struve's inquiries, by the method of micrometric measures, into the parallax of *α Lyræ*; and to Mr. Henderson's, by that of meridian observations, on the parallax of *α Centauri*.

* The orbit described by the two stars of 61 *Cygni* about each other will, therefore, be about 50 times the diameter of the earth's about the sun, or 2½ times that of *Uranus*.

α Lyræ is accompanied by a very minute star, at the distance of about 43″. That this star is unconnected with *α* by any physical relation, is clear from the fact ascertained by Sir James South and myself, that it does not participate in the proper motion of the large star. The mutual angular distance of these stars has been made by M. Struve the subject of a very extensive series of micrometric measures with the celebrated Dorpat achromatic, bearing this object steadily in view, and working it out to a conclusion of the very same kind, and, though materially inferior in the degree and nature of its evidence to that of Bessel, yet certainly entitled to high consideration. M. Struve's observations on this star, and for this purpose, extend from Nov. 1835 to Aug. 1838, and are distributed over sixty nights, averaging twenty per annum; and from their combination, according to the principle of probabilities, he concludes a parallax of 0″·261. Mr. Main has subjected these observations to an analysis and graphical projection, precisely similar in principle to those I have explained in the case of 61 *Cygni*. The curves so projected have been subjected to your inspection, and that inspection certainly does leave a very strong impression of a real and tolerably well-ascertained parallax *having* been detected in this star. But at the same time an impression no less decided, owing to irregularities in the march of the curve, when compared with the true parallactic curve, is created,—that the errors of observation are far from being eliminated,—that, on the contrary, they bear such a proportion to the parallax itself as to leave room for some degree of hesitation, and to justify an appeal to a longer series of observations, and to concurrent evidence from other quarters, before declaring any positive opinion. The evidence of this kind, in short, is not equal to that afforded by the similar projection of Bessel's observations of *either* of his two comparison stars. And to this it must be added, that only one star of comparison existing in the line of *α Lyræ*, the possible effect of temperature and *annual* instrumental variation is not eliminated from the result in the way in which it is from the measures of 61 *Cygni;* while all that great mutual support which the observations of parallaxes of the two comparison stars afford each other in the latter case, is altogether wanting in the former. These considerations, without any under-estimation of the great importance and value of M. Struve's researches yet formed essential drawbacks on the immediate admission of his results.

In a word, I conceive the question of discovery as between these illustrious, but most generous and amicable rivals, may be thus fairly stated. M. Struve's meridian observations in 1819–1821 seem to have made the first impression on the general problem, but too slight to authorize more than a hope that it would yield at no distant day. His micrometric measures of *α Lyræ* commenced more than a year earlier, and have extended altogether over a longer period than M. Bessel's of 61 *Cygni*. From their commencement they afford indications of parallax, and these indications accumulating with time have amounted to a high

degree of probability, and rendered the supposition of parallax more admissible than that of instrumental or casual errors producing the same influence on the measures. On the other hand, M. Bessel's measures commencing a year later, and continued on the whole through somewhat less time, have exhibited a compact and consistent body of evidence drawn from two distinct systems of measures mutually supporting each other, and so steadily bearing on their object as to leave no more reasonable doubt of its truth than in the case of many things which we look upon as, humanly speaking, certain. And this conviction once obtained, reacts on our belief in the other results, and induces us to receive and admit it on the evidence adduced for it; which, without such conviction so obtained, we might hesitate to do until after longer corroboration of the same kind.

The other series of observations to which I must now call your attention are those of Mr. Henderson, made at the Cape of Good Hope, on the great star α *Centauri*, the third star in brightness which the heavens offer to our view. It is a magnificent double-star consisting of two individuals, the one of a high and somewhat brownish orange, the other of a fine yellow colour, and each of which I consider fairly entitled to be classed in the first magnitude.* Their distance is at present about 15″ asunder, but it is rapidly diminishing, and in no great lapse of time they will probably occult one another, their angular motion being comparatively small. Their apparent distance was formerly much greater: how much we cannot say for want of observations, but probably the major axis of their mutual orbit is little short of a minute of space. They, therefore, afford strong indications of being very near our system. Add to which their proper motion is very considerable, and participated in by both, which proves their connexion as a binary system; and an additional presumption in favour of their proximity may be drawn from their situation in what, from general aspect, I gather to be the nearest region of the milky way, among an immensity of large stars.

Mr. Henderson observed these stars with great care both in right ascension and declination with the very fine transit, and (in spite of certain grievous defects in the axis) the otherwise really good and finely divided mural circle of the Royal Observatory in that colony. Since his return to England, he has reduced these observations with a view to parallax, and the result is the apparent existence of that element to what, after what has been said, we must now call the great and conspicuous amount of a full second. Mr. Main, to whom I am so largely indebted for allowing me to draw so freely on his labours, has also discussed these results, and comes to the conclusion that (as might, perhaps, be expected) the right-ascension observations afford a trace, but an equivocal one, of parallax, but that in declination (I use his words) "The law of parallax is

* I have seen *both* their images projected on a screen of three thicknesses of stout paper, the eye being on the opposite side of the screen from that on which the images were depicted.

followed remarkably well. There is scarcely an exception to the proper change of sign, according to the change of sign of the coefficients of parallax. This is quite as much as can reasonably be expected in a series of individual results obtained from any meridional instrument for observing zenith distances. We cannot expect to find the periodical function regularly exhibited by the differences. On the whole, therefore, we should say that, in addition to the claims of *α Centauri* on our attention with relation to its parallax, arising from its forming a binary system, its great proper motion, and its brightness,— it derives now much additional importance, in this point of view, from the investigation of Mr. Henderson. This we are at least entitled to assume until some distinct reason, independent of parallax, shall have been assigned for the changes in the declinations. Such I do not consider impossible, having before my eyes the results which Dr. Brinkley derived, in the cases of certain stars, from the Dublin circle. For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its first discovery, and the warmest thanks of astronomers, as an extender of the knowledge which we possess of our connexion with the sidereal system."

With this view of Mr. Henderson's labours I fully agree, and await with highly excited interest the result of Mr. Maclear's larger and complete series of observations on this star both with the old circle and with that more perfect one with which the munificence of government has recently supplied the observatory. Should a different eye and a different circle continue to give the same result, we must, of course, acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of *a* fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in so far anticipating a decision which time alone can stamp with the seal of absolute authenticity.

Gentlemen of the Astronomical Society, I congratulate you and myself that we have lived to see the great and hitherto impassable barrier to our excursions into the sidereal universe; that barrier against which we have chafed so long and so vainly— (*æstuantes angusto limite mundi*) — almost simultaneously overleaped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed. Perhaps I ought not to speak so strongly — perhaps I should hold some reserve in favour of the bare possibility that it may be all an illusion — and that further researches, as they have repeatedly before, so may now fail to substantiate this noble result. But I confess myself unequal to such prudence under such excitement. Let us rather accept the joyful omens of the time, and trust that, as the barrier has begun to yield, it will speedily be effectually prostrated. Such results are among the fairest flowers of civilization. They justify the vast expenditure of time and talent which

have led up to them; they justify the language which men of science hold, or ought to hold, when they appeal to the governments of their respective countries for the liberal devotion of the national means in furtherance of the great objects they propose to accomplish. They enable them not only to hold out but to redeem their promises, when they profess themselves productive labourers in a higher and richer field than that of mere material and physical advantages. It is then when they become (if I may venture on such a figure without irreverence) the messengers from heaven to earth of such stupendous announcements as must strike every one who hears them with almost awful admiration, that they may claim to be listened to when they repeat in every variety of urgent instance, that these are not the last of such announcements which they shall have to communicate,— that there are yet behind, to search out and to declare, not only secrets of nature which shall increase the wealth or power of man, but TRUTHS which shall ennoble the age and the country in which they are divulged, and by dilating the intellect, react on the moral character of mankind. Such truths are things quite as worthy of struggles and sacrifices as many of the objects for which nations contend, and exhaust their physical and moral energies and resources. They are gems of real and durable glory in the diadems of princes, and conquests which, while they leave no tears behind them, continue for ever unalienable.

It must be needless for me to express a hope that these researches will be followed up. Already we have to congratulate astronomy on the resolution taken by one of our great academic institutions to furnish its observatory with an heliometer of the same description as Bessel's; nor can we fear but that the research will speedily be extended to other stars, offering varieties of magnitude and other indications to draw attention to them.

On the whole, then, the award of our medal, which the Council have agreed on, seems to me, under the circumstances, fully justified. I will now request the foreign secretary to convey it to our distinguished associate; and in so doing, I will add our hope that, in the painful and distressing visitation with which it has pleased Providence recently to try him, he may find occasion to withdraw his mind awhile from that melancholy contemplation to receive with satisfaction such a tribute to this his last and perhaps his greatest achievement, accompanied as it is by the truest regard for his private worth and the most respectful sympathy for his present distress.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: the Hon. John Wrottesley, M.A.— *Vice-Presidents:* George Biddell Airy, Esq. M.A. F.R.S. *Astronomer Royal;* Sir John F. W. Herschel, Bart. K.H. M.A. F.R.S.; John Lee, Esq.

LL.D. F.R.S.; Rev. Richard Sheepshanks, M.A. F.R.S.—*Treasurer:* George Bishop, Esq.—*Secretaries:* Rev. Robert Main, M.A.; Lieut. Henry Raper, R.N.—*Foreign Secretary:* Richard W. Rothman, Esq. M.A.—*Council:* Francis Baily, Esq. F.R.S.; Rev. W. Rutter Dawes; Augustus De Morgan, Esq.; George Dollond, Esq. F.R.S.; Bryan Donkin, Esq. F.R.S.; Rev. George Fisher, M.A. F.R.S.; Thomas Galloway, Esq. M.A. F.R.S.; Edward Riddle, Esq.; Captain W. H. Shirreff, R.N.; Lieut. William S. Stratford, R.N. F.R.S.

The following Gentlemen were elected Fellows of the Society:

The Rev. John Berrington, LL.D. F.L.S.; John Smeaton, Esq. Civil Engineer, at the London Docks; William Galbraith, Esq. M.A., of Edinburgh; and Edward Kater, Esq. F.R.S.

London:—Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. . *March* 12, 1841. No. 13.

George Bishop, Esq., Treasurer, in the Chair.

The following communications were read :—

I. On a Reformation of the Constellations, and a Revision of the Nomenclature of the Stars. By Dr. Olbers. Translated and communicated by Sir J. F. W. Herschel, Bart.

From the earliest periods of history, it has been found that as soon as mankind turned their attention to astronomy, it became requisite to group the stars into different constellations, whose outlines might seem to designate various figures. Hence arose those several constellations that have been handed down to us from the Indians, Chinese, Egyptians, Persians, Arabians, Peruvians, and others; as well as those which the Greeks adopted, and which have survived to our own times. However curious and interesting these constellations may seem, yet few of them will be found to express with any accuracy the figures which they are intended to represent. It is probable that out of the strange mixture of men, animals, and other objects, which the first astronomers invented, the imaginative Greeks made the present combination or selection. That the Greeks derived these constellations from an Asiatic people, and that they did not (as Newton supposed) invent them shortly after the Argonautic expedition, is evident from their not being able, for some time at least, to explain the constellations according to their mythology. I need only mention the Greek constellation Εν γονασι, or kneeling figure, which we now call Hercules, and the Ορνις, or bird, which we now designate as the Swan; neither of which are explained in their mythology. Combined with this mythology the constellations were sung by the Greek and Roman poets, and are now become classical. In the present advanced state of astronomy, however, we do not arrange the stars wholly according to the constellations, but according to their right ascensions: yet these constellations are a valuable assistance towards an artificial memory, and afford us an excellent method whereby we

are enabled to know and distinguish the various stars in the heavens, and to remember and record their places. The ancient Greeks reckoned only forty-six constellations, or, at most, forty-seven, if we include the Χηλαι, or claws, of the Scorpion as a separate constellation, which had been denoted as Libra before the existence of the Alexandrian school; to which Hipparchus added the forty-eighth, namely, Equuleus. The flattery of some courtiers was exerted to create two other constellations, viz. the Hair of Berenice and Antinous, but without success; till Tycho at last gave them a permanent place in the heavens.

In the fifteenth century, when navigation was extended beyond the equator, and sailors noted those stars in the southern hemisphere which were not visible to the ancients, they also found it convenient and useful to adopt the same plan of grouping the new stars into constellations. They did not, however, adapt them to the Greek mythology, but selected principally such objects as presented themselves in the newly discovered countries: whence we have, for the southern constellations, the Phœnix, the Toucan, the Little Water-snake, the Sword-fish, the Flying-fish, the Fly, the Chameleon, the Bird of Paradise, the Peacock, the Indian, and the Crane. The ancients took only those parts of the heavens, as the ground-work of the constellations, where the bright stars existed: consequently, there were many places where there were no constellations, and the stars which were scattered over such situations were called αμοςφωτοι, or *informes*. There was no inconvenience in this: but some of these empty spaces were very great, and exhibited here and there stars that seemed to be as much entitled to be formed into a constellation as several of the existing ones. Therefore modern astronomers, as Bartschius relates, (and, perhaps, he himself, partly) invented and formed the new constellations called the Camelopard, the Unicorn, the Fly, and the rivers Jordan, Euphrates, and Tigris. The heavens would now appear to be sufficiently covered, and it seemed that all the advantages which the constellations could give to the memory and imagination, in learning astronomy, had been obtained. But when Hevelius, in the latter part of the seventeenth century, had finished, with incredible labour and care, his valuable catalogue of stars, he considered that those persons who were not observers themselves had no right to institute new constellations: and although, with much reluctance, he retained the Camelopard, the Unicorn, and the Fly of Bartschius, yet he rejected the rivers; and instead of them, and in some other vacant spots, introduced the Hounds, the mountain Menalus, Cerberus, the Fox and Goose, the Lizard, the Shield of Sobieski, the Lynx, the Little Lion, the Little Triangle, and the Sextant, and also gave to Antinous a bow and arrow. Unnecessary as this increase of the constellations may be, the indefatigable Hevelius may be allowed to retain it, as the best means of preserving a remembrance of his great work. For, his enumeration and classification of the fixed stars, for which he had sacrificed the greatest part of his life, his strength, and his fortune, and by which

he hoped to have gained immortal fame as an astronomer, was soon after doomed to yield to the better and more complete British catalogue of Flamsteed, and has now become nearly useless. Astronomers now no longer use it: nor, indeed, can they use it, except in a few cases, and in some researches of little importance. Hevelius's constellations may be said to be analogous to the ancient ones; some of them may be considered as mythological: and as to the rest, they, for the most part, represent animals. Therefore it may be stated, if not in recommendation, at least in defence of them, that if they overwhelm our maps of stars they do not disfigure them. We have got on our maps only two of the constellations that were introduced in the seventeenth century, namely, Charles's Oak and the Brandenburgh Sceptre; for the Heart of Charles is merely the name of a star, and no constellation. Halley had formed Charles's Oak out of the stars that belonged to Argo; and, notwithstanding the protest of Lacaille against this usurpation, this constellation still remains. Kirch was desirous of introducing the Swords, the Orb, and the Sceptre of Brandenburgh. The electoral Swords were covered by the mountain Menalus; and the Orb yields its place to the Bow and Arrow which Antinous had received from Hevelius; and although the new globes very often disarmed Antinous, yet he has not yet taken the Orb in his hand. Moreover the Sceptre of Brandenburgh, although it did not interfere with any other constellation, yet would not have had a place on our globes, if Bode had not been the astronomer-royal of Prussia. The Cock, which was formed from a portion of the ship Argo, has likewise disappeared: the Sceptre of Louis XIV. with which Royer wished to honour his sovereign, yielded its place to the Lizard of Hevelius; the French Lily could not push away the Fly; and so on with many others: for example, the Little Crab, the South Arrow, &c. are quite forgotten, and not even known at the present day. One would now suppose that nearly eighty constellations were quite enough for all useful purposes; but the vanity of introducing new constellations had, in the eighteenth century, exceeded all bounds, and twenty-six more were added to the number. This extravagant number of new constellations, some of which were formed of scarcely visible stars, by no means made the study of astronomy more easy; but, on the contrary, confused it, and rendered it more difficult. Moreover these new constellations are so unsuited to the others, and chosen with so little taste, that no one can look on our modern globes without disgust. The first who introduced this objectionable system was the excellent and distinguished Lacaille. Surely if it were requisite that the whole heavens should be filled with constellations, they might have been chosen according to some general principle. We might have embellished the apparatus and inventions of our chemists, if indeed they could be embellished by them: and as the ancient figures of heroes and animals must be retained, some latitude might be allowed also to astronomical instruments. But figures like the shop of the sculptor, the

chemical furnace, the easel, the microscope, the air-pump, &c. have no relation to the sky, and their being mixed up with the others is heterogeneous, disagreeable, and without any taste. The same remark will apply to the Printing-press and Electrical-machine of Bode; also to Lalande's Air-balloon, although this latter constellation may seem to have some connexion with the heavens. The Hermit-bird (*Solitarius*) of Le Monnier might remain, if it did not interfere with Libra; but his Reindeer is quite absurd, on account of its smallness, it being scarcely so large as the Lizard, and much smaller than the Hare. Also Lalande's Messier, which is covered by the horns of the Reindeer, is but a very small figure in comparison with the immense human figures of the ancients that surround him, although it has robbed the greater part of its little possession from Cepheus and Cassiopeia, and has contracted the throne of the latter into a bent form; the little man is in fact quite ridiculous amongst such enormous figures. The immortal name of Frederick the Great needed not the aid of a constellation for its preservation; a constellation, in fact, carved out of Andromeda, and styled *Honores Frederici*. And if it be an honour to this great monarch to have his name enrolled amongst the stars, we must bear in mind that he enjoys this apotheosis, not only with the brave Sobieski, but also with Poniatowski and the insignificant Charles II. of Great Britain. The name of George III. will also be handed down to posterity without the George's Harp of Pater Hell; and the discovery of Uranus will preserve the name of Herschel as long as astronomy exists, without the necessity of placing his telescope in a narrow slip in the heavens. By the Lion and the Lynx the feline tribe had been sufficiently represented in the sky, without any necessity of introducing a Cat amongst the stars, merely because Lalande was fond of this domestic animal. I appeal to the judgment of all those who have compared any of the old celestial maps with the more modern ones, whether they do not feel a repugnance to the absurd mixing of so many heterogeneous constellations. And since by such an immoderate number of them the knowledge of the stars is rendered more difficult, and the taste vitiated, I would entreat astronomers to assist in freeing the heavens from such an useless accumulation, and to remove all the constellations that have been introduced since the time of Hevelius and Flamsteed. If it should be found desirable to take away some of Hevelius's constellations, and even to retain some of those which have been introduced in the eighteenth century, there should be no partiality shewn, so as to endanger the wished-for uniformity in our maps; therefore, it might appear quite unnecessary for me to fix a precise point where the line was to be drawn. It would be advisable also that the constellations should be delineated in such an uniform manner in all maps that there should be the same stars in the same parts of the figures. It is true that we do not, after the manner of the ancients, and of Hevelius, denote different stars merely by their place, but more distinctly by letters or numbers; yet it is very

useful if we could at once denote the place which a new phenomenon (for example, a comet) has taken, and also the direction of its motion, by the portions of those constellations in which it was observed. We might, in this regard, take the figures in Flamsteed's great atlas as our types; and with the more propriety, since Flamsteed has constructed them according to the ancient figures and the descriptions given by Ptolemy; with this exception, that some of his figures are ugly and badly drawn. This is a point, however, that might easily be remedied, by following the beautiful and pleasing forms of Senex, Vaugondy, Pater Chrysologue, and others. But when once the proposed forms have been adopted there should be no further uncertainty or deviation.

II. Continuation of the Investigation for the Correction of the Elements of the Orbit of Venus. By Mr. Glaisher.

In this paper the author has combined the equations formed for the correction of the elements from the Greenwich observations for the year 1839, with those given in his preceding paper, a notice of which is contained in the eighth Number of Vol. V. of the *Monthly Notices*.

Venus was near her inferior conjunction in the autumn of the year 1839, and the observed errors were consequently very well suited for the correction of the elements. The results deduced now depend on eighty-two equations, which, the author remarks, are formed from the combination of as great a number of observations as have ever been applied in determining the elements of any planet.

The formation of the equations is arrived at in precisely the same manner as in the preceding paper, and the resulting corrections to the elements are as follows:—

Correction of the	Semi Axis Major....	= −	0·00000776
——	Eccentricity........	= +	0·00002303
——	Epoch of Mean Long.	= +	2″·13
——	Aphelion............	= +	225
——	Inclination..........	= +	3 ·23
——	Long. of the Node....	= −	21 ·40

And Lindenau's elements corrected, for the epoch Jan. 1, 1836, are

			s	°	′	″
Epoch of Mean Long.	=	11	2	1	35	·23
—— Aphelion...	=	10	9	15	3	
—— Node......	=	2	15	12	3	·60
—— Eccentricity	=	0 00684568				
—— Inclination	=	3°23′34″·33				

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *April* 7, 1841. No. 14.

The President, The Right Honourable Lord Wrottesley, in the Chair.

Bartholomew Bidder, Esq., Actuary of the Royal Exchange Assurance Company, and Thomas Granville Taylor, Esq., Astronomer to the Honourable East India Company, were severally balloted for, and duly elected Fellows of the Society.

The following communications were read :—

I. Occultations of Stars by the Moon, observed at Ashurst and Dulwich, in the year 1840. By Robert Snow, Esq.

II. Observations on the Variability of the Star *α Cassiopeiæ* during the years 1839 and 1840. By Robert Snow, Esq.

III. Description of the Observatory erected at Starfield, by W. Lassell, Esq.

The author states that this observatory was erected in the summer of the year 1840, at Starfield, near Liverpool, in latitude 53° 25′ 7″ north, and longitude $0^h\ 11^m\ 41^s$ west of Greenwich. The account of the building and instruments is accompanied by drawings and a model, giving a very distinct idea of the ground-plan and elevation of the building, and of the relative position of the instruments. The building consists of one circular apartment, of 14 feet 6 inches diameter, surmounted by a dome, and is based upon a structure of undisturbed clay. The two instruments contained within it are a transit instrument and an equatoreal instrument, both supported on parts of the same pier, which is 11 feet in length, the part supporting the transit instrument being 3 feet 6 inches broad, and that supporting the equatoreal, 2 feet 8 inches broad; and the whole of the pier is kept quite separate from the walls, to avoid any communication of tremour. The dome revolves on eight rollers of 6 inches in diameter, placed at equal distances, and the friction is so small, that the strength of a boy of twelve years of age is quite adequate to turn it. The transit instrument is placed a little to the west of

the meridian line, passing through the centre of the dome, and by revolution of the dome commands the whole 180° of sky.

The telescope of the equatoreal is a Newtonian reflector of 9 inches clear aperture, and of 112 inches focal length. The object-mirror the author believes to be of very great perfection of figure, shewing the stars in the most favourable states of the atmosphere with perfect roundness, and answering very well to the severe test recommended by Mudge in the *Phil. Trans.*, of alternately exposing different portions of its surface to the rays from the object. The penumbra of a star seen out of focus is truly round, and equal in brightness in its various parts, presenting also the same form on both sides of the place of distinct vision.

The mirror is mounted in a copper tube, 9 feet long and of 9½ inches in diameter, and there is a peculiar contrivance, by means of a lever and a weight, which are brought into action for great altitudes, but which have no effect when the telescope is horizontal, for preventing its flexure, and for securing its steady adjustment in all positions. The effect of this the author describes as rendering the images of stars equally round at all altitudes, and certainly never better than when near the zenith. The declination and hour-circles are each divided to 15′, and by means of verniers, can be read to 30″ and 2ˢ respectively. The polar axis is made in three parts. The lowest part is a hollow cone of metal half an inch thick, the lower end being solid, and turned into a ball 2 inches in diameter. The ball is received into a nearly hemispherical brass step, provided with adjusting-screws. The upper part of the cone is turned and driven into the bored, internal surface of a cap plate or flange, which affords an attachment almost as firm as if the whole had been cast in one piece. The exterior circumference of the collar of the flange is also turned with great care, and rests on two cast-iron friction-wheels, 7 inches in diameter, with axles of steel. The only bearings of the motion in right ascension being the step at the foot of the axis, and two points in the circumferences of the friction-wheels, the motion is rendered very smooth, and the required impulse for motion as small as can be desired.

For observation of objects at great altitudes, a very simple contrivance is made to elevate the observer conveniently to the required position at the eye-end of the telescope. Fastened to the following side of the opening of the dome is a stiff bar of iron, notched in the manner of a saw, upon which is hung, by iron hooks, a wooden frame, in which a moveable foot-board is adjustable to different heights, in the manner of the shelves of a bookcase, by which combination of elevations every variety of altitude can be obtained at the same time that the sides of the frame afford an agreeable support to the observer. The operation of advancing the opening of the dome, also carries on the observing-ladder, and the position of the notched bar is such that the sliding of the ladder upon it, for the most part gives the necessary variation in altitude, and is very appropriately fitted to the change of position of the eye-tube of the telescope. The object-end of the telescope and the declination-

circle are counterpoised by weights applied to the framework, not to the telescope itself, by which it remains in equilibrio in all positions. With a view of conveying as correct an impression as possible of the general arrangement of the parts of the observatory and the instruments, the author has accompanied his description by a model, on a scale of one-tenth the full size in linear dimensions.

IV. Observations on Bremicker's Comet, with its apparent places, as obtained, with the Equatoreal, at Mr. Bishop's Observatory, and an account of the methods employed in deducing them. By the Rev. W. R. Dawes.

The extreme faintness of this comet rendering it observable only by persons possessing large telescopes, Mr. Dawes was induced to employ for the purpose Mr. Bishop's equatoreally-mounted telescope of 7 inches clear aperture, and $10\frac{3}{4}$ feet focal length. The micrometer used has four thick wires, two of which are parallel to the screw, and, therefore, parallel to the webs, and fixed at an angular distance of 7" between their adjacent edges, and the other two parallel to the webs, and moveable with them. By means of these four thick wires a small square was formed, as nearly as possible in the centre of the field, and the nucleus of the comet placed in it for observation.

The nucleus resembled a star of about the 10th or 11th magnitude, abruptly diffusing itself into the nebulosity around it. It was very decided, with powers 63 and 105, the latter of which was generally employed in the observations. The first day of observation of the comet was Nov. 14, when, as well as on Nov. 16, 19, and 24, the stellar nucleus was remarked as distinctly visible. On Nov. 19 it appeared rather excentric on the north following side. After Dec. 3 a succession of cloudy evenings occurred, and the comet was not seen again till Dec. 22, when the stellar nucleus was again visible, and the apparent diameter of the comet was certainly larger. It must also have been brighter, since the observations of it were considered good, though the sky was so hazy that stars of the 4th magnitude were scarcely visible to the naked eye.

The last night of observation was Dec. 29, when it still exhibited a decided stellar nucleus, and the nebulosity appeared more extended and dense. No observations were made to determine its diameter, but it was estimated at $1\frac{1}{4}'$. At no time has any appearance of a tail been suspected.

The method of observation adopted by Mr. Dawes was that of transit-comparisons in right ascension, and of micrometrical comparisons in north polar distance with neighbouring stars whenever it was practicable. Most of the comparison-stars have been identified as contained in the Catalogue of Groombridge; and the author enters greatly into detail respecting the catalogue-places of all his observed stars, preferring the places given in Pond's Catalogue of 1112 stars, whenever they were found there.

The following table exhibits the observed apparent places of the Comet:—

Date.	Greenwich Mean Time.	Right Ascension.	N. P. D.	Remarks.
1840.	h m s	h m s	° ′ ″	
Nov. 14	9 4 19	... ...	30 39 6	Compared with π Draconis. Rather doubtful.
...	9 29 9	20 31 13	... ...	Ditto ditto ditto.
16	9 55 53	20 46 37·8	31 18 45	Compared with χ and κ Cephei.
19	9 22 5	21 9 30·6	32 16 7	Mean of 3 sets of comparison with χ Cephei.
20	7 8 48	21 16 33·2	32 38 15	Compared with ζ Cephei.
21	9 9 39	21 24 55·7	33 6 59	Compared with ζ and ι Cephei
...	... ...	21 24 58·5	33 7 4	Compared with Groombridge 3462 and 3636.
...	10 48 18	21 25 30·5	33 8 56	Ditto ditto ditto.
24	8 51 1	21 48 6·2	34 38 5	Compared with Groombridge 3806.
26	7 11 14	22 2 49·4	35 45 34	Mean of 2 sets of comparisons with ζ and ι Cephei.
Dec. 2	7 31 58	22 46 40·2	40 0 12	Mean of 4 comparisons with Groombridge 3917.
3	6 38 59	22 53 14·1	40 46 18	Compared with 3 Andromedæ, or Groombridge, 3972.
...	7 36 31	22 53 32·8	40 48 30	Comet precisely in the same parallel with 3 Andromedæ.
22	7 55 52	0 37 39·5	57 59 42	Mean of 2 comparisons with π Andromedæ.
29	11 38 59	1 5 41·2	63 57 45	Mean of 2 comparisons with ν Piscium.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *May* 14, 1841. No. 15.

The President, Lord Wrottesley, in the Chair.

Solomon Moses Drach, Esq. of Castle Street, Bevis Marks; the Rev. J. Wright, M.A. Incumbent of Lockwood; and James Glaisher, Esq. of the Royal Observatory, Greenwich, were severally balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. Description of a Dioptric Telescope, and of a Micrometrical Lunette. By M. Chevalier. Translated by R. W. Rothman, Esq.

The dioptric telescope constructed and described by M. Chevalier differs from other telescopes chiefly in the arrangement of the object-glass. This consists of a large achromatic combination of long focus, and of a small one which reunites the rays transmitted by the primary one. The chief advantage derived from this construction is the great diminution of spherical aberration, and, consequently, the power of increasing the aperture of the telescope with the same focal distance. This advantage is obtained by dividing the excess of curvature between the two achromatic combinations, and by calculating the distance between them, so as best to secure the diminution of the spherical aberration and perfection of the achromatism. The author considers that a great advantage is gained by the process which he adopts of *luting* together the crown and flint-glasses forming the achromatic combinations, by means of balsam of Canada almost cold, this process being not subject to the inconvenience attached to the use of mastic in drops recommended by other artists. He also suppresses the use of diaphragms in the tubes of his telescopes, which he lines with black velvet.

The author's micrometrical lunette consists of a telescope bent at right angles, in the manner of a Newtonian reflector, carrying a sight on the object-end and a small perforated mirror on the eye-piece, which combination permits the observer to see at once, and

with the same eye, an object at a fixed distance, and the image of the object produced by the telescope. All the usual micrometrical and illuminating methods are applicable to the sight, on which may be traced divisions as small as we choose, being read by a small telescope parallel to the first, and placed before the metallic mirror. By the author's arrangement, the errors to which common micrometers are subject, depending on the thickness of the wires, the imperfection of the screws, &c. are got rid of. He considers his micrometer as particularly applicable to geodetic operations, and suitable for the purposes of the surveyor as well as of the astronomer.

II. Observations of the Aurora Borealis. By Robert Snow, Esq.

In this paper Mr. Snow records his observations of this interesting phenomenon made at Ashurst and Dulwich, from the autumn of the year 1834 to the autumn of 1839, within which period several remarkable auroræ appeared; among which, as particularly deserving notice, he describes the various appearances of those of November 17, 1835, and of February 18, 1837; the latter being rendered additionally remarkable by its happening on the same evening as the occultation of *Mars* by the moon. Other fine auroræ described by the author appeared on November 12, 1837; on September 13, 1838; and on January 19 and September 3, 1839.

The author deduces from his observations the following invariable circumstances of the phenomenon: That the aurora may be expected at any season of the year; that it assumes nearly every variety of colour; that it resembles both in shape and motion every variety of ordinary cloud; that its appearances are, in the course of the same evening and without any determinate order, undulating, radiating, and streaming, with other capricious forms not easily expressible; that the length of time during which it is visible is very uncertain; that it appears to the eye (geometrical considerations apart) as if it existed at various distances from the earth's surface; that, although for the most part it is not influenced by the presence of clouds, it occasionally tinges them with its own prevailing colours; that this has been noticed only when the clouds are low; that there are also certain *lofty cirrous* clouds, which have the appearance of arranging themselves in peculiar bands or strata, as if in connexion with the aurora; that these strata are visible during daylight, when the visibility of the *dark portion of the arch* has sometimes been strongly suspected; that the stars are seen both well and ill defined through the auroral light and the auroral darkness; that it is by no means confined to the northern regions of the sky, though originating about the magnetic north; that, with the exception of a diminution of its general effect, it is uninfluenced by moonlight; that its appearance generally accompanies weather the reverse of frost, such

as heavy wind and rain; and, lastly, that it is wholly inaudible. The author concludes by warning the spectators of this phenomenon against the false impressions to which the senses are liable, especially with regard to the sensation of heat and the notion of sound, as attending phenomena in which our idea of either of these qualities has been predominantly awakened.

III. Mean Positions of the Stars mentioned in Mr. Baily's Address to Observers, determined at San Fernando in the years 1834, 1835, 1836, 1837, and 1838. By M. Montojo. Translated by Captain Shirreff, R.N.

The whole number of stars contained in this catalogue is 126, of which 121 are mentioned in Mr. Baily's "Address to Observers," as being of doubtful position, or not recently determined. The reductions of the observations of right ascension have been performed strictly by means of the *Tabulæ Regiomontanæ*, the mean positions of the fundamental stars and the numbers and constants of that work having been adopted. The equinox to which they are referred is that of Bessel, coinciding with that determined at San Fernando from 206 observations of the sun.

The transit-instrument and clock employed are described in the published *Observations* of the years 1834 and 1835; and the instrumental errors there explained have been rigorously taken into account.

The observations in declination include only the years 1837 and 1838, the mural circle having come into operation in 1837.

This instrument differs in no essential respect from those of the Observatory of Greenwich, and was made by Mr. T. Jones.

The mode of observation is essentially similar to that pursued with other mural circles, the horizontal points being obtained by direct and reflexion observations, at the same transit, of different stars ranging through a large arc of zenith distance, and the mean of the results of three days' observations being usually employed. Each observer is made to determine his own horizontal point, some slight disagreement between the results of different observers, similar to what in transit observing is called *personal equation*, having been recognised.

The latitude was determined by superior and inferior transits of *Polaris*, of β, δ, and λ *Ursæ Minoris*, and of *Cephei* 51 (Hev.), using only such observations of them as were made by direct vision and by reflexion at the same transit.

The refraction is computed from the *Tabulæ Regiomontanæ*, and the precession from the values of m and n given in that work.

The resulting right ascensions are compared with the Astronomical Society's Catalogue, with Pond's Catalogue of 1112 Stars, with Wrottesley's Catalogue, and with Johnson's of 606 Southern

Stars, and also with the stars found in the Greenwich observations of 1833, 1836, and 1837.

The N. P. D.'s are compared with the Astronomical Society's Catalogue, with Pond's, Johnson's, and Henderson's Catalogues, and with the Greenwich observations of 1836 and 1837.

Errata in last Notice.

Page 107, line 17, *for* 1840, *read* 1839.
Page 109, line 16, *for* parallel, *read* at right angles.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *June* 11, 1841. No. 16.

The President, Lord Wrottesley, in the Chair.

Samuel E. Cottam, Esq., John B. Duncan, Esq., M.A., and the Rev. Charles Strong, M.A., were severally balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. A New Catalogue of Moon-culminating Stars, observed at South Kilworth. By the Rev. Dr. Pearson.

This catalogue contains 520 stars which fall within the passage of the moon, in every situation of the nodes, and which had been published in the Appendix to the first volume of the author's work on *Practical Astronomy*. The stars in this catalogue had been selected by Caturegli from the determinations of different observers, and therefore might be supposed to have their places assigned with different degrees of accuracy, so as to require a new series of careful observations, and reduction with improved constants, to render them suitable objects for so delicate a purpose as the determination of longitudes, by comparison with the moon's limb, upon or near the meridian. This consideration induced Dr. Pearson to confine his stellar observations to moon-culminating stars, employing for the purpose, contemporaneously, the transit instrument and circle, described in his work before referred to, and a clock by Hardy, which is an exact fellow to the transit-clock at Greenwich in its original state. The observations were continued uninterruptedly from November 4, 1830, to May 7, 1834; when, on account of inconvenience arising from the smoke of the village, the instruments were removed to another building in the author's private grounds, lying in the same meridian and 4″ to the south of the former. They were here continued till the end of the year 1837, when it was found that, with very few exceptions, the whole of the stars had been observed at least six times.

The reductions were made with the constants given in the Astronomical Society's Catalogue for the epoch 1830, for the pur-

pose of direct comparison with that catalogue, using the annual precessions given in that work. Brinkley's refractions were used in the reduction of the circle observations, which include stars as low as 8° of altitude, and may therefore possibly furnish data for correction of the tables of refractions; with which view the original observations have been preserved. The telescopes used are by Tulley, and the object-glasses, of 3¼ inches aperture and nearly 44 inches focal length, may be considered excellent in their definition of a large star. The collimation in altitude was determined by a small collimating telescope, by reversion of the azimuthal positions; and comparison of the sum of reversed readings with 90° proved highly satisfactory in every instance.

The resulting right ascensions of the stars used for clock-error are given in an appendix, that their close agreement with the right ascensions derived from a multiplicity of observations at Greenwich may be exhibited.

Numerical corrections were not applied for the instrumental errors, but the instruments themselves were kept in adjustment by means of hanging levels. The meridian mark on the northern wall of a house, directly to the south of the transit instrument, was never observed to deviate in position; though the pillar, carrying the northern mark, was liable to deviations when the sun was powerful or the frost severe.

II. On the Advantages to be attained by a Revision and Re-arrangement of the Constellations, with especial reference to those of the Southern Hemisphere, and on the Principles upon which such Re-arrangement ought to be constructed. By Sir J. F. W. Herschel, Bart. K.H. V.P.R.A.S., &c. &c.

The stars must have been divided into groups, which, as also single stars, would have received names long before they began to be considered with reference to the seasons, or the sun's motion in the ecliptic. The best-defined groups would be the first named, but in the stars of the zodiac this principle would be afterwards modified, for the convenience of dividing the circle into equal parts. In the extra-zodiacal regions no principle seems to have prevailed, either of subdivision or nomenclature; and many of the figures of the constellations scarcely resemble the forms intended, with the exception of a few, as *Scorpio* for example. Names, however, would be imposed from other associations than mere form. The author proposes to consider how far the present system of constellations is adapted to the purposes of astronomy, and to examine by what modifications it may be made more serviceable; and also to inquire into the circumstances which render a systematic revision of them desirable, or even necessary, at the present advanced epoch.

The use of constellations to the astronomer is to enable him to refer to a particular star. For this purpose, a distribution on any principle would serve; yet even on this point the present system often leads to confusion: for, 1st. the similarity of the names of

several constellations, and the bestowing on a new constellation the name of an old one, with an adjunct for the sake of distinction, render it necessary in many cases, in order to avoid confusion, to write the names at full length: thus, for example, we have (with several others) *Ursa major* and *minor*, three *Triangles*, *Pisces* and *Piscis*, *Apis* and *Apus*, *Telescopium* repeated three times, *Quadrant*, *Sextant*, and *Octant*, &c. &c. 2dly. Some constellations are so extensive that they exhaust three or more alphabets, and therefore it is sometimes necessary, beside the letter of the star, to specify also its right ascension and declination: for example, in *Argo* three stars are marked *a*, and seven A. Again, such constellations extend over so many hours of right ascension, that the name of the constellation is of no use in finding, in the catalogues, one of the stars composing it: thus, *Argo* occupies 8 hours of right ascension. 3dly. The imperfect and uncertain boundaries of the present constellations lead to confounding the Greek letters of one constellation with those of another contiguous to it. Moreover, the boundaries are not always the same in different maps—a circumstance which alone is decisive of the necessity of some system which might be more favourable to a general understanding.

To the astronomer who refers to stars by their catalogued places, these inconveniences may not afford serious ground of complaint; since even in the southern hemisphere but few stars of the fifth magnitude remain uncatalogued. Nor are the defects of the present system felt by seamen, who have little to do with the constellations beyond referring to a few well-known stars. But to those who employ themselves in the physical departments of practical astronomy, such as variable stars, photometrical determinations, and other subjects, and who require a perfect familiarity with the aspect of the heavens, the present arbitrary and capricious allotment of the stars renders reference to maps constantly necessary. And when the leading stars in the map are not those which in the heavens catch the eye by their brightness, it becomes necessary alternately to inspect the map by candlelight, and then to rush out into the darkness, to compare the impression made on the memory with the visible aspect of the stars, to the loss of all delicacy of vision, and the injury of the organ itself.

The author had proposed to himself to follow out, in the southern hemisphere, the plan adopted by the late Sir William Herschel, in order to place on record the relative apparent magnitudes of the stars at this epoch; and it was while thus engaged that he became impressed with the necessity of a total reformation of the ancient system.

In enumerating the qualities which a system of sidereal arrangement should possess, the writer observes that the new subdivisions should be of moderate extent, the figures easily traced, and the groups such as naturally arrange themselves in distinct forms. Again, the boundaries ought to be definite, so as to be transferred from one map to another without variation; they should accordingly be arcs of great circles, or parallels to a great circle: that is,

circles of right ascension and parallels of declination. The limits, thus assumed, should correspond to a particular epoch, being reduced to any other time, by the necessary tables, like stars. The advantages of this system would be that each star would be at once referred to its proper district; that the observer, becoming familiar with the limits, would know the time when each star approached his meridian, as also the limits of altitude between which it would be comprised.

After some remarks on the mode of carrying the proposed plan into execution, the writer suggests:—

1. That the names of constellations should be such as not to be easily confounded either when spoken or printed.

2. Each name should be Latin, with a regular genitive case; to which the astronomers of different nations should conform, without translating them into their own languages.

3. In the names, low or homely associations, technicalities of science, and national and political allusions, should be avoided.

4. General names should be preferred, as Rex, Regina, Miles, Sculptor, Poeta, &c.; and they would be selected from mythology and classical antiquity, as neutral ground.

5. The naming of constellations after their imagined figures must be abandoned.

Individual stars are most conveniently designated by Greek letters, the letters of the alphabet being assigned in the order of the brightness of the stars. The inconvenience which would sometimes attend this last condition might be remedied.

The practice of a giving proper name to each star is so convenient, that the author would wish to see it extended to all stars of the third magnitude, at least; and he concludes with some further suggestions on this point. The paper is accompanied by a plate in illustration of the principle of arrangement.

Erratum in Monthly Notice, Vol. V. No. 12.

Page 93, line 27. *for* have, *read* has.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *November* 12, 1841. No. 17.

The President, Lord Wrottesley, in the Chair.

The following communications were read :—

I. On the Longitude of Dr. Lee's Observatory at Hartwell.

The longitude of this observatory was assumed from various authorities to be $3^{m}\ 20^{s}\cdot6$ west from the Royal Observatory at Greenwich, by the late Mr. Epps, for some time after his arrival at Hartwell. These authorities appear to have been as follows:

	m s
Captain Smyth, by means of two trips with a chronometer from Bedford Observatory	3 20·2
By the moon's culminations as computed by Mr. Riddle	19·9
Mr. Epps, by chronometers	21·7
Ditto	20·7
Mean longitude	3 20·6

The mean of these determinations was naturally supposed by Mr. Epps to be very near the truth. In October 1838 this mean result was found, however, to differ considerably from the difference of meridians as determined by twelve chronometers, taken by Mr. Dent from the Royal Observatory (which was $3^{m}\ 24^{s}\cdot46$). It was evident, therefore, that there was either an error of nearly four seconds of time in the longitude of Hartwell, as previously assumed, or in the observations made there on this occasion to determine the error of the clock with which the chronometers were compared. A careful recomputation of the observations, as recorded in the Hartwell transit books, was therefore made, and the result (as far as the reductions were concerned) was found to be correct.

A reference was then had to Aylesbury church spire, the position of which had been determined by the Trigonometrical Survey. This was done by means of an estimated distance of the spire from the Hartwell Observatory, taken from a county survey, and the observed azimuth of the former from the observatory.

This gave a result ($3^m\ 23^s{\cdot}07$) differing $2^s{\cdot}5$ of time from Mr. Epps's former determination, and $1^s{\cdot}5$ from that obtained from Messrs. Arnold and Dent's chronometers, and was, therefore, far from being satisfactory.

In the following January another series of results was obtained by means of ten chronometers, which were taken by Mr. Dent as before, from the Royal Observatory to Hartwell, on the 6th of that month, and the comparisons made with the transit clock at the latter place on the same day. The chronometers were brought back to the Royal Observatory on the 9th following. The difference of meridians by these observations was $3^m\ 24^s{\cdot}06$.

Other results were also obtained by means of chronometers taken from the Royal Hospital Schools at Greenwich to Hartwell Observatory; and, in reference to these results, as well as to those before obtained, Mr. Epps observes, in a letter to Mr. Fisher, "The results agreeing so well with the former, I think we may conclude that $3^m\ 24^s{\cdot}2$ (as you have already noticed) is extremely near the truth. This may be called the mean result of thirty chronometrical determinations. I may remark to you, that my observations for time are made with as much attention as possible to the state of the transit instrument; viz. that it works with no apparent error in collimation, nor level error, but correcting as occasion may require for azimuthal deviation. With the exception therefore, of minute differences in the right ascensions of the stars by which the clock-errors were determined, and some trifling optical defects, I conclude that nothing of importance can be urged against the mean of all the results. Indeed, all the observations respecting the chronometrical comparisons are plain and straightforward matters of fact in conjunction with the transit observations, as recorded in the observation books."

The error in the former assumed longitude being now fully confirmed by so many chronometrical results, it was resolved to connect in a more accurate manner than before the position of Aylesbury spire with that of the observatory at Hartwell by actual measurement and triangulation; since it was possible that an error might have occurred so as to have caused the discrepancy observed between the chronometrical longitude and that obtained by the trigonometrical survey. This was done in April 1840, and the result was nearly identical with that previously deduced by means of the county survey.

As there is a considerable error in the longitude of this spire as given in the third edition of the *Requisite Tables*, Mr. Yolland, of the Ordnance Map Office, very kindly undertook the recomputation of its geographical position from the original data of the trigonometrical survey, and found it to be as follows:—

Latitude	$51°\ 49'\ 1''{\cdot}0$	North.
Longitude..................	$0\ 48\ 50{\cdot}15$	West.
In time.....................	$3^m\ 15^s{\cdot}34$	

From this corrected position of the spire, we have the following for the position of the observatory at Hartwell :—

Latitude	51° 48′ 14″·8 North.
Longitude..................	$3^m\ 22^s{\cdot}57$

Final results for difference of meridians :—

	m s
By the chronometrical determinations...........................	3 24·26
By Aylesbury Spire, as determined by the Trigonometrical Survey	3 22·57
Difference	1·69

II. Observations of the Beginning and Termination of the Solar Eclipse of July 18, 1841, at Aberdeen. By Charles Crombie, Esq.; communicated by George Innes, Esq.

The eclipse was observed in the garden attached to Mr. Crombie's residence, which is a short distance from the Marischal College. The instrument used was a 2½ feet achromatic telescope, with a power of about thirty-six; and the times were taken with a pocket chronometer, whose rate was determined by two comparisons with a clock belonging to Mr. Innes, and the error by several altitudes of the sun.

The Aberdeen mean solar times of the beginning and ending of the Eclipse, resulting from the observations, are :—

	h m s
For the beginning.....................	2 17 48·7
And for the ending	2 58 10·2

III. Observation of the Lunar Occultation of *Venus* on September 11, 1841, at Mr. Bishop's Observatory, in the Regent's Park.

The occultation of *Venus* by the moon was observed here, but not under favourable circumstances. The morning was clear, but the wind easterly. The equatoreal telescope was charged with a power of 105. *Venus* was badly defined in general, the air being in a very disturbed state. The enlightened edge of the moon completely hid the planet at about $18^h\ 31^m\ 21^s$, Greenwich mean astronomical time. The time was not accurately noted, the observer's attention being principally directed to the phenomena of the occultation. No projection on the moon's limb, nor any distortion of the form of *Venus*, was perceivable. The edge of the moon was well seen, and sharply defined on the planet's disk.

The commencement of the reappearance at the unenlightened edge was not well caught, the planet becoming visible at some distance from the centre of the field. This being instantly rectified, the dark edge was well seen on the planet, which did not appear in the least distorted. The reappearance was complete at about $19^h\ 41^m\ 54^s$, Greenwich mean time, and was observed with the power 105. The air had become very smoky, and vision was extremely bad.

IV. Notice of the Occultation of *Venus* on the Morning of the

12th of September, 1841. Observed at Malta by Capt. Basil Hall, R.N. Communicated by Capt. Beaufort, R.N.

"The beginning of this interesting occultation was observed at Valetta within a second of time, I think I may venture to say. An unlucky cloud prevented my observing the planet's reappearance. Telescope magnifying sixty times.

"The following are the times by chronometer:—

	h	m	s
First contact of the north limb of *Venus* with the south limb of the moon (civil reckoning)	6	45	54
Instant when the centre of *Venus* appeared cut by the enlightened limb of the moon, as nearly as I could judge...	6	46	26
Contact of the eastern, or enlightened, limb of *Venus* with the eastern, or enlightened, limb of the moon	6	46	36·0
Chronometer slow of Malta mean time	1	6	33·2
Mean time at Malta of the disappearance of the eastern limb of *Venus* behind the east limb of the moon	7	53	9·2
Difference of longitude		58	1·8
Mean time at Greenwich of the disappearance of the eastern limb of *Venus* behind the moon	6	55	7·4

"The time was ascertained by equal altitudes of the sun, and, I think, may be considered correct to about a second. The difference of longitude is taken from the Table No. 8, in Lieut. Raper's recently published work, in which you will observe that the observatory (which is no longer an observatory) on the palace, is placed in .... 14° 30′ 42″ = $58^m\ 2^s\cdot8$. But my house lies west of the palace .... 1·0.

Consequently the difference of longitude is ...... 58 1·8.

"The latitude of my house is the same as that of the observatory, viz. 35° 53′ 54″, as given by Lieut. Raper; but I have not yet had an opportunity of verifying this point.

"On the voyage to Malta from England, and since my arrival here, I have had ample means of examining the work above alluded to; and I feel it right to say,—and I hope you will communicate my testimony (such as it is worth) to the Astronomical Society, in favour of the book of my highly valued friend, their secretary,—I have gone over almost every part of the *Practice of Navigation*, and some of the parts a great many times, and I can say without qualification, that I am acquainted with no work so well adapted for the use of sailors, none so luminous and precise in its style, nor so simple in its use. The tables, too, are well arranged and of very ready application, in consequence not only of the distinctness of the precepts, but the good selection of illustrative examples. It is much to be desired that Lieut. Raper should publish his second volume, for such works contribute greatly to the improvement of practical navigation, not merely by the information they furnish, but by raising the standard of accuracy, and teaching that, even by moderate, but *well-directed*, exertions, any ship may be navigated with far more certainty and speed than by the ordinary and loose methods still, unfortunately, too much in use afloat."

V. Observations of Bremicker's Comet made with the Equatoreal Instrument of the Observatory of Padua. By M. Santini.

As soon as the notice of this discovery was received, the comet was immediately sought for at the Observatory of Padua; but clouds and the light of the moon prevented it from being seen till the evening of the 22d of November: it was extremely faint, and presented itself under the appearance of a light mass of vapour faintly illuminated, without sensible trace of a nucleus. It was observed till the evening of the 27th of November; after which time other occupations hindered M. Santini from making further observations of it till the 1st of December. After this time the clouds and the light of the moon caused him to give up the hope of seeing it again.

Day, 1840.	Mean Time at Padua.	Apparent R.A. of the Comet.	Apparent Declin. of the Comet.	Comparison-Stars from Piazzi's Catalogue.
	h m s	h m s	° ′ ″	
Nov. 23	9 3 6·5	21 40 12·78	+55 54 37·1	Piazzi xxi. 385
24	7 26 8·1	21 47 24·58	55 25 1·8	Ditto xxi. 373 & 385
	7 51 5·4	21 47 31·92	55 24 31·8	
25	7 1 33·4	21 54 57·55	54 51 45·1	Ditto xxi. 54 & 92
	7 32 4·9	21 55 4·13	54 50 47·1	
	8 5 56·2	21 55 6·19	54 49 4·1	
26	7 13 13·7	22 2 35·76	54 15 23·4	Ditto
	7 36 40·5	22 2 41·47	54 15 9·5	
	7 58 32·0	22 2 43·06	54 14 6·5	
27	7 33 31·0	22 10 12·29	53 36 56·0	Ditto xxii. 92 & 137
	7 56 38·6	22 10 16·95	53 35 40·8	
	8 8 34·7	22 10 26·32	+53 36 23·8	

M. Santini has computed elements of the parabolic orbit of the comet, based on the observation made at Berlin on Oct. 28, communicated to astronomers by M. Schumacher; on that made at Vienna on Nov. 12; and on the mean of the above positions of Nov. 24.

The following are the elements derived:—

Perihelion Passage, Nov. 15·25525,* Berlin Mean Time.

Long. of the perihelion	23° 42′·5	from the true equinox.
Long. of the node	248 47·7	———
Inclination	58 5·05	———

Motion Direct.

Log. Perihelion dist. = 0·16984
Perihelion dist. = 1·4786

* In the manuscript the time of the perihelion passage is also written 320^d·24525.

VI. Introduction to a Catalogue of 1677 Stars included between the Equator and 10° of North Declination, observed at the Royal Observatory of Padua. By M. Santini. Communicated by Sir J. F. W. Herschel, Bart.

The observations of the stars in this catalogue were made with a meridian circle constructed by Starke, a description of which is to be found in the fifth volume of the *Transactions of the Academy of Padua*. The object of M. Santini has been so to arrange his new catalogue that, at every eight or ten minutes of right ascension, there should be found in each parallel of declination a well-determined star, with the view of facilitating the comparisons of planets and comets with neighbouring stars, by means of micrometrical measurements.

The brightest stars that could be found were chosen for this purpose, very few being admitted which are below the eighth magnitude. They were observed for convenience of reduction in contiguous groups, in such a manner that the corrections necessary for reducing them to the mean equinox of 1840 might be applied to the mean of the apparent positions observed, for the mean instant of the series; and the greater number of the stars were observed three times in both elements. It is the author's intention to proceed immediately with similar observations of stars in the zone extending from the equator to 20° of south declination; and he invites astronomers to participate in his labours by observing some other zones.

The observed right ascensions of Bessel's fundamental stars were compared with their right ascensions given in the *Berlin Ephemeris*, for obtaining the clock-correction; and the azimuthal deviation of the instrument was obtained by the superior and inferior transits of *Polaris*.

The polar point of the circle was obtained by observed zenith distances of *Polaris* and the same fundamental stars, using Carlini's Refraction Tables, and the apparent declinations of the *Berlin Ephemeris*. The agreement of the individual results both for clock errors and for polar point was in general highly satisfactory. To obtain the mean places for 1840, small special tables were used similar to those employed for Bessel's zones, the values of the constants, f, g, h, i, G, H, of the *Berlin Ephemeris* being adopted; and in the annual variations no allowance has been made for proper motions of any of the stars.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *December* 10, 1841. No. 18.

G. B. Airy, Esq. Vice-President, in the Chair.

The Rev. Peter Holmes, B.A. head-master of the Plymouth Grammar School, was balloted for, and duly elected a Fellow of the Society.

The following communications were read :—

I. Observations of the Solar Eclipse of July 18, 1841. By the Rev. Professor Chevallier.

The eclipse was observed at a place five miles west of Durham, in about latitude 54° 46′ N., and longitude $6^{m}\ 25^{s}$ E. The eclipse had commenced about two minutes when the sun's disk became visible. The edge of the moon simply skirted the sun, the border of which, seen through a hazy cloud, was tremulous, and did not permit the cusps at any time to come to a fine edge.

The telescope used was an achromatic inverting telescope of 2 inches aperture, and 2 feet 6 inches focal length. Two projections were distinctly seen on the edge of the moon during the progress of the eclipse, one of which was observed to approach the right-hand side of the eclipsed part of the sun, and finally to disappear at about $2^{h}\ 50^{m}$ mean solar time.

The termination of the eclipse was observed at $2^{h}\ 58^{m}\ 17^{s}$; but the sun's edge was so ill defined, and the motion of the moon so unfavourable for observation, that it could not be determined with any great accuracy.

II. A new Method for greatly facilitating the Computation of the Moon's Co-ordinates. By S. M. Drach, Esq.

The author's object in this paper is to endeavour to diminish the labour of taking into account the numerous equations in the moon's longitude, latitude, and parallax. He proposes three methods, by each of which some of the difficulties may be lessened, but he confines himself principally to one of them, which he has developed, and whose results he has tabulated in forms adapted to

computation. He has annexed to his paper a skeleton form of computation adapted to his arrangement of the tables, and has placed it in juxtaposition with the form used by the computers of the *Nautical Almanac*.

III. Thoughts on Shooting Stars and Comets, suggested by the perusal of Mr. Galloway's paper on the subject, read before the Society on January 8, 1841. By S. M. Drach, Esq.

The greatest ascertained height of those meteors above the earth's surface appears to be 550 miles, or $\frac{1}{7{\cdot}2}$ of the earth's radius, and if the moon be supposed to be distant 60 radii of the earth from the centre of our globe, and its relative mass to be $\frac{1}{80}$, the ratio of the lunar to the terrestrial attraction at the above-named height would be as 1 : 213690; and consequently the lunar gravity may be neglected. The visible path of a meteor would be, therefore, some conic section concave to the earth's centre, the particular kind of section depending on the initial circumstances of projection. The paths of different meteors have been found, however, to be sometimes *convex*, and even *serpentine*, which may be accounted for by supposing them to be very light cometary bodies deflected by the ether; the ethereal resistance increasing very rapidly with the velocity, and depending on the anterior surface of non-spherical bodies opposed to it. The author remarks that this theory of lunar emission might be easily tested by the method of quadratures, which is found so useful in the cometary theory.

If we assume with Chladni and others that these meteoric bodies move in distinct groups or zones, we may conceive each group as having a distinct orbit round the sun, and as intersecting the ecliptic in a different point; and, allowing the probability of their having every variety of motion, and of inclination and eccentricity of orbit, the different times of appearance of these bodies, as well as their great velocity, might be accounted for. If, in addition, we assume that the groups have a rotary motion independently of their motion of translation, the great velocity attributed to them in M. Wartmann's observations will be accounted for.

A further argument in favour of their cometary origin is derived from their trains, and their apparent self-luminosity, analogous to the non-appearance of phases in comets. The author thinks it not impossible that part of their light is owing to electricity, and suggests a method of discovering the velocity of the electric fluid (assuming that the light of comets of short period is partly owing to this cause) by means of the difference of the aberrations which would result from the difference between the velocity of their intrinsic light and of that which they derive from the sun, and which would cause an apparent flattening or elongation of the semi-nucleus which is turned from that luminary.

With respect to their alleged origin in the zodiacal light, this is, perhaps, only true for those that are really electrical phenomena of the ethereal medium in contact with the solar heat and light; and it would be a curious field of inquiry, should the non-sphericity of the solar orb, by differently attracting the luminous superstrata, originate this phenomenon. It remains to be shewn how these electrical bodies are prevented from falling on the sun, or how they could move with so small a velocity, if any imponderable cause were the agent.

As regards Mr. Galloway's second adduced theory, — if so many millions of meteors have been seen, how great must the supposed large planet have been to have furnished them! Now, as from modern observations elements are deduced, giving positions which represent the most ancient observations with wonderful accuracy, the alleged explosion must have occurred upwards of two thousand years ago. How is it that there are still so many millions of fragments as yet only half way in their progress to the sun?

The fifth hypothesis is yet in too infant a state to be discussed. The author's object in this paper is chiefly to remove Mr. Galloway's objections to the fourth hypothesis, which appears to give the most feasible explanation of these remarkable and apparently anomalous phenomena.

Before the close of the meeting, Mr. Baily gave a short verbal statement of the result of his labours in the repetition of the Cavendish Experiment, explaining the cause of the difficulty which had so long baffled all his efforts to obtain consistent results, and which has been at length satisfactorily found attributable to the radiation of heat from the larger balls. This cause of disturbance, which was suggested by Professor Forbes, has been successfully got rid of by gilding the balls and the surfaces of the box containing the torsion apparatus, and by wrapping the box in flannel. The results, which have been since obtained, are stated by Mr. Baily to be as satisfactory as can be desired; and the differences, between the partial results and the mean, are clearly within the limits of probable error in experiments requiring so great delicacy, both in the adjustment of the apparatus and in the observations by which the deflections of the balls are measured. Mr. Baily is preparing his Report on this subject, which will be read at a future meeting of the Society.

A letter was read from Mr. Snow, corroborating, by the casual observations of a friend who was at Syra on the 10th, 11th, and 12th of August of the present year, the observed fact of the great number of meteors which are usually visible at that season of the year.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *January* 14, 1842. No. 19.

George Bishop, Esq. Treasurer, in the Chair.

The following communications were read:—

I. Observations of Halley's Comet, made at the Observatory of Geneva in the years 1835 and 1836. By M. Müller, under the direction of M. Gautier, Director of the Observatory. Communicated by Sir J. F. W. Herschel, Bart.

These observations were made on fifty-two nights, beginning with August 31, 1835, and ending with May 7, 1836; of which thirty-one were before the perihelion passage of the comet, and twenty-one after the passage. The instrument used is an equatoreal of Gambey, whose telescope has an object-glass of four French inches diameter, and of forty-two French inches focal length. The declination circle and the hour circle of the instrument are each thirty inches in diameter; the former being divided to every three minutes of a degree, and by means of its verniers giving arcs of 3″; and the latter being divided in time, and by means of its verniers giving the fifth part of seconds of time. The times were taken with a clock by Lepaute, which was every evening compared with the transit clock. The index corrections, obtained chiefly by observations of stars found in the A. S. C., and whose observed places were compared with the places taken from that Catalogue, and from Pond's Catalogue of 1112 stars, were very consistent throughout the whole series of observations, and shew that the firmness of the instrument, as well as its state of adjustment, were highly satisfactory. Absolute observations of both elements were obtained in every instance by reading off both circles; this method being preferred by M. Gautier to differential observations with a micrometer. A reticular micrometer, made of fine plates of metal, was used, the faintness of the comet scarcely ever admitting of any illumination of the field.

In the reduction of the observations, the mean refractions were computed for all the observations of the comet and the comparison-stars; and the instrumental right ascensions and north polar dis-

tances are given, cleared of the effects of them. The index corrections obtained from all the observations of stars are also given. It is, however, left to those who may be desirous of using the observations of the comet to apply them, and also the effects of parallax, to the observed places.

The height of the observatory above the level of the sea (above 400 métres) caused the comet to be visible at this observatory longer than at most other places in Europe; and the author hopes that this circumstance may render the latter part of the series especially valuable, the southern position of the comet and the unfavourable state of the weather causing the observations of it to be in general very scarce, after its perihelion passage.

II. Note on the Masses of *Venus* and *Mercury*. By R. W. Rothman, Esq.

If the longitudes of the ascending nodes of *Mercury*, *Venus*, *Mars*, *Jupiter*, *Saturn*, and *Uranus*, be respectively designated by $☊_0$, $☊_1$, $☊_2$, $☊_3$, $☊_4$, $☊_5$, $☊_6$, and the inclinations of their orbits to the ecliptic by ι_0, ι_1, ι_2, ι_3, ι_4, ι_5, ι_6, then the motion of the node of *Venus* on the real ecliptic may, as is well known, be expressed by the equation:—

$$\begin{aligned}\frac{d☊_1}{dt} = &-\left\{(1,0)+(1,2)+(1,3)+(1,4)+(1,5)+(1,6)\right\}-(2,1)\\ &+\left\{(1,0)-(2,0)\right\}\frac{\tan\iota_0}{\tan\iota_1}.\cos(☊_1-☊_0)\\ &+\left\{(1,3)-(2,3)\right\}\frac{\tan\iota_3}{\tan\iota_1}.\cos(☊_1-☊_3)\\ &+\left\{(1,4)-(2,4)\right\}\frac{\tan\iota_4}{\tan\iota_1}.\cos(☊_1-☊_4)\\ &+\left\{(1,5)-(2,5)\right\}\frac{\tan\iota_5}{\tan\iota_1}.\cos(☊_1-☊_5)\\ &+\left\{(1,6)-(2,6)\right\}\frac{\tan\iota_6}{\tan\iota_1}.\cos(☊_1-☊_6)\end{aligned}$$

where (1,0), (1,2) ...... (2,0), (2,1) ...... are certain quantities depending on the masses and major-axes.—*Mécan. Céleste*, Vol. I. p. 295. I have calculated these quantities with the following values, multiplying the masses assumed respectively by the factors $(1+\mu_0)$, $(1+\mu_1)$, $(1+\mu_2)$ ...... where μ_0, μ_1, μ_2, are undetermined corrections of the same.

	Masses Assumed.	Semiaxes Major.
☿	$\frac{1}{1909706}$	0·38709812
♀	$\frac{1}{401839}$	0·72333230
♁	$\frac{1}{356354}$	1·00000000
♂	$\frac{1}{2680337}$	1·52369352
♃	$\frac{1}{1046 \cdot 77}$	5·20116636
♄	$\frac{1}{3512}$	9·53787090
♅	$\frac{1}{20000}$	19·18330500

I have thus obtained

$$
\begin{array}{ll}
(1,0) = (1+\mu_0)\ 0''\!\cdot\!44799 & (2,0) = (1+\mu_0)\ 0''\!\cdot\!10351 \\
(1,2) = (1+\mu_2)\ 6\cdot86011 & (2,1) = (1+\mu_1)\ 5\cdot17404 \\
(1,3) = (1+\mu_3)\ 0\cdot10205 & (2,3) = (1+\mu_3)\ 0\cdot29823 \\
(1,4) = (1+\mu_4)\ 4\cdot21136 & (2,4) = (1+\mu_4)\ 7\cdot08278 \\
(1,5) = (1+\mu_5)\ 0\cdot19836 & (2,5) = (1+\mu_5)\ 0\cdot32565 \\
(1,6) = (1+\mu_6)\ 0\cdot00425 & (2,6) = (1+\mu_6)\ 0\cdot00692
\end{array}
$$

Taking then the following values of the nodes and inclinations for 1800 (which is nearly a mean epoch, for the interval I am about to consider), viz.:—

$$
\begin{array}{ll}
☊_0 = 45^\circ\ 57'\ 9'' & \iota_0 = 7^\circ\ 0'\ 5''\!\cdot\!9 \\
☊_1 = 74\ \ 51\ \ 41 & \iota_1 = 3\ \ 23\ \ 28\cdot5 \\
☊_2 = 0\ \ 0\ \ 0 & \iota_2 = 0\ \ 0\ \ 0 \\
☊_3 = 47\ \ 59\ \ 38 & \iota_3 = 1\ \ 51\ \ 6\cdot2 \\
☊_4 = 98\ \ 25\ \ 45 & \iota_4 = 1\ \ 18\ \ 51\cdot6 \\
☊_5 = 111\ \ 56\ \ 7 & \iota_5 = 2\ \ 29\ \ 35\cdot9 \\
☊_6 = 72\ \ 59\ \ 21 & \iota_6 = 0\ \ 46\ \ 28\cdot0
\end{array}
$$

I obtain, finally,

$$
\begin{aligned}
\frac{d☊_1}{dt} = &-17''\!\cdot\!5632 \quad +0''\!\cdot\!1770\,\mu_0 - 5''\!\cdot\!1740\,\mu_1 - 6''\!\cdot\!8601\,\mu_2 \\
&- 0\cdot1975\,\mu_3 - 5\cdot2304\,\mu_4 - 0\cdot2730\,\mu_5 - 0\cdot0051\,\mu_6
\end{aligned}
$$

Now, Mr. Glaisher's researches give the observed tropical motion of the node from 1750 to 1836 $= +30''\!\cdot\!95$ annually, which, with a mean precession of $50''\!\cdot\!22$, leaves for the annual sidereal motion of the node $-19''\!\cdot\!27$:—

$$
\begin{aligned}
\therefore\ -1''\!\cdot\!71 = 0''\!\cdot\!177\,\mu_0 - 5''\!\cdot\!174\,\mu_1 - 6''\!\cdot\!860\,\mu_2 - 0''\!\cdot\!198\,\mu_3 \\
-5\cdot230\,\mu_4 - 0\cdot273\,\mu_5 - 0\cdot005\,\mu_6
\end{aligned}
$$

And it follows, that if the masses of the other planets be rightly determined, the mass of *Venus* must be increased by about three-tenths.

To form an idea of the possible error of this determination, let us suppose the assumed mass of *Uranus* to be wrong by one-fourth, that of *Saturn* by one-tenth, that of *Jupiter* by one-hundredth, *Mars* one-fifth, the *Earth* one-hundredth, and *Mercury* one-fifth, which are liberal allowances. Let us also suppose, which is very improbable, that the signs of these errors are such as to affect the result with the sum of the whole. Finally, let us admit an uncertainty on $\frac{d\Omega_1}{dt}$, as found from observation, to the extent of 0″·05. Yet, with all these suppositions, μ_1 cannot be less than $\frac{1\cdot46}{5\cdot17} = 0\cdot28$. I may add, that the results here obtained appear to indicate that the mass of *Mercury* should be diminished; and this is confirmed by a consideration of the motion of the perihelion of *Venus*. This motion is expressed by the following equation, in which ϖ_0, ϖ_1, ϖ_2, represent the perihelia of the respective planets.

$$\frac{d\varpi_1}{dt} = \left\{(1,0)-(1,0)\frac{e_0}{e}\cos.(\varpi_0-\varpi_1)\right\}\{1+\mu_0\}$$
$$+\left\{(1,2)-(1,2)\frac{e_2}{e_1}\cos.(\varpi_2-\varpi_1)\right\}\{1+\mu_2\}$$
$$+\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$\&c. \qquad \&c. \qquad \&c.$$

Reducing this expression to numbers with the same assumed masses as before, and taking the values of e_0, e_1, e_2, &c. ϖ_0, ϖ_1, ϖ_2, &c. for 1800, I get

$$\frac{d\varpi_1}{dt} = -2''\cdot4137 - 4''\cdot5753\,\mu_0$$

after supposing μ_2, μ_3, μ_4, μ_5, μ_6, each equal to zero. (It will be remarked that the mass of *Venus* does not enter into this expression.) Now, Mr. Glaisher has obtained for the annual tropical motion of the perihelion of *Venus* 49″·62, or, with a precession of 50″·22, a sidereal motion of —0″·60 : —

$$\therefore\ -0''\cdot60 = -2''\cdot41 - 4''\cdot58\,\mu_0$$
$$\text{and } \mu_0 = -0\cdot40 \text{ approximately.}$$

From the uncertainty attaching to the observed amount of the perihelion motion of *Venus*, I only give this result as confirming the idea that the mass of *Mercury* must be diminished.

On the whole, it is very remarkable that the planetary masses given in the *Mécanique Céleste* (Vol. III. p. 61), satisfy the secular motions affecting the orbit of *Venus* much better than the masses of later astronomers. It appears that in later times the mass of *Mercury* has been too much increased, and that of *Venus* too much diminished. What has been previously remarked concerning the masses of *Venus* and *Mercury* is confirmed by the motion of the

node of *Mercury*. If this motion be calculated by theory with the masses of the *Mécanique Céleste*, the result agrees almost exactly with the motion determined from observation by Lindenau.—See his *Tabulæ Mercurii*, p. 9.

III. Observations of the Immersion of p^2 *Leonis* behind the Dark Limb of the Moon. By R. Snow, Esq.

The observed Ashurst sidereal time of the immersion was 15^h 37^m $23^s \cdot 9$. The observation was made with a power of 75 on the telescope of the five-feet equatoreal, under very favourable circumstances.

IV. Extract of a Letter from Professor Encke to Mr. Airy, dated 20th December, 1841. Translated from the German. Communicated by G. B. Airy, Esq.

The comet of short period comes to perihelion on the 12th of April next; and judging from its present course, and from former experience during Mr. Henderson's residence at the Cape of Good Hope, it may be well observed there during the end of April and the whole of May, and probably also in June. May I then trouble you with the request to get the accompanying ephemeris conveyed there, or to the southern hemisphere generally, and also to provide for its circulation in England? I should think that, with the present active communication between England and all parts of the world, there is yet time enough to send the ephemeris to the astronomer at the Cape of Good Hope by the end of March; and it would have the greater interest for him, because, to this time, *no* return of the comet since 1819 has been missed. The ephemeris is not strictly founded on all the earlier observations, because it was impossible for me, notwithstanding all my endeavours, to reduce completely the observations of the comet made here in 1838. The compared stars still required some more observations for their determination. Meanwhile I have provisionally determined a correction of the last observations of 1838, or rather a correction of the calculations relating to that time, which will not be far from the truth. Upon this provisional reduction, and the calculation of the perturbations produced by *Jupiter alone*, the elements now given for 1842 are founded. Judging, however, from earlier experience, I believe that even with this incomplete calculation, the predicted place will be wrong by only about a few minutes. The error certainly cannot amount to half a degree, consequently the comet must be found, if it is really visible, and if the search be made with care.

The difference between the observation and the calculation, in the year 1838, is with great probability to be attributed to a very important error in the hitherto received mass of *Mercury*. This mass is the same that Lagrange, in the *Berlin Memoirs* 1782, has derived from a hypothesis on the density of the planets, according to which the density ought to increase very much with their proximity to the sun. Laplace has adopted the number without alteration. In

order to obtain approximately the mass of *Mercury*, which has *never* yet been determined, I have first substituted the most accurate values of the other masses. Thanks to your excellent observations, which are completely confirmed by Bessel (Bessel finds the mass of *Jupiter* $= \frac{1}{1047 \cdot 871}$), we are quite clear about the most important of them, that of *Jupiter*. The others have a smaller influence, even if they are erroneous. I have then used a double calculation. In the first I have assumed that till 1835 inclusive, the earlier determined elements were quite accurate. In fact, the errors of 1832 and 1835 are small. With this assumption I have sought to remove the apparent error in 1838 by an alteration of the mass of *Mercury* and an alteration of the constant of the resistance. This succeeded perfectly, and I obtained a slightly changed constant of the resistance and a mass of *Mercury* $= \frac{1}{3091947}$, about $\frac{2}{3}$ of the former mass. In the second calculation I have endeavoured to remove the errors of 1832 and 1835 as well as that of 1838 (each being previously treated as if it had arisen from an erroneous time of perihelion passage), by an alteration of the same elements; namely, the mass of *Mercury*, and the constant of the resistance. Here remain likewise only small differences, if the constant of the resistance is left quite unchanged, and the mass of *Mercury* assumed at $\frac{1}{4865751}$, or about $\frac{5}{12}$ of that of Lagrange. This last determination I intend to adopt as a basis for accurate comparison, and to correct it more closely. It has the truly remarkable peculiarity that with it the *densities* are,—

☿ = 1·12	♀ = 0·92	♁ = 1·00	♂ = 0·95
☉ = 0·25	♃ = 0·24	♄ = 0·14	♅ = 0·24

Consequently, the solar system appears to be composed of two sets of bodies, whose densities are either nearly = 1 or = $\frac{1}{4}$, and which are separated by the interval between *Mars* and *Jupiter*, where there exists no large planet, but only the four small ones.

Ephemeris of the Periodical (Encke's) Comet, 1842.

ELEMENTS.

Epoch, 1842, April 12·0^h Berlin mean time.
Mean anomaly .... 359° 58′ 34″·3
Sidereal daily motion .... 1070″·61433
Angle of eccentricity .... ϕ = 57° 39′ 13″·8 ($e = \sin\phi$)
Longitude of perihelion .... 157 30 4 ·7 } Mean Equinox
Longitude of ascending node .... 334 39 1 ·8 } of April 12, 1842.
Inclination .... 13 20 24 ·8

The Right Ascensions and Declinations are referred to the Mean Equinox of April 12, 1842.

1842. 0^h Berlin Mean Time.	Right Ascension of Comet: In Arc.	Right Ascension of Comet: In Time.	Declination of Comet.	Comet's Log. Distance from the Earth.	Comet's Log. Distance from the Sun.
	° ′ ″	h m s	° ′ ″		
Jan. 26	351 30 5	23 26 0·3	+ 4 46 57	0·29172	
28	352 13 3	23 28 52·2	5 3 20	0·29020	
30	352 57 19	23 31 49·3	5 20 21	0·28847	0·15938
Feb. 1	353 42 55	23 34 51·7	5 38 0	0·28652	
3	354 29 53	23 37 59·5	5 56 17	0·28433	
5	355 18 14	23 41 12·9	6 15 13	0·28192	
7	356 8 1	23 44 32·1	6 34 47	0·27925	0·12431
9	356 59 15	23 47 57·0	6 54 59	0·27634	
11	357 51 59	23 51 27·9	7 15 49	0·27316	
13	358 46 15	23 55 5·0	7 37 16	0·26971	
15	359 42 6	23 58 48·4	7 59 21	0·26598	0·08421
17	0 39 36	0 2 38·4	8 22 3	0·26196	
19	1 38 47	0 6 35·1	8 45 22	0·25762	
21	2 39 44	0 10 38·9	9 9 17	0·25297	
23	3 42 32	0 14 50·1	9 33 47	0·24799	0·03759
25	4 47 17	0 19 9·1	9 58 51	0·24265	
27	5 54 3	0 23 36·2	10 24 29	0·23694	
March 1	7 2 57	0 28 11·8	10 50 39	0·23084	
3	8 14 6	0 32 56·4	11 17 20	0·22433	9·98237
5	9 27 37	0 37 50·5	11 44 29	0·21736	
7	10 43 39	0 42 54·6	12 12 4	0·20992	
9	12 2 18	0 48 9·2	12 39 59	0·20197	
11	13 23 44	0 53 34·9	13 8 11	0·19345	9·91550
13	14 48 4	0 59 12·3	13 36 32	0·18433	
15	16 15 28	1 5 1·9	14 4 54	0·17454	
17	17 46 2	1 11 4·1	14 33 5	0·16401	
19	19 19 51	1 17 19·4	15 0 50	0·15267	9·83250
21	20 56 58	1 23 47·9	15 27 51	0·14041	
23	22 37 20	1 30 29·3	15 53 40	0·12713	
25	24 20 48	1 37 23·2	16 17 46	0·11268	
27	26 7 0	1 44 28·0	16 39 22	0·09691	9·72841
29	27 55 11	1 51 40·7	16 57 28	0·07962	
31	29 44 9	1 58 56·6	17 10 46	0·06061	
April 2	31 31 54	2 6 7·6	17 17 28	0·03964	
4	33 15 24	2 13 1·6	17 15 17	0·01650	9·60828
6	34 50 12	2 19 20·8	17 1 20	9·99102	
8	36 10 22	2 24 41·5	16 32 19	9·96325	
10	37 8 46	2 28 35·1	15 44 48	9·93347	
12	37 38 22	2 30 33·5	14 36 1	9·90241	9·53775
14	37 33 57	2 30 15·8	13 4 45	9·87118	
16	36 53 55	2 27 35·7	+ 11 12 0	9·84115	

1842. 0h Berlin Mean Time.		Right Ascension of Comet — In Arc.	Right Ascension of Comet — In Time.	Declination of Comet.	Comet's Log. Distance from the Earth.	Comet's Log. Distance from the Sun.
		° ′ ″	h m s	° ′ ″		
April	18	35 40 44	2 22 42·9	+ 9 1 3	9·81365	
	20	34 0 5	2 16 0·3	6 36 53	9·78968	9·60596
	22	31 59 10	2 7 56·7	+ 4 5 15	9·76985	
	24	29 45 28	1 59 1·9	− 1 31 50	9·75428	
	26	27 25 26	1 49 41·7	0 58 32	9·74276	
	28	25 4 15	1 40 17·0	3 22 15	9·73482	9·72610
	30	22 45 37	1 31 2·5	5 37 0	9·72987	
May	2	20 31 57	1 22 7·8	7 41 41	9·72732	
	4	18 24 36	1 13 38·4	9 36 5	9·72658	
	6	16 24 5	1 5 36·3	11 20 40	9·72716	9·83064
	8	14 30 21	0 58 1·4	12 56 16	9·72866	
	10	12 43 0	0 50 52·0	14 23 53	9·73074	
	12	11 1 20	0 44 5·3	15 44 38	9·73316	
	14	9 24 36	0 37 38·4	16 59 32	9·73574	9·91400
	16	7 51 55	0 31 27·7	18 9 34	9·73834	
	18	6 22 28	0 25 29·9	19 15 36	9·74088	
	20	4 55 23	0 19 41·5	20 18 25	9·74329	
	22	3 29 53	0 13 59·6	21 18 39	9·74554	9·98115
	24	2 5 12	0 8 20·8	22 16 53	9·74763	
	26	0 40 35	0 2 42·3	23 13 34	9·74954	
	28	359 15 21	23 57 1·4	24 9 6	9·75130	
	30	357 48 51	23 51 15·4	25 3 48	9·75292	0·03658
June	1	356 20 27	23 45 21·8	25 57 53	9·75446	
	3	354 49 34	23 39 18·3	26 51 30	9·75595	
	5	353 15 40	23 33 2·7	27 44 42	9·75743	
	7	351 38 16	23 26 33·1	28 37 31	9·75897	0·08333
	9	349 56 57	23 19 47·8	29 29 52	9·76064	
	11	348 11 24	23 12 45·6	30 21 36	9·76248	
	13	346 21 21	23 5 25·4	31 12 29	9·76458	
	15	344 26 39	22 57 46·6	32 2 15	9·76698	0·12357
	17	342 27 16	22 49 49·1	32 50 36	9·76976	
	19	340 23 17	22 41 33·1	33 37 10	9·77298	
	21	338 14 54	22 32 59·6	34 21 35	9·77668	
	23	336 2 26	22 24 9·7	35 3 29	9·78092	0·15871
	25	333 46 18	22 15 5·2	35 42 28	9·78574	
	27	331 27 4	22 5 48·3	36 18 12	9·79117	
	29	329 5 24	21 56 21·6	36 50 22	9·79724	
July	1	326 42 7	21 46 48·5	37 18 43	9·80397	0·18978
	3	324 18 5	21 37 12·3	37 43 1	9·81136	
	5	321 54 12	21 27 36·8	38 3 9	9·81942	
	7	319 31 28	21 18 5·9	38 19 3	9·82812	
	9	317 10 50	21 8 43·3	38 30 46	9·83745	0·21756
	11	314 53 15	20 59 33·0	38 38 24	9·84738	
	13	312 39 31	20 50 38·1	38 42 9	9·85786	
	15	310 30 26	20 42 1·7	38 42 14	9·86886	
	17	308 26 37	20 33 46·5	38 38 59	9·88032	0·24259
	19	306 28 33	20 25 54·2	38 32 43	9·89220	
	21	304 36 35	20 18 26·3	38 23 47	9·90443	
	23	302 51 0	20 11 24·0	38 12 32	9·91697	
	25	301 11 54	20 4 47·6	37 59 18	9·92977	0·26541
	27	299 39 21	19 58 37·4	37 44 24	9·94278	
	29	298 13 17	19 52 53·1	−37 28 9	9·95596	

V. Comparisons of the Planet *Venus* in Right Ascension and N. P. D. with the Star A. S. C. 423, made with the Equatoreal Instrument of the Observatory at Ashurst, on April 9, 1841. By R. Snow, Esq.

The equatoreal instrument employed for these observations is of Fraunhofer's construction, and furnished with clockwork; the object-glass is of five feet focal length, and of four inches aperture. It is supported on a very firm pier, and retains its position very well.

The observations were made with a position micrometer, adjusted for transit and declination observations. They consist of thirty transits of the star and of the first limb of *Venus* over the meridian wire, and of nine micrometrical measures of the differences of N. P. D. of the star and the south limb of the planet: the corrected sidereal times of the observations are given.

The value of a revolution of the micrometer-screw had been determined by 400 transits of stars near the equator. Measures of the semi-diameter of *Venus* were made at the same time, by which it was found that the measured value exceeded the tabular value given in the *Nautical Almanac* by 3″·1.

The circumstances of the observations were favourable.

VI. Reduction of Mr. Snow's Observations of *Venus* and the Star A. S. C. 423, with some remarks upon the employment of equatoreals in Planetary Observations. By the Rev. Richard Sheepshanks.

Mr. Snow's observations admitted of being so grouped as to furnish four sets of comparisons in right ascension and five sets in declination. The effects of parallax and refraction were computed by the formulæ used at Greenwich (*Greenwich Observations*, 1836, pages lxiv. and lxv.) The right ascension of the star was taken from Lord Wrottesley's Catalogue, the declination from the A. S. Catalogue, and the semidiameter of *Venus* from Mr. Snow's Observations; and thus the right ascension and declination of the planet were obtained for the Ashurst sidereal times of observation and compared with the places interpolated from the *Nautical Almanac* for the same times. The resulting corrections to be applied to the right ascensions and declinations of the *Nautical Almanac* are as follow:

	Right Ascension.	Declination.
	s	″
	− 1·10 from 15 obs.	+ 3·1 from 1 obs.
	− 1·32 ... 5	+ 5·4 ... 1
	− 1·25 ... 5	+ 5 3 ... 4
	− 1·27 ... 5	+ 5·2 ... 2
		+ 3·6 ... 1
Mean	− 1 19 from 30 obs.	Mean + 4·9 from 9 obs.

The mean epoch is about $8^h\ 30^m$ Greenwich mean solar time.

The author remarks generally with respect to the treatment of such observations, that they may be boldly grouped without sensible

error, so as to make one reduction serve for a considerable number of observations; and that to ensure the greatest facility for grouping, the observations of one element (if both cannot be made simultaneously) should be repeated several times as rapidly as possible alternately with similar sets of observations of the other element.

With respect to the value of such observations, the results above given will shew that an equatoreal, when thus used, is no mean rival to meridian instruments. The star can be subsequently determined with any required degree of accuracy, and the observations can be made with as great freedom from constant error with an equatoreal as in the meridian. In this latter respect, indeed, the power of repetition gives to the equatoreal a great superiority, and may be made to counterbalance the disadvantages arising from want of steadiness. The last-named quality can, however, in most instances, be obtained in as great a degree as is requisite. The hour-circle being firmly clamped, if the instrument be well balanced, sudden changes can arise only from careless handling.

The supposed uncertainty and instability of the adjustments are probably the principal obstacle to the free use of equatoreals in England; but the author considers that most equatoreals can be adjusted very nearly, and that when ordinary care has been taken, the position remains sufficiently permanent; and it is certain that when rationally used, the effect of any unavoidable derangement is so nearly annihilated as to be quite insensible. The *difficulty* of performing the adjustments of an equatoreal is very trifling, if it be methodically undertaken, and the residual errors much smaller than would at first sight seem possible. With well-turned collars and pivots an error of half a minute, arising from flexure or other causes, must be looked upon as an impossible quantity, in which case the differential effects upon objects in the zodiac might be disregarded. With respect to methods of observing, the author recommends that the telescope be moved in declination like a transit, in order that the star and planet may pass over the same part of the wire. In this case reliance is placed only on the adjustment of the cross-axis; but when the declination is not changed, it is presumed that the position of the wire is correct; and this can be ascertained with only a moderate degree of certainty. In equatoreals which can be reversed in every position, the observations should be made, one group in one position, and the second in the position reversed. The best wiring for such observations, the author considers to consist of three, five, or seven immovable wires, at equal distances, and parallel to the meridian, transit-wires, in fact, and seven equidistant wires at right angles to these, at 5′ interval, the plate which carries the latter wires being moved by a micrometer-screw. The advantages of this system are a saving of time in screwing the micrometer, less wear of the screw, and less dependence on it for large intervals.

Thus far it has been shewn, that an equatoreal instrument may be made to rival meridian instruments, by the bestowal of a little more time and trouble; there are, however, many cases where the equa-

toreal is more convenient, and many where *it* can, and the *others* cannot be used.

A planet which comes to the meridian at a late or inconvenient hour of the night may be observed several hours earlier with the equatoreal. In so variable a climate as ours, it is not too much to say, that the number of good planetary observations might be thus very much increased; and if an equatoreal were steadily directed to this object in the southern hemisphere, to meet the case where the planet has considerable south declination, we should soon have the materials by which the present sufficiency of theory might be satisfactorily tested. The superior planets cannot always be observed in full daylight with large meridian instruments, yet equatoreals of even a small size might be made to determine their places with great accuracy after sunset. Again, large equatoreals, which are now tolerably abundant, might take charge of the minor planets. Micrometrical observations only have been taken notice of in the preceding remarks, the divided circles of the instrument being considered only as finders, and for performing the adjustment, though in some instruments they are large and good enough to be used in differential observations. Still the proper use of the equatoreal is the ascertaining of small differences by means of the micrometer and time.

In conducting the observations, the author recommends that there should be made each night two or three transits of the star of comparison, and of two other stars, one above and one below it a few degrees, the instrument being clamped in right ascension, by which means it would be made evident whether the derangement of the adjustment had any sensible effect upon the place of the planet. It is the want of observations to *accuse* derangement which makes the stars observed as moon-culminators less satisfactory than if they were more widely spread in declination.

With respect to observations of the moon, the author mentions one set, originally suggested by Struve, but never carried into effect. There are three observations which might be made when a bright star is occulted by or reappears from under the moon's bright limb:—

1. The time of disappearance or reappearance of the star.

2. Micrometrical measures of distance between the star and the moon's bright limb, the clock-work and the wire micrometer with the slipping piece being used.

(This is the common observation of distance, and might be usefully applied to the case of a near approach.)

3. Differences of right ascension between the moon and star, the hour-circle being clamped as in ordinary transit observations.

If the place of the moon be computed from these three observations, we *ought* to arrive at the same result; and if we do not, the difference between the first and second result arises from the moon's irradiation, and will give a measure of it; also a difference between the second and third results would shew some error in the mode of

taking the transit of the moon's limb, which is at present rather a doubtful point in practical astronomy. If by certain corrections, constant either to the observer or the telescope, these results can be made to agree in *each* case, and *always* the longitude might be determined in a shorter period, though with more calculation than at present, and a greater certainty be obtained from transits of the moon's limb.

In conclusion, the author hopes that the attention of persons who possess good equatoreals may be directed to the planets whenever those bodies are favourably situated with respect to an observable star. The adjustment is really nothing, and if pairs of stars above and below be observed, any error arising from mal-adjustment can be ascertained and allowed for. The artist will take care, if warned, that the cross-axis shall be at right angles to the polar axis, and the reductions, in ordinary cases, are very trifling, especially if by judicious grouping one reduction is made to serve for several observations.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *February* 11, 1842. No. 20.

Report of the Council of the Society to the Twenty-second Annual General Meeting, held this day.

The return of the Anniversary of this Society affords the Council another opportunity of congratulating the members on the steady and progressive improvement of the science, for the promotion and encouragement of which they were originally formed. The public press has furnished us with the usual productions of the national observatories, while the labours and pursuits of private individuals have gradually added to that stock of knowledge and information which it is our desire to accumulate and place on record. The numerous books that have been presented to the Society shew the activity that has prevailed, both in this country and on the continent; and the papers, that have been read at the meetings, exhibit the interest which our members take in the progress of astronomy.

The Council are happy in being able to announce, that the expences of the Society have been kept greatly within the annual income; and that, in fact, the sum of £400 Consolidated 3 per Cents Stock has, during the last year, been purchased out of a portion of the surplus. This sum, together with £500 already in that fund, is available for any expences of the Society, should it at any future time be required; and is totally distinct from the sum of £1979 5*s*. 1*d*. New 3½ per Cents, which are the regular investments of the compositions of members, from time to time, agreeably to the original regulation of the Society, and are never appropriated to current expences. The Report of the Auditors, which is here subjoined, will shew the state of the finances of the Society.

RECEIPTS.

	£.	s.	d.
Balance of last year's account	448	16	3
1 year's dividend on £900 Consols	27	0	0
½ year's ditto on £1957 19s. 8d. New 3½ per Cents	34	5	3
½ year's ditto on £1979 5s. 1d. ditto	34	12	8
On account of arrears of contributions	88	4	0
83 annual contributions (1841–1842)	174	6	0
4 ditto (1842–1843)	8	8	0
2 compositions	42	0	0
11 admission fees	23	2	0
9 first year's contributions	18	18	0
Sale of Memoirs, by J. Hartnup, from Jan. 30, 1841, to Jan. 29, 1842	31	16	0
	£931	8	2

EXPENDITURE.

				£.	s.	d.
Purchase of £42 8s. 8d. New 3½ per Cents				42	0	0
Ditto of £400, 3 per Cent Consols				351	10	0
W. Magnay, for paper				54	11	6
Moyes and Barclay, for printing Monthly Notices, &c. for Session 1840-1841				22	18	0
W. Cubitt, for work done on basement story				12	14	7
1 year's salary to the assistant-secretary				80	0	0
Harrison and Co. for stationery				7	8	3
J. Hartnup, for commission on collecting £344 14s.				17	4	9
Postage of letters				12	10	4
Charges on books, and carriage of parcels				3	3	6
Porter's and charwoman's work, &c.				7	6	6
Tea, sugar, cakes, &c. for the evening meetings				13	13	0
Coals, candles, &c.				11	8	0
Conveyance of Memoirs and Greenwich Observations				3	19	8
Sundry disbursements by the treasurer				6	11	10
Taxes: Poor's rate	4	17	9			
Taxes: Land tax	3	2	6			
Taxes: Window duty	2	12	4			
Taxes: Church and rector's rate	2	6	9			
				12	19	4
Balance in the hands of the treasurer (Jan. 29, 1842)				271	8	11
				£931	8	2

The assets and present property of the Society are as follow:—

				£.	s.	d.
Balance in the hands of the Treasurer				271	8	11
Arrears 29th January, 1842.						
3 contributions of four years' standing	£25	4	0			
4 ——— of three ditto	25	4	0			
6 ——— of two ditto	25	4	0			
31 ——— of one ditto	65	2	0			
1 admission fee	2	2	0			
1 first year's contribution	1	1	0			
				143	17	0
£1979 5s. 1d. 3½ per Cent Annuities, £900 3 per Cent Consols } valued at				£2850	0	0
In the hands of J. Weale, on account of Memoirs sold by him				38	3	3

Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year, viz. : —

	Compounded.	Annual Contributors.	Non-resident.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1841	91	127	88	6	312	36	348
Since elected	2	10	...	...	12	...	12
Deceased	...	−2	−2	...	−4	−1	−5
Resigned	...	−6	...	...	−6	...	−6
Removals	...	2	−2	...	...	...	...
February 1842	93	131	84	6	314	35	349

With respect to the instruments of the Society, it may be proper to place on record the present state of them.

The *Harrison* clock,
The *Owen* double portable circle,
The *Owen* quadruple portable sextant,
The *Beaufoy* circle,
The *Herschelian* 7-feet reflector,

are in the apartments of the Society.

The brass quadrant, said to be Lacaille's,

is in the apartments of the Royal Society, for safe custody.

The Standard Scale

is deposited at Mr. Baily's, for safe custody.

The remainder are lent, during the pleasure of the Society, to the several parties undermentioned : viz.

The *Fuller* theodolite, to the Lords of the Admiralty.
The *Beaufoy* clock, } to the Royal Society.
The two invariable pendulums, }
The *Beaufoy* transit, to Mr. T. Jones.
The *Lee* circle, to Mr. Hopkins.
The *Wollaston* telescope to Professor Schumacher.
The other *Beaufoy* clock, to Major-General Pasley, R.E.

Amongst the losses by death, the Council have here to notice one among the Foreign members, which was announced at the last anniversary, namely, that of Professor Littrow, of Vienna, who was one of the earliest members of this Society. He contributed several papers which were read at the meetings, and which have been printed in the first four volumes of the *Memoirs*, exhibiting a spirit of research and inquiry into a variety of subjects connected not only with astronomy, but also with other branches of physical science. He was also the author of a valuable Treatise on Astro-

nomy, in 3 volumes octavo, in the German language, and continued, till the time of his death, to conduct the affairs of the Imperial Observatory at Vienna.

The Council have also to regret the loss of Mr. Frend, lately one of the Members of the Council of the Society.

William Frend was the son of George Frend, an alderman of Canterbury, in which city he was born, November 22, 1757. He received his education in his native place, at the King's School; and, after staying some time at St. Omer, was placed in a mercantile house at Quebec; but the breaking out of the disturbances in America destroyed his commercial prospects, and he returned to England. His wishes being directed towards the Church, he was placed at Christ's College, Cambridge, in 1775, and took the degree of B. A., with the honour of second wrangler, in 1780. After taking his degree, he almost immediately removed to Jesus College, of which he was elected fellow and tutor. In 1783 he was ordained, and afterwards obtained the living of Madingley, near Cambridge. In 1787, a change in his religious opinions took place, which ended in his adoption of the views of the Unitarians. The resignation of his living and the loss of the tutorship followed of course; but the laws of the University still allowed him to retain his fellowship. After some years of travel, he returned to Cambridge, and occupied himself further in the study of Hebrew and divinity. In 1793, a pamphlet, entitled *Peace and Union recommended to the Associated Bodies of Republicans and Antirepublicans*, was published by him, which contained distinct expressions of dislike to the doctrines and discipline of the Established Church. Immediately upon the publication of this pamphlet, both his college and various members of the senate commenced proceedings against Mr. Frend. The master and fellows of the former (by seven to four) "removed" him from residence in college, until proof of "good behaviour," and this sentence was confirmed by the visitor. Thirty-four members of the senate cited the author of the pamphlet before the Vice-chancellor (Dr. J. Milner), and a trial took place in his court, which lasted eight days. The result was, that a form of recantation was proposed to Mr. Frend, which he refused to sign; and sentence of "banishment" from the University was passed. This banishment is not expulsion, as persons unacquainted with the University generally believe, but a deprivation of the right to reside within the limits of the University; and, accordingly, though the sentence was confirmed on appeal, Mr. Frend continued to hold his fellowship till his marriage, and remained to the day of his death a Master of Arts, and a member of Jesus College. He retired of course from Cambridge, and came to London, where he maintained himself till 1806, by adding the profits of teaching and writing to the income derived from his fellowship. When the Rock Life Assurance was founded (1806), Mr. Frend, who had previously been consulted in the formation, was appointed actuary of that company, a post in which he remained until a severe illness compelled him (in 1826) to retire

from active life. His health, however, recovered, and he continued his mental employments with an activity very unusual at his age, until the beginning of the year 1840, when he was attacked by paralysis, under which he lingered with almost total loss of speech and motion, though with the smallest possible decay of mind or memory, until February 21 of the last year, when he closed a life which is regarded, even by those who differed from him, as a splendid example of honesty in the pursuit of truth, and of undaunted determination in the assertion of all that conscience required.

The losses and inconveniences which attended his banishment from college were not among the greatest risks which he ran. At a subsequent period, when the political struggle was at its height, and government prosecutions were frequently directed against the mere expression of opinion, Mr. Frend was one of the foremost among the despised minority which advocated emancipation and enfranchisement for all who were under religious or political disqualifications. At the time of certain of the prosecutions alluded to, it was currently said, that had the government succeeded in obtaining convictions, there was an intention of instituting several more; and Mr. Frend, it was stated, was to have been one of the defendants. This supposition cannot now be verified, even if it were true; but the rumour itself constitutes its object one of the leading opponents of the system which has since been so materially modified. With his political writings,* of which there were several, we have here nothing to do, any more than with those of a religious character. A true account of his scientific views cannot be easily given in a short space; nor can reasons for enlargement be better given than in the description itself of these views.

It generally happens that in recording the career of our departed members, we have little to say on their opinions, but only to specify the manner in which they carried them into practice; and small space may serve for great results. In the present instance we have to point out the singularities of thought which made Mr. Frend the last, we should suppose, of the *learned* Anti-Newtonians, and a noted oppugner of all that distinguishes algebra from arithmetic. Opposition to the theory of gravitation must in future be left to those whose mechanics do not distinguish velocity from force; and the rejection of the distinctive principles of algebra to those who would teach like philosophers what they have learnt like schoolboys, without going through any intermediate stage. But the subject of the present Memoir stands in neither of these predicaments; and it would be highly interesting in itself, and no less than due to expiring tenets, to specify the probable influences under which such a mind as that of Mr. Frend directed him to stand quite alone among men of his philosophical acquirements; especially when it is considered that, up to the age of thirty-six,

* The titles of these will be found in the *Gentleman's Magazine* for May 1841 (pp. 541–543).

he had been a successful teacher of those scientific doctrines which he afterwards opposed, both by serious argument and ridicule.*

Undoubtedly the prime mover of this curious change was the alteration which took place in his doctrinal views of religion. Having been led to conclude that he had been betrayed by authority into the belief of propositions both inexplicable and false, the tendency to think that the inexplicable must be false, or at least to regard the former with strong suspicion, was a necessary ingredient of his future reflections on all subjects. The manner in which several leading doctrines of physics and mathematics had been handled by names of celebrity, was highly calculated to call out this disposition. The doctrine of attraction,—a mysterious connexion between matter and matter, with no existence but in its results; the theory of quantities less than nothing, a phrase which, arithmetically considered, is a simple contradiction of terms, were adopted at the time when Mr. Frend taught, in a most positive and substantive sense, by the majority of investigators and all elementary writers.

It was in vain that Newton, obviously hoping for some further elucidation of his great regulator, concluded the *Principia* with a caution that he had *not yet* (*nondum*) found out the source of gravitation; his successors and commentators, with one voice, pronounced him the discoverer of the final mechanical cause of the planetary motions; and popular writers, who seldom refuse to say B when their leaders have said A, added that Newton had found out *why* water runs down hill. With respect to algebra, the matter was still worse. Euler asserted downright that a penniless man, fifty crowns in debt, has fifty crowns less than nothing; and offered proof. He assumes that a gift of fifty crowns would make this man richer; and supposing him to employ the gift in the payment of his debts, then concludes that he *had* less than nothing, because, being now richer than before, he *has* only nothing. Others admitted the negative and impossible quantities as mysteries, and, reversing Mr. Frend's process, brought them forward as auxiliaries to the mysteries of the orthodox forms of Christianity; a practice not extinct in our own day, even after all that was inexplicable about impossible quantities has disappeared. At the time when Mr. Frend first thought on the subject, the assertion of mystery was the escape from the confession of incompleteness; the great mass of readers followed with implicit confidence, while, of those who thought for themselves, an enormous majority was too sensible of the value of the results of algebra to abandon it on account of difficulties. Some few rejected the peculiar doctrines of algebra altogether; among whom those of most note were, in succession,

* In a magazine which lasted for a few months of 1803, *The Gentleman's Monthly Miscellany*, of which Mr. Frend was editor, or co-editor, is an article by him, entitled "Pantagruel's Decision of the Question about Nothing," in which the manner of Rabelais is so well caught, that any one on a first perusal would think it likely to be an actual adaptation or parody, until a search through the writings of Rabelais satisfied him that it was simple imitation. It is a satire against some parts of algebra.

Robert Simson, Baron Maseres, and Mr. Frend. Most of those who were independent of authority united in blaming the method of the elementary writings, and were content to hope that a palpable guide to truth would not always be without rational connexion with undeniable axioms. Woodhouse, the restorer of thought on first principles at Cambridge, in a letter to Baron Maseres, preserved among Mr. Frend's papers, and dated November 16, 1801, distinctly lays it down that, in these matters, it is not the principles which prove the conclusions, but the truth of the conclusions which proves that there must, somewhere or other, be principles. "Whether or not," says he, "I have found a logic, by the rules of which operations with imaginary quantities are conducted, is not now the question; but surely this is evident, that, since they lead to right conclusions, they *must have a logic*." And he goes on thus: "Till the doctrines of negative and imaginary quantities are better taught than they are at present taught in the University of Cambridge, I agree with you that they had better *not* be taught; and the plan of our system of mathematical education, much as it is praised, needs, in my opinion, considerable alteration and reform; and perhaps you think that our late mathematical publications will not much increase the love or improve the taste for luminous and strict deduction." As concerns the mystics, then, there is no need to object to Mr. Frend's entire abandonment of their principles, but the reverse; for it may be asserted that most of those who thought about first principles did the same. Those who imposed on matter, in the name of Newton, a primary power of attracting other matter, with those who could, on their own definitions, be made to say that *a command to subtract* 2, *repeated as many times as there are units in a command to subtract* 3, *gives a command to add* 6, ought to have been surprised that they found so little opposition.

But the circumstance relative to Mr. Frend's ultimate views which is peculiar to himself, and which cannot be remembered without surprise, is, that in clearing the trammels of mystery he had to force so thick an enclosure, that he left behind him not only the mysterious explanation, but the very facts which were professed to be explained, and which, it may be thought, could have admitted of no doubt. It seems to any one who reads his writings, that he means that Newton had done nothing out of mathematics, and that the *results* of algebra are all delusion. That the planets, attraction or no attraction, move about the sun, and are disturbed, precisely as it would be if there were attraction; that the truth of an equation, though produced by aid of impossible quantities, may be verified by numerical computation — may be made purely experimental realities, and would, to most minds as well acquainted with the subject as that of Mr. Frend, remain true, even though attraction were the atheism which some formerly called it, and the doctrine of negative quantities were a part of the black art. Nor would it have been wonderful if he had rejected incomplete explanations in elementary writing, the object of which is to teach clear results of clear principles. But there was more than this: sometimes, though rarely,

he seemed to have a power of admitting the facts as facts; but for the most part, when they were presented to him in conversation, his mind did not appear capable of dwelling on them long enough to decide whether an answer was required or not; they seemed to slip like water through a sieve. In this there was neither affectation nor evasion; it was a peculiar state of mind with regard to what could be contemplated as a scientific truth, and may be partly explained.

Mr. Frend had an admiration of simplicity, and an indisposition to arrive at complex results, which was perhaps a consequence of the desire to have no secret in philosophy. Next to the abandonment of all that was difficult to explain, followed the practical rejection of every thing in which the mind could not hold the full explanation at once before itself, in all its parts. The simple theory of numbers, that is, of integer numbers, was therefore naturally a favourite study; and this branch of mathematics is well known to be an extremely powerful stimulant of that disposition which leads to its pursuit. Legendre has said that it almost always becomes a species of passion with those who give themselves to it at all. With Mr. Frend it went still farther; an equation with a fractional root, even if commensurable, was a pseudo-equation; and $x^2 + y^2 = 1$, x and y being rational fractions, was an illegitimate child of $x^2 + y^2 = z^2$, x, y, and z being integers. In this particular Mr. Frend differed greatly from another remarkable person, his own most intimate friend, Baron Maseres, whose leading idea it seems to have been to calculate more decimal places than any one would want, and to reprint the works of all who had done the same thing.

There was also another peculiar circumstance which no doubt had considerable effect. Mr. Frend had studied Hebrew thoroughly, and was, in the opinion of learned Jews, better versed in that language than any English Christian of his day. No one who became acquainted with him could long avoid noticing the interest which he took in every matter directly or indirectly concerning the history and progress of Christianity. This knowledge of their language, history, and customs, with a community of opinion on the nature of the Deity, led him much into the acquaintance of his *elder brethren*, as he frequently termed them, of the Jewish race; and he would have held any biography of himself very imperfect which omitted to note how strongly he felt toward their persuasion. It seldom happens that any person devotes himself so keenly to any history without imbibing some opinion of the superiority of its subjects; and Mr. Frend carried to the very verge of paradox, or it may be a little beyond, the notion that the mathematical and astronomical science of ancient Judea was substantially equal at least to that of any period of modern Europe, not excepting the present. Their lunar calendar was as good as if it had been made from modern observations, and much better adapted to represent a long period than any other; as much of pure mathematics as any one ought to admit flourished among them in the time of Solomon. It is needless to say, that not a vestige of historical evidence was ever produced in favour of these opinions, nor did we ever hear of any

modern Jew who had carried his notions of the learning of his ancestors to such a length. Among modern nations, Mr. Frend had a peculiar respect for the Chinese, and was impressed with the opinion (not by any means peculiar to himself) that their government and social state is a model. The rudiments of science which he found among these nations, the ancient Hebrews and the modern Chinese, were easily magnified by his temperament, which was both sanguine and contemplative, into as much of astronomy and arithmetic as he had been able to save from the pollution of attraction and negative quantities; consequently, these countries were the depositories of real science, uncorrupted by sophistry. For the ancient Greeks and their writings he had an open contempt: they were children who had learned of the Jews, and spoiled their masters' doctrines: the good was due to their teachers, the bad was their own. All this time, and in the midst of such strange singularities of opinion as were never long absent from his mind, there was an eagerness to see the good of every thing actually present, which made his approbation very easy to gain. No one who talked with him could soon fathom the wide difference of sentiment between the two; for whatever might be the subject, there was a side on which it could be favourably viewed; and for that side Mr. Frend's mind, or that part of it which regulated his first expressions, had the quality (we must not say the attraction) of a magnet. His persuasion of the rapid advances which his contemporaries were making in morals, arts, and even sciences (however corrupted), was a spring of comfort to his age which never ran dry; and his interest in every thing new, which promised improvement to any class of mankind, in any one of those particulars, was, even after he was unable to speak or move, a commanding instinct, which he could not have disobeyed if he would. This unvarying effort to detect good in whatever came before him was essentially linked to his religious feelings, the source of his daily comfort, by the view which he never ceased to take of the ultimate consequences of Christianity; which he looked upon as the gradual restorer of mankind to a state of perfect goodness and knowledge. Every advance in art, learning, or science,—every amelioration of social evils,—every improvement in the law,—every evidence, however slight, of disposition to act, think, or hope, for the better, brought before him his cherished prospect of the final state of mankind, and was, in his opinion, only a step towards it. The consequence was, that any one who would wish to describe his age, must simply invert each and all of the characteristics which Horace* makes significative of the advanced periods of life.

* "Multa senem circumveniunt incommoda; vel quòd
Quærit, et inventis miser abstinet, ac timet uti,
Vel quòd res omnes timidè gelidèque ministrat
Dilator, spe longus, iners, avidusque futuri,
Difficilis, querulus, laudator temporis acti
Se puero, censor castigatorque minorum."

Mr. Frend's scientific writings were particularly distinguished by simplicity and earnestness. The greater part of the whole consists in short pamphlets, or communications to periodical publications; and many proofs might be given, both of the extreme importance he attached to truth, and of his conviction that error, even in matters of science, is a noxious weed in the field of morals. His principal distinct writings on subjects of science are his *Algebra* (Part i. 1796, Part ii. 1799), and his *Evening Amusements* (1804–1822). The latter was an astronomical elementary work of a new character, which had great success; and the earlier numbers went through several editions. It embraces a metonic cycle, and therefore describes the places of the moon, in a manner which would make it useful for a considerable time to come, in the elementary instruction for which it was intended. This present year is that which answers to 1804, so that the opportunity to repeat the process of instruction, so far as the moon is concerned, has just commenced. The phenomena of the different months are described, and to each month is usually attached a short religious reflection, an account of some astronomical process or discovery, a hit at the Newtonian philosophy, or some such preface. We do not see much acquaintance with the new doctrines of physics, which had then excited attention for some years; but it must be remembered that a man, who took his degree at Cambridge in 1780, had very little training in experimental deduction apart from mathematics.

Mr. Frend's scientific peculiarities strongly illustrate what those who have carefully considered the reading of that time will perhaps think to be the natural consequence of it, upon an exceedingly honest, clear, and decided mind, placed in circumstances favourable to the developement of opposition. The Cambridge student was isolated from experimental physics by the habits of his university, and from the progress of mathematics by its adherence to the fluxional notation. In essentials, the academic system was nearer to what it might have been at the death of Newton than those who now see its state could readily imagine to be possible: the theory of gravitation was taken wholly and solely from the *Principia;* no Englishman had made the smallest addition to it; and Clairaut, D'Alembert, &c. were only known by name as *French philosophers*, the most odious appellation of the time. One question might be asked which would, perhaps, add some force to the preceding remarks, if reasons for an answer were sought: — How came the men of science, who were bred at our English universities, to let Priestley, whose life was one turmoil of controversy, and who visibly must have written four pages a-day, or thereabouts, of theological discussion during his whole experimental career, run off with such a splendid portion of the first-fruits of real chemistry?

The other work of Mr. Frend, his *Elements of Algebra*, will lead every one who peruses it to think, with sincere regret, of his having preferred rejection to amendment; and will be a lesson to

writers yet to come, that they should let that stand which appears to lead to truth, whatever warning they may think it necessary to give that the reason why it does so lead is imperfectly understood. It is, on the points which it treats, the clearest book in our language. Something of this is due to the rejection of difficulty; something to the use of no problems except those which can be answered in integers; but there remains enough to shew that a work from such a writer, which should have taken algebra as it stood, distinguishing the part of which the logic was then complete from that of which the principles remained insufficiently understood, would have been the most valuable present which could have been made to the elementary student, and would, perhaps, have greatly accelerated the transition to the present state of the science, in which none need find a mystery. In all probability, the attack of Mr. Frend did materially influence this result. Among his papers is preserved a letter to him from M. Buée, a Frenchman residing in England, dated June 21, 1801, containing the form in which the perusal of Mr. Frend's work made the writer put together his own views of the subject; and admirably expressed. Of course it cannot be said how much suggestion was derived from the necessity of replying to specific objections: what is certain is, that in a few years from that time, this same M. Buée was, though in an imperfect manner, what Dr. Peacock calls the first formal maintainer of that exposition which removes the long standing difficulty.

Finally, whatever may be our opinion on the peculiarities of Mr. Frend's views, we must remember with high satisfaction that he was, during the last years of his life, one of our fellows; and, also, that no narrow idea of the necessity of conformity of opinion prevented a man of his intellectual station from being called to the Council of the Society. The sincere regret with which the Council announces the loss which our Body has sustained is materially lessened by the reflection that his extensive learning, practical wisdom in the affairs of life, chivalrous assertion of all that he thought true, and extraordinary benevolence of feeling, were permitted a long and useful career, terminated only by natural decay, and followed by the love of many, and the respect of all.

It is well known to many of the Members of this Society that an enlarged and improved Catalogue of the Stars, arranged after the manner of the Catalogue of this Society, has been a long time in progress, under the auspices of the British Association. That work is now nearly completed, and ready for the press, and will contain above 8000 stars. To each star will be annexed not only the annual precession, but also the secular variation of such precession, and the proper motion when it can be ascertained. The usual constants for determining the apparent positions of the stars at any required epoch, will also be given. This work

cannot fail of being a valuable addition to the resources of the astronomer.

The Members may be interested in learning that the Standard Scale of this Society has been reported to Her Majesty's Government, as one of the best means of regaining an accurate determination of the Standard Yard that was destroyed in the conflagration of the two Houses of Parliament: and that an indirect overture has been made for the acquisition of it, should the Government eventually consider it desirable. The Council apprehend that the Members would readily accede to any arrangement in this respect, which would promote the object that the Government has in view, and at the same time not be injurious to the interests of the Society.

The British Association having appointed a Committee to consider the propriety of revising and re-arranging the constellations in the heavens, Sir John Herschel has drawn up an interesting paper on this subject, which has been read before the Society, and printed in the forthcoming volume of the *Memoirs*. As it was considered desirable that an early and extensive circulation of his views on this subject should take place, the Council ordered an additional number of copies of this paper to be printed, which have been generally distributed, with a view of drawing the attention of astronomers to this branch of the science. Sir John's revision has been confined to the southern hemisphere, where the greatest confusion prevails in the nomenclature of the stars and in the distribution of the constellations: and if the reform, which is here suggested in the south, should meet the approbation of astronomers, it may become a matter of consideration, whether the principle may not be extended into the northern hemisphere, which has been sadly confused by modern innovations.

Since the last Anniversary, Her Majesty's Government has put the Society in possession of two rooms on the basement story of the present building; which have been cleaned out and appropriated for the erection of any apparatus that may be required for pendulum experiments, or for prosecuting any other investigations that may be carried on in such apartments.

It had long been a subject of regret that the immense magazine of facts contained in the Annals of the Royal Observatory from the time of Bradley's appointment, downwards, till a very recent epoch, should remain in a great degree unavailable for astronomical use. Our illustrious associate Bessel, in his *Fundamenta Astronomiæ*, corrections to the solar tables, and finally by his *Tabulæ Regiomontanæ*, rendered this vast labyrinth permeable, and extracted and exhibited in a finished shape much of its valuable contents. Some years ago, the British Association proposed to the Government the reduction of all the Greenwich *planetary* observations under the gratuitous superintendence and responsibility of the present Astronomer Royal, and at his own suggestion. That work is now completed, and it is understood that the funds required for printing the results will be furnished by the Board of Admiralty.

The planetary places are compared with the best existing tables, and the difference in heliocentric longitude and latitude given exactly as in the recent volumes of the *Greenwich Observations*, with a term which takes into account the errors of the solar tables, should any sensible errors be therein found. It need not be said to the members of this meeting that every care has been taken, by duplicate computations and frequent comparisons, to attain all practicable accuracy. The geometer who undertakes the revision of the theory of a planet will now have no labour which could be spared, and will be freed from every difficulty which is not inherent in the problem itself; so that we may feel tolerable confidence a few years will see us in possession of tables very far indeed advanced towards perfection.

But this work, laborious as it has been, yields in importance to that which has been subsequently undertaken by the Astronomer Royal (also gratuitously), the reduction of all the Greenwich observations of the moon, from Bradley downwards, together with a comparison of the observed places with those deduced from Plana's theory. Considerable progress has already been made. The R. A. and N. P. D. of the moon's bright limb, with the corresponding mean solar time, are computed; MSS. tables, consisting of an extension of Damoiseau's tables for 1824, modified by the introduction of Plana's co-efficients and new terms, are nearly ready. The skeleton forms are prepared, and some steps in the computations taken. The liberality of Her Majesty's Government has enabled the Astronomer Royal to employ fourteen calculators on the work, which is consequently advancing with all possible speed and economy. Let us hope that no pause will be made until a new set of lunar tables of home manufacture are produced, which shall define the place of our hitherto incorrigible satellite with the accuracy of the best observations, and sufficiently for the nicest purposes of geography. Your Council feel that you will heartily join with them in their respect for the talents, disinterested activity, and *official* piety of the Astronomer Royal, and in thanks to the Government for its discriminating and liberal patronage of our science.

The Council are glad to have it in their power to report to the meeting, that the difficulties which seemed to lie in the way of successful completion of the Cavendish experiment have been removed, by new precautions against the radiation of heat from the large balls. Though many experiments may, in the early investigations, have been apparently wasted, yet in reality much good must result from the new light thus thrown upon the details of the operation itself, and on the torsion-balance, which is the principal instrument employed. Considering the nature of the quantity required, the results begin to assume a degree of accordance with each other which promises a very accurate determination of that great element of the solar system, the mean density of the earth. The slight discrepancies which still remain, and which appear to shew that some-

thing depends on the substance employed, and more on unknown circumstances connected with the torsion-balance itself, are not such as to throw any reasonable doubt on the density obtained being true within less than a hundredth part of the whole. So much can safely be said at the present time; and it is not improbable that a still smaller limit of error may be substituted for the one just named. Mr. Baily's final report may be soon expected, and in the meantime some detail of the history of the experiment is actually in the hands of the Secretaries, and will shortly be read at an ordinary meeting of the Society. The work itself will form the fourteenth volume of the *Memoirs*, and a portion of the tables is already in the hands of the printer.

The Council have the satisfaction of announcing that the thirteenth volume of the *Memoirs* will be ready, perhaps, before the completion of the twelfth; Mr. Baily, having been lately engaged in reprinting, at his own expense, the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, and Hevelius, in the type and form of our *Memoirs*, has offered the whole to the Council, to form the volume in question. As might have been expected, these catalogues have undergone such a revision and comparison as will materially increase their utility, and make these integrant portions of the history of astronomy familiar to the observer of our own day, who now looks upon them as difficulties, and refers to them (if, indeed, he have so much as the means of doing so at all) as little as he can help. The outlay saved to the Society by the manner in which this volume comes to us, though deserving and obtaining our warm acknowledgments, is the least part of the benefit; nor could the Council have omitted one word of the preceding testimony, if the manuscript, being, as it is, such as would gladly have been received, had been presented in the usual manner.

The whole of the volume is printed, excepting the preface, of which a circumstance well known to the Society at large has delayed the execution. And here, though it may be unusual to refer to the incidents of private life, yet the Council are sure that this meeting would feel disappointed if some opportunity were not given to the members of the Society to congratulate each other, and Mr. Baily, upon his most welcome and providential escape from the consequences of one of those accidents to which the inhabitants of crowded cities are daily exposed: an accident which, as all present remember, almost removed all hope of recovery, and made it seem next to impossible that life, if spared, should have been again occupied in the promotion of knowledge, and least of all in active research. Seeing him once more among us, in perfect health of mind and body, and remembering how much more probable it lately appeared that we should now be commemorating his innumerable services to the Society than anticipating their continuance, the Council drop the subject with the expression of their earnest hope that a life preserved against all expectation may be preserved beyond all expectation, and that a distinguished career may yet

await one of the earliest and the most indefatigable friends of the Society.

In the Address of the President at the last Anniversary of the Society, honourable mention was made of Mr. Henderson's investigations relative to the presumed parallax of *α Centauri*. These investigations have been continued to the present time; and from some observations recently received by him from Mr. Maclear, at the Cape of Good Hope, he is confirmed in his opinion relative to this subject, and considers the parallax to be about 1″. The Council trust that they shall soon receive from Mr. Henderson a detailed memoir on this important subject, which will then be read at the ordinary meeting of the Society.

The Council regret that they have to announce the retirement of Lieut. Raper from the office of Secretary to this Society; an office which he has filled with the greatest zeal and attention, and which calls from this meeting the expression of their best thanks. Nothing but the love of science and the talents which he possesses could have induced him to take so active and important a duty, oftentimes at a sacrifice of private ease and convenience: but this remembrance is at once the source of our approbation and the cause of our regret.

The Council trust that the award of the medal to Professor Hansen will meet the approbation of the Society. The labours of M. Hansen are well known to those astronomers and mathematicians who have attended to, and cultivated, that branch of inquiry which more especially relates to those abstruse and intricate points of investigation that require the greatest exercise of mental exertion. The grounds on which this award has been made will be more fully explained in the Address of the President at the close of this Report.

Titles of Papers read before the Society, between February 1841 *and February* 1842.

1841.

Mar. 12. On a Reformation of the Constellations, and a Revision of the Nomenclature of the Stars. By Dr. Olbers. Translated and communicated by Sir J. F. W. Herschel, Bart.

Continuation of the Investigation for the Correction of the Elements of the Orbit of *Venus*. By J. Glaisher, Esq.

April 7. Occultations of Stars by the Moon, observed at Ashurst and Dulwich, in the year 1840. By R. Snow, Esq.

Observations on the Variability of the Star *α Cassiopeiæ*, during the years 1839 and 1840. By R. Snow, Esq.

Description of the Observatory erected at Starfield, Liverpool. By W. Lassell, Esq. Jun.

Observations on Bremicker's Comet, with its Apparent Places, as obtained, with the Equatoreal, at Mr.

1841.

Bishop's Observatory; and an Account of the Methods employed in deducing them. By the Rev. W. R. Dawes.

May 14. Description of a Dioptric Telescope, and a Micrometrical Lunette. By M. Chevalier. Translated by R. W. Rothman, Esq.

Observations on the Aurora Borealis. By R. Snow, Esq.

Mean Positions of the Stars mentioned in Mr. Baily's Address to Observers, determined at San Fernando in the years 1834 to 1838. By M. Montojo. Translated by Capt. Shirreff, R.N.

June 11. A New Catalogue of Moon-Culminating Stars, observed at South Kilworth. By the Rev. Dr. Pearson.

On the Advantages to be attained by a Revision and Re-arrangement of the Constellations, with especial reference to those of the Southern Hemisphere, and on the Principles upon which such Re-arrangement ought to be constructed. By Sir J. F. W. Herschel, Bart.

Nov. 12. On the Longitude of Dr. Lee's Observatory at Hartwell. By the Rev. George Fisher.

Observations of the Beginning and Termination of the Solar Eclipse of July, 1841, at Aberdeen. By Charles Crombie, Esq. Communicated by George Innes, Esq.

Observations of the Lunar Occultation of *Venus* on September 11, 1841, at Mr. Bishop's Observatory, in the Regent's Park. By the Rev. W. R. Dawes.

Notice of the Occultation of *Venus* on the 11th of September, 1841. Observed at Malta, by Capt. Basil Hall, R.N. Communicated by Capt. Beaufort, R.N.

Observations of Bremicker's Comet made with the Equatoreal Instrument of the Observatory of Padua. By M. Santini.

Introduction to a Catalogue of 1677 Stars included between the Equator and 10° of North Declination, observed at the Observatory of Padua. By M. Santini. Communicated by Sir J. F. W. Herschel, Bart.

Dec. 10. Observations of the Solar Eclipse of July 18, 1841. By the Rev. Professor Chevallier.

A new Method for greatly facilitating the Computation of the Moon's Co-ordinates. By S. M. Drach, Esq.

Thoughts on Shooting Stars and Comets, suggested by the perusal of Mr. Galloway's paper on the subject, read before the Society on January 8, 1841. By S. M. Drach, Esq.

1842.

Jan. 14. Observations of Halley's Comet, made at the Observatory of Geneva, in the years 1835 and 1836. By M. Müller, under the direction of M. Gautier, Director of the Observatory. Communicated by Sir J. F. W. Herschel, Bart.

1842.
Jan. 14. Note on the Masses of *Venus* and *Mercury*. By R. W. Rothman, Esq.

Observation of the Immersion of p^2 *Leonis* behind the Dark Limb of the Moon. By R. Snow, Esq.

Extract of a Letter from Professor Encke to Mr. Airy, dated 20th December, 1841. Translated from the German. Communicated by G. B. Airy, Esq.

Comparisons of the Planet *Venus* in Æ. and N. P. D. with the Star A. S. C. 423, made with the Equatoreal Instrument of the Observatory at Ashurst, on April 9, 1841. By R. Snow, Esq.

Reduction of Mr. Snow's Observations of *Venus* and the Star A. S. C. 423, with some Remarks upon the Employment of Equatoreals in Planetary Observations. By the Rev. Richard Sheepshanks.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

American Philosophical Society.
Society of Arts.
Royal Asiatic Society.
The Lords Commissioners of the Admiralty.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences, Brussels.
Cambridge Philosophical Society.
Editor of the Athenæum Journal.
Royal Society of Edinburgh.
L'Académie Royale des Sciences de l'Institut de France.
Royal Geographical Society.
Geological Society of London.
Royal Irish Academy.
Imperial Academy of Sciences at St. Petersburg.
Historical Society of Sciences.
Her Majesty's Government.
Meteorological Society.
Linnean Society of London.
The Editor of the Microscopic Journal.
L'Administration des Mines de Russie.
Royal Observatory of Brussels.
The Observatory at Milan.
The Observatory at Vienna.
The Observatory at Vilna.
Royal Society of London.

Professor Bessel.
Miss F. B. Burton.
F. G. Bunt, Esq.
Rev. J. Challis.
M. Dien.
Professor Encke.

G. Fischer, Esq.
M. Francoeur.
T. Forster, Esq.
W. Galbraith, Esq.
J. O. Halliwell, Esq.
J. Herrapath, Esq.
E. O. Kendall, Esq.
Dr. Lamont.
A. Parsey, Esq.
Rev. B. Powell.
Professor Quetelet.
Rev. R. Sheepshanks.
Professor Schumacher.
M. Simonoff.
Capt. W. H. Smyth.
Lieut. W. S. Stratford.
R. Taylor, Esq.
M. Wartman.

The President (the Right Honourable Lord Wrottesley) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—Since the great discovery of the law of gravitation, the means by which the astronomy of the solar system has been advanced to its present state of perfection are of two distinct kinds. The first consists in the collection of facts from observation,—or, it may be said, in the application of that complicated and refined system of operations whereby the practical astronomer is enabled not only to assign the exact positions which the several bodies belonging to the system occupy at the moment of observation, but also to determine the paths they describe in space, and the laws by which their motions are governed. The second is that which is employed by the geometer. Setting out from the law of gravitation as established by Newton, and borrowing only from observation the elements which are necessary for the institution of his calculus, his object is to deduce from theory alone the whole of the phenomena of the system, even to their minutest details, and, by a comparison of his results with observation, to determine the masses of the different bodies, the influences which they exercise on the motions of each other, and the amount by which the elements of their fluctuating orbits deviate from their average conditions;—to express in formulæ the state of the system and the position in space of every body belonging to it at any given instant in past or future duration; and, finally, to convert his formulæ into numerical tables, for the uses of navigation and the other important purposes to which astronomy is subservient.

It is for researches in this second department of our science, undoubtedly the most arduous and difficult of the two, that your Council have awarded the Society's Gold Medal for the present year to Professor Hansen, the Director of the Observatory at Seeberg, and, according to annual custom, the duty devolves on me of stating to you the grounds of their decision. The subject is not very susceptible of popular explanation: in fact, the especial services which Mr. Hansen has rendered to astronomy consist in the developement of new formulæ, and the exhibition of new artifices of calculation, in the remotest and most abstruse departments of mathematical analysis. Nevertheless, I trust I shall be able to convey such an idea of their nature and object as will enable you at least

to appreciate the motives which have influenced your Council in conferring on our illustrious Associate this testimony of the Society's approbation.

In proceeding to determine the motions of a celestial body urged by a central force, and disturbed by the action of other bodies, the accelerating forces in the direction of rectangular co-ordinates are expressed by three differential equations of the second order, which, as is well known, can only be integrated by approximation. To obtain approximate integrals, two methods have been principally followed. The first consists in deducing from the differential equations, expressions for the variations of the radius vector, longitude, and latitude of the disturbed body in a function of the disturbing force and its partial differentials; and in integrating these expressions, either by developing them in series which proceed according to the powers of the eccentricities and inclinations, or else by the method of parabolic quadratures. This is the most obvious method of determining the perturbations, and also the simplest when the approximations are only carried to terms of the order of the eccentricities and inclinations; but when a closer approximation becomes necessary, and terms of a higher order are required to be included, the expressions become complicated, and the method accordingly loses its advantages.

The other method of obtaining approximate results is known in analysis as the method of *variation of arbitrary constants*. This method, though undoubtedly entitled to be regarded as one of the most ingenious artifices of modern analysis, is suggested in a manner by the peculiar constitution of our solar system, in which the disturbing forces which act upon any body bear so small a proportion to the principal force which determines the general orbit, that the body may be regarded as moving always in an *ellipse*, but in an ellipse whose elements are in a state of continual though extremely slow change. In accordance with this idea, the origin of which may be referred to Newton himself, the accelerating forces which act on a celestial body are conceived to be divided into two parts, one of which renders integrable the differential equations between the co-ordinates and the time, and gives the elliptic orbit which the body would describe about a centre of force if there was no disturbance; while the arbitrary quantities introduced by this first integration are supposed to be rendered variable by the other part, and their variations determined by means of differential equations of the first order, whose integrals (usually obtained by successive approximation) give the elements of the true perturbed orbit, from which the radius vector, longitude, and latitude of the body at any given time are computed.

The first example of this method of computing the planetary perturbations was given by Euler in the *Berlin Memoirs* for 1749, where he obtains the differential equations of the first order of the inclination and longitude of the node by varying the arbitrary constants which express these two elements in the elliptic orbit. But

though Euler afterwards succeeded in finding expressions for the variations of some of the other elements, the complete development of the method, and its application not only to physical astronomy, but to the general theory of mechanics is due to Lagrange; and it forms the distinguishing feature, so far as dynamics are concerned, of the beautiful system of mathematical analysis which that illustrious geometer has bequeathed to science in the Mécanique Analytique.

The method of analysis which we are now considering, is attended with peculiar advantages when applied to the determination of the *secular* inequalities of the orbits, in the development of which the greatest triumphs have been achieved of which physical astronomy can boast since the discoveries of Newton. It was by this means that Lagrange demonstrated that the greater axes of the planetary orbits are affected by no inequalities independent of the configuration of the bodies, and consequently that amidst all the fluctuations of the system, the mean distances of the planets from the sun, and therefore also their mean motions, remain for ever and unchangeably the same. It was by the same means Laplace formed exact expressions for the secular variations of the eccentricities and inclinations, and thence proved that the changes of those elements must always be inconsiderable; that they do not increase indefinitely with the time, but after a longer or shorter period again resume their former values. These conclusions, which were confirmed by the subsequent and more complete analysis of Poisson, lead immediately to what may be regarded as the most remarkable triumph of modern science, namely, the stability of the solar system; for they shew that, however the motions and positions of the several planets and satellites may be deranged and disturbed by their mutual perturbations, the variations which take place in the magnitudes and forms and positions in space of the different orbits are not only periodic, but confined within narrow limits.

But although in the hands of these great masters of analysis the method of varying the elliptic elements led to the sublime discoveries now alluded to, it is not without defects, which become particularly sensible in the numerical computations. Among these is to be reckoned the length of the calculations which it renders necessary for two reasons; first, because the number of elements of an orbit being twice the number of the co-ordinates which determine the place of the body, the calculation of a much greater number of quantities is required than by the first-mentioned method; and, secondly, because when the perturbations of the elements have been computed, there still remains the labour of substituting the altered elements in the expressions of the co-ordinates derived from the elliptic motion, in order to obtain the disturbed co-ordinates and the place of the body in its actual orbit. The principal defect of the method, however, consists in this, that the coefficients of the different terms of the series which express the disturbed elliptic elements have in general much larger values than the corre-

sponding terms of the expressions of the disturbed co-ordinates which determine the position of the body, so that the series expressing the disturbed elements converge slowly, even when they correspond to small perturbations of co-ordinates. If we conceive, for example, a system of forces of short period to disturb the curvature of an orbit many times in a single revolution, it will be easy to see that in each of these periods the elements of the orbit may have been greatly altered, while the disturbance of co-ordinates (of the longitude and radius vector, for example) may have been trifling. But in order to obtain these small disturbances, it is necessary to pass through the perturbations of the elements, which, relatively, are very considerable, and of which the calculation is rendered laborious by reason of the slow convergence of the series; and this inconvenience exists not merely in the case of the perturbations of the first order with respect to the masses, but in a still greater degree in the case of those of the second and of the higher orders. For these reasons the calculation of the perturbations has hitherto been in some respects imperfect and unsatisfactory; the computer always finding himself obliged to omit a number of the smaller terms without having any assurance that the error resulting from the omission is insensible; or, as Mr. Hansen has remarked, rather from a sort of presentiment that the omitted terms have no appreciable influence, than from a mathematical demonstration of their influence being insensible.

It was with a view to remove these defects from the lunar and planetary theories that Mr. Hansen undertook the series of remarkable investigations which have appeared from time to time, during a considerable number of years (partly in Professor Schumacher's invaluable Repertory, the *Astronomische Nachrichten*, and partly in two separate publications,— one on the perturbations of *Jupiter* and *Saturn*, and the other on the lunar theory), for which the Council has now awarded the Society's medal. His method of expressing the perturbations is based on that of Lagrange; but the modifications which he has introduced are of an important kind, and lead, in fact, to an entirely new mode of conducting the numerical calculations; so that, if it cannot be said that he has furnished us with a new instrument wherewith to attack the difficulties of the problem, he is at least entitled to the merit of having taught us a new method of applying that of which we were already in possession.

On taking a general view of Hansen's method, the point which first presents itself as remarkable, and that indeed in which the novelty of his process essentially consists, is the original and highly ingenious artifice which he employs in order to arrive at the expressions for the perturbed co-ordinates,— namely, the longitude, radius vector, and latitude. In the usual method of proceeding, the arbitrary constants introduced by integration are determinate functions of the elliptic elements and time, and the perturbations of co-ordinates are obtained by supposing the elements to vary. Instead of the true time, Mr. Hansen introduces into the *functions*

an analogous, but indeterminate, quantity, and considers the elements as invariable. He then determines the variations which this quantity must undergo (in other words, he finds what alteration must be made in the *time*, in the place where it enters explicitly into the elliptic formulæ), in order that the elliptic formulæ, with altered time and invariable elements, may give the same value of the indeterminate functions as would be found by using the true time and variable elements. Suppose, for example, the function of elements and time to be the true longitude; then the problem, according to Mr. Hansen's method of viewing it, amounts to this:— To find the perturbations which must be applied to the mean longitude, in order that the true longitude deduced from it with the use of invariable elements, may be the true perturbed longitude.

It is evident that the use of invariable elements, and time altered so as to give the correct value for longitude, would not, with the elliptic formulæ, give a correct value of the radius vector; but this difficulty is surmounted in an extremely ingenious manner by the introduction of subsidiary terms which, being applied as corrections to the radius vector of the unaltered elliptic orbit (*i. e.* unaltered except in time), give its true perturbed value. By similar considerations an expression is found for the latitude in the disturbed orbit. It would be impossible, however, without the aid of algebraic symbols, to give an idea of the analytical processes employed for determining these subsidiary terms; and for the same reason I must content myself with a bare allusion to the still more remarkable artifice to which he has recourse in order to obtain an expression for the continuous variation of the perigee and node of the lunar orbit, for which, by reason of their rapid revolution, invariable elements will clearly not suffice, and a departure in some degree from the original principles becomes necessary.

These deviations from the usual methods lead to very important advantages in the calculation of the tables, for the series expressing the perturbations of co-ordinates are not only rendered more convergent, whereby a smaller number of terms is required to be computed, but the coefficients of the individual terms are obtained with a smaller amount of labour than was necessary in the methods hitherto employed.

It will be readily seen from what has now been said, that the general aim of Mr. Hansen's researches is the improvement of the methods of expressing the lunar and planetary perturbations, so as to render the numerical calculations more easy and more certain. There is, however, one advantage which Mr. Hansen states to belong to his method, of by far too important a kind to be passed over without particular notice. It is this:—In the series which express the values of the disturbed co-ordinates, every term exceeding a certain numerical value, assumed at pleasure, can be immediately recognised, so that all those terms which fall below the assumed value may be rejected from the first, with the certainty that their sum falls within a given limit. The certainty thus

acquired that every term having a sensible value is retained in the calculation, is an improvement in the theory on which it would be difficult to set too high a value; and in fact it removes the principal defect which has hitherto attended all the methods of approximation which have been proposed. Nor is this advantage obtained by any sacrifice of generality; for neither with respect to the eccentricity and inclination, nor powers of the mass, is any other restriction introduced than is inseparable from the nature of the problem.

Besides these principal advantages of more rapidly converging series, and certainty with respect to the value of the omitted terms, there are some minor advantages incidental to the new method, which, however, are still of great importance. Among these may be mentioned certain relations subsisting among the analytical expressions of the co-ordinates, pointed out by Mr. Hansen, from which equations of condition are deduced which not only facilitate the calculations but afford a ready means of verification.

The applications which Mr. Hansen has as yet made of his method are to the Inequalities of *Jupiter* and *Saturn*,* in a Memoir which obtained the prize of the Royal Academy of Sciences of Berlin; and, to the lunar theory, in a work recently published.† In the former Memoir the theory is worked out to a numerical result. The expressions for the differential values of the longitude, latitude, and radius vector, are integrated by the method of quadratures, and results obtained which agree with those derived from the ordinary methods of approximation founded on the smallness of the eccentricities and inclinations. The approximations are, indeed, only carried to terms of the second order inclusive, with respect to the masses; but, in the case of *Saturn*, all the terms of this order exceeding a certain numerical value are calculated. His theory of the lunar perturbations, which presents difficulties of a peculiar kind, is not so far advanced, and much is still wanting to render it complete even as a symbolical theory. But in a recent number of the *Nachrichten*‡ he has announced that the calculations, on which he has been for some time engaged, are now proceeding towards a conclusion; and he has given some results which shew that the new methods apply with as much advantage to the moon as to the planets.

Thus, gentlemen, I have endeavoured to place before you a sketch of Mr. Hansen's researches, which, brief and imperfect as it is, will enable you to understand their object, and appreciate their importance. Should it be thought that these investigations refer only to matters of detail, and that the results at which he has arrived include none of those grand discoveries which enlarge the boundaries of science, and give us, as it were, a new insight into the

* Untersuchung über die gegenseitigen Störungen des Jupiters und Saturns. Berlin, 1831.

† Fundamenta nova Investigationis Orbitæ veræ quam Luna perlustrat. Gothæ, 1838.

‡ No. 403.

constitution of the universe, let it be remembered that the progress already made in physical astronomy has narrowed the field to the present inquirer, and that, in proportion as science advances, its processes become more and more intricate. The problem of the universe, difficult as it is, is still a limited problem; and the successive steps in its solution may be assimilated to the terms of one of those converging series expressing the perturbations we have been speaking of, in which each succeeding term is not only smaller in value than the preceding, but also more difficult of calculation. It is with the smaller terms only that the theoretical astronomer has now to concern himself; but his labours, though necessarily attended with less brilliant results, are not on that account the less necessary or useful. On the contrary, no more valuable service remains to be rendered to Astronomy, in the present state of the science, than the improvement of the existing methods of computing the lunar and planetary perturbations. The labours of Mr. Hansen have been steadily, and perseveringly, and successfully directed to this end. Whether the new methods which he has so ingeniously developed will be found in all cases preferable to those with which we are already familiar, or whether they will ultimately be adopted by astronomers as affording the most convenient forms under which the conditions of the solar system can be expressed, is a question which your Council do not venture to decide, and on which, perhaps, it would at present be premature to form an opinion. But, with respect to the profound ingenuity and consummate analytical skill which he has brought to bear on the intricate subjects of his investigation, there can be but one voice. His researches, which have been of the most laborious and abstruse kind, have been directed to the highest and most important questions of astronomy; and the means by which he has sought to conquer the still remaining difficulties, present more of novelty and originality, and afford stronger hopes of removing the differences which still exist between the Tables and Observation, than any which have been employed since the variation of arbitrary constants was propounded by Lagrange. On the whole, having respect to the importance of the subject, the results which have already been obtained, and the promise afforded of future improvements, the Council doubt not that the Society, and astronomers in general, will ratify its decision.

The President then, addressing the Foreign Secretary, continued as follows:—

Mr. Rothman,—In transmitting this medal to Professor Hansen, assure him of the lively interest which this Society takes in the continuance of his important labours; and convey to him our warmest wishes for his health and happiness, that he may be enabled to complete those improvements in the most arduous departments of our science which he has so auspiciously commenced, and thereby acquire a still stronger title to the gratitude of astro-

nomers, and to a place among those who have most signally contributed to the developement of the theory of Newton.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: the Right Hon. Lord Wrottesley, M.A. F.R.S.—*Vice-Presidents:* Francis Baily, Esq., F.R.S.; Rev. George Fisher, M.A. F.R.S.; Sir John F. W. Herschel, Bart. K.H. M.A. F.R.S.; Rev. Richard Sheepshanks, M.A. F.R.S.—*Treasurer:* George Bishop, Esq.—*Secretaries:* Rev. Robert Main, M.A.; Richard W. Rothman, Esq., M.A.—*Foreign Secretary:* Thomas Galloway, Esq., M.A. F.R.S.—*Council:* George Biddell Airy, Esq., M.A. F.R.S., *Astronomer Royal;* Rev. W. Rutter Dawes; Augustus De Morgan, Esq.; Thomas Jones, Esq., F.R.S.; John Lee, Esq., L.L.D. F.R.S.; Major-General C. W. Pasley, R.E. F.R.S.; Lieut. Henry Raper, R.N.; Edward Riddle, Esq.; Lieut. William S. Stratford, R.N. F.R.S.; Charles B. Vignoles, Esq.

SPECIAL MEETING.

A Special Meeting was holden after the business of the Annual Meeting was concluded, agreeably to the Bye-laws (Sect. iv. § 9), to consider the following Resolution, which was proposed on the part of the Council, the President in the chair:—

That Thomas Stevens Davies, Esq., Andrew Ure, Esq. M.D., and William West, Esq., being resident in England and having treated with neglect the repeated applications made by the Council, agreeably to the 5th Section of the Bye-laws, for payment of the arrears due by them, and having suffered their names to be suspended in the Meeting Room as defaulters since June 11, 1841, be expelled from the Society.

It having been ascertained at the commencement that more than twenty-four Fellows were present, a ballot was taken upon each name mentioned in the resolution; at the close of which the President announced that the three Fellows in question were severally expelled from the Society.

Errata in the Monthly Notice for January 1842.

Page 130, line 15, insert *the Earth* after *Venus*.

— 132, line 18, *for* $(1,0)\,\frac{e_0}{e}\cos(\varpi_0-\varpi_1)$, *read* $[1,0]\,\frac{e_0}{e_1}\cos(\varpi_0-\varpi_1)$.

— 132, line 19, *for* $(1,2)\,\frac{e_2}{e_1}\cos(\varpi_2-\varpi_1)$, *read* $[1,2]\,\frac{e_2}{e_1}\cos(\varpi_2-\varpi_1)$.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *March* 11, 1842. No. 21.

Francis Baily, Esq. Vice-President, in the chair.

Alfred Wrigley, Esq B.A. Mathematical Master at the East India Military College, Addiscombe, and Mark Noble, Esq. Head Master of the School, Albion House, Worcester, were balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. On an Instrument adapted for observing Right Ascensions and Declinations of Stars independently of time, accompanied by Drawings made with the Camera Lucida by Captain Basil Hall, R.N. By M. Wettinger. Communicated, with a Letter of Description, to Sir J. F. W. Herschel, Bart., by Capt. Basil Hall, R.N.

The instrument contrived by M. Wettinger is so fully described in Captain Hall's letter, that an independent abstract of M. Wettinger's paper is unnecessary. The following is a copy of the letter, dated Malta, Dec. 6, 1841:—

"My dear Sir John,—I have had my attention lately called to an invention which appears to me so ingenious, and grounded upon such good principles, that I think a description of it may interest you, and perhaps be considered by you as worthy of being brought to the notice of the Astronomical Society. Of this, however, you are the best judge; and I shall, therefore, merely give you the means of examining the pretensions of the instrument. In this view I have made three sketches of the model with the camera lucida, and I have added to each the same letters of reference to the same parts. I transmit to you also the opinion of Carlini, of Milan, and of his colleagues, as to this instrument, which was submitted to their examination some time ago.

"I may begin by stating that the chief object of the instrument is to determine the difference of right ascension between any two stars, without the agency of time as an element, the equatoreal angular difference between them being measured directly, *in arc*, on an hour-circle, graduated in degrees and minutes for that purpose. It is true that *time* does enter as an element into the principle of the instrument, inasmuch as a certain part of the machinery is moved by clock-work, in the manner used in many equatoreals; but this agency is purely mechanical and subsidiary, and does not require that the absolute time should either be exactly known, or its march be exactly kept.

"The instrument is essentially an equatoreal arc, in its struc-

ture,— that is to say, its principal axis is directed to the pole, and it carries a telescope capable of being directed to any star which is above the horizon, (I should mention, in passing, that the clock-work machinery is not included in the model; and there may be observed some other mechanical omissions, it not having been thought worth while to encumber either the model or the description with more details than are necessary to an explanation of the principle and workings of the instrument).

"The principal or polar axis of the instrument is made hollow— in fact, is a telescope, having at its upper extremity a small reflector or speculum capable of being directed at any angle into the tube of this axis telescope. The object-glass of this telescope is fixed not at its extremity, but about half way between the upper end and the centre. At the centre there is placed another reflector, which stands at an angle of 45° with the length of the tube, to receive the image of a star formed by the object-glass from the rays reflected from the upper speculum. The side of the axis telescope is perforated, in order to allow the image of the star which is reflected from the central speculum to pass into the middle of another telescope, which, for distinction, may be called the *declination telescope*, as it is attached to, and carries with it, a declination circle. In the middle of this declination telescope there is fitted a very small reflector, at an angle of 45° to its length, on which the image of the star reflected from the central speculum is received and transmitted to the eye of the observer, in every position of the declination telescope.

"The further arrangements of the instrument will, perhaps, be more readily understood by describing the manner of using it, than by giving a detailed explanation of the parts.

"In commencing an observation, the upper speculum is directed to a standard star of the first or second magnitude, partly by moving it on its own axis of rotation, so as to direct the rays into the principal axis telescope, and partly by the equatoreal motion, either of the whole apparatus, or by the rotatory movement of the principal axis, by means of the declination telescope. This motion, I may mention by the way, of the principal or polar axis, may be made at pleasure, independently of a large frame-work attached to the instrument, which is moved by the clock-work. There is an hour-circle in the plane of the meridian, fixed to this outer frame-work, and another circle fixed to the lower extremity of the polar axis, which may be clamped or freed from that which belongs to the framework. The speculum, at the other extremity of the axis, is so contrived that it moves along with the frame-work.

"It will, therefore, be understood that, if the upper speculum be so directed towards a star, that the rays reflected from it pass down the polar axis telescope, they will be received and reflected, first, from the central speculum, and secondly, from the speculum in the declination telescope, to the eye, in whatever position the declination telescope may be. Now, if the hour-circles be clamped, so as to form one, and the frame-work be put into gear with the clock

work, the whole will move round at the rate observed by the heavens, and, consequently, the image of the star reflected from the upper speculum will continue in the centre of the field of the declination telescope, for any required length of time, and in every possible position of that telescope.

"Suppose, now, that the relative position of the equatoreal circle, fixed to the frame, and that carried by the polar axis, be carefully ascertained by reading off their graduated circumferences, by microscopes or otherwise, and that then the circle carried by the polar axis be unclamped, that axis will be left free to revolve and to carry with it the declination circle, and likewise the declination telescope, but without interfering with what may be called the *celestial movement* of the frame, or that of the upper speculum, which, by going along with, continues to reflect the rays from the star to which it was originally directed; and, consequently, to preserve the image of that star constantly in the centre of the field of the declination telescope. This declination telescope is now directed to any other given star whatsoever, the image of which, viewed directly, is to be brought into coincidence with that seen by reflection from the upper speculum. If now the equatoreal circles be clamped, and a second set of readings be made, it is obvious that the difference between the two will be the difference, in arc, of the right ascensions of the stars.

"When the observation commences, the declination telescope is directed to the standard star, as well as the upper speculum, so that the images, seen direct and by reflection, are made to coincide in the centre of the field of view of the declination telescope. The graduations of the declination circle are then read off, to be compared with those when the second observation is made, or that of the star whose place is to be determined. The difference of these readings will give the difference of the declinations of the two stars, in the same manner that the difference of the readings of the two concentric hour-circles (as they may be called) at the lower end of the polar axis, gives the difference of the right ascensions.

"If clock-work machinery be not in such perfect adjustment as to keep the standard star, first observed, correctly in the centre of the field of view, it may be brought to that point by a tangential movement of the frame-work, to be made by hand, at the moment of making the second observation, without in any respect vitiating the integrity of the observation, for this small movement does no more than compensate for any error in the going of the clock.

"As it may not be always convenient to move the whole frame which is attached to the clock-work, the upper speculum, at the upper end of the axis, is so fitted as to be capable of being turned round independently of the frame, to which it is fixed by a moderately stiff collar. This movement, which may be made roughly by hand, or more nicely by a tangent screw, enables the observer, without stopping the clock-work machinery, to direct the speculum to any given standard star; and I may observe that only those of the first and second magnitudes are named for this purpose by M. Wettinger, as he fears the light lost by the three successive re-

flections might render any less brilliant stars invisible. This, however, does not affect stars viewed through the declination telescope, which looks directly to its object, and is supposed to be capable of seeing small stars as readily as large ones.

"Observations for determining the differences of right ascension and declination, in arc, between a standard star and any other, both being above the horizon, may be repeated as often as required; and it does not appear how, supposing the machinery perfect, any error can enter into these determinations, except what arises from the false position in which refraction places celestial objects. In the determination of right ascensions by an instrument placed in the meridian, this source of error is avoided; but it remains in full force as to declinations. The question with respect to right ascensions, therefore, resolves itself chiefly (if I rightly understand M. Wettinger) into the fact of its being both easier and more exact to determine the difference of right ascension in arc, by a leisurely and direct observation of the angle formed by the two meridians in which the stars lie, than to *infer* that difference of arc by the uncertain agency of a clock, which is further vitiated, he thinks, by the uncertainty of marking the exact moments when the stars respectively pass the wires of the meridian instrument. To these sources of error he adds that of the ear in appreciating the beats of the clock.

"M. Wettinger is of opinion that, although only experience can determine the degree of accuracy with which such an instrument could give the desired results, very fair estimates may be formed by practical astronomers familiar with the difficulties and errors of the existing methods, of the probable advantages of his invention. Whether, for example, the effects of refraction on stars above a certain altitude, on their right ascensions and declinations, are not sufficiently well known to admit of such exact corrections being applied to the determinations made by his instrument, as would render their results more worthy of confidence than those made with the existing instruments. It being taken into account, also, that, while only one observation can be made in the day on all stars which are not circumpolar, and only two on some of those which never set, with an instrument fixed in the meridian, the number of observations which may be made with M. Wettinger's instrument is unlimited; and as these observations might be made at all altitudes from that when the stars passed the meridian to the moment of their rising or setting, many curious inferences might possibly be deduced from it on the subject of refraction, while the observations might be so arranged as to counteract the vitiating effects of refraction, and, by the combination, to give correct results.

"It would seem that this instrument would be very useful in determining the place of a comet by direct observation, instead of inferring it, as is usual, even with an equatoreal instrument. For this purpose any standard or other star sufficiently brilliant to bear the triple reflection may be used.

"It will be observed in Signor Carlini's report, that, a doubt having been expressed as to the possibility of applying the prin-

ciple of this instrument to the sun, M. Wettinger, in order to try the experiment, fixed the small reflector or speculum of his model to the great equatoreal at Milan, in such a way that, while *Sirius* was observed *directly* by the telescope, the image of the sun, duly darkened and submitted to one reflection, was observed in the same apparent direction; and both, as he informs me, with such perfect precision, that the star could be seen on the disk of the sun, or be brought in contact with the limb with the utmost certainty.

"It will be observed that Signor Carlini and his colleagues, in their report, advert to the multiplicity of parts and variety of movements in M. Wettinger's instrument, as contrasted with the fixed nature and simple operations of the large meridian instruments now in use. But still they appear to be disposed to look with a favourable eye to the capabilities of M. Wettinger's invention, and they seem anxious that one of sufficient dimensions should be made; but for this, in their opinion, there are no means in Italy, and they recommend Munich or Vienna. Why not London?

"M. Wettinger is of opinion that prisms of glass might probably be substituted with advantage in place of the reflectors.

"As I may probably have omitted some material points in this explanation, I have requested M. Wettinger to draw up a description of it in Italian, the only language which he speaks; and I have asked him to employ the same letters of reference which I have used, so that the same sketches may do for both.

"I ought to add, that M. Wettinger is one of the professors of the university established here, and that he has long been highly esteemed for his knowledge and ability, and he is a person well acquainted both with the principles and the practice of astronomy.

"Should you wish it, or should you think it would prove interesting to the Astronomical Society, to see the model which M. Wettinger has constructed, I have no doubt he would readily allow it to be sent to England; or should you wish any further information respecting it, you will do him a favour by writing to him at Malta. I shall not be here above a month longer, as I go on to Egypt with my family in January; but M. Wettinger being fixed to this spot, will always be available.—I remain, &c.

"BASIL HALL."

II. A Letter from Professor Henderson to the Secretary, dated Edinburgh, January 31, 1842, on the Determination of the Parallax of α *Centauri*, by recent Observations made by Mr. Maclear at the Cape of Good Hope.

"My dear Sir,—Within these few days, I have received from Mr. Maclear a series of observations of α^1 and α^2 *Centauri*, made with a view to ascertain the parallax; and I find that they confirm the existence of a parallax amounting to about one second. The observations are of the double altitudes of the stars made with the mural circles, and they extend from April 16, 1839, to August 12, 1840. Twenty-six observations of the double altitude of each star were made with the old circle between April 16 and June 16,

1839; and 108 observations of the double altitude of α^1, and 112 of α^2, were made with the new circle between August 4, 1839, and August 12, 1840. In each observation the star was observed both by direct vision and by reflection at the same transit. The results which I have obtained are as follow:—

"From the 272 observations made with both circles,

Parallax	= 0″·91.	Weight 147·93 observations.
Coefficient of Aberration	= 20·55.	—— 142·47 ——

"From the 220 observations made with the new circle,

Parallax	= 0″·92.	Weight 138·81 observations.
Coefficient of Aberration	= 20·53.	—— 127·97 ——

"The observations made with the old circle extend over too short a period to warrant any results being deduced from them alone for parallax and aberration which could be relied upon.

"On computing the observations of each star separately, I find for α^1:—

"From the 134 observations made with both circles,

Parallax	= 0″·86.	Weight 70·37 observations.
Coefficient of Aberration	= 20·61.	—— 70·02 ——

"From the 108 observations made with the new circle,

Parallax	= 0″·91.	Weight 65·83 observations.
Coefficient of Aberration	= 20·54.	—— 63·71 ——

"And for α^2:—

"From the 138 observations made with both circles,

Parallax	= 0″·96.	Weight 77·55 observations.
Coefficient of Aberration	= 20·48.	—— 72·44 ——

"From the 112 observations made with the new circle,

Parallax	= 0″·93.	Weight 72·99 observations.
Coefficient of Aberration	= 20·52.	—— 66·27 ——

"If the coefficient of Aberration be assumed = 20″·36, as in the Astronomical Society's Catalogue, then, from all the observations with both circles, parallax = 0″·98, the separate results for the two stars being 0″·95 and 1″·00; and, from all the observations with the new circle, parallax = 0″·99, the separate results being 0″·98 and 0″·99.

"I believe that the observations are still continued to be made at the Cape; and I will write to Mr. Maclear immediately, requesting him to send the additional observations.

"The two stars appear to be approaching each other, the difference of declination being in 1826 = 18″, in 1833 = 15″, and in 1840 = 11″. When all the observations are collected, an attempt may be made to determine the orbits, and thence the masses of the stars.

"I will as early as possible prepare a detailed memoir on the subject, and transmit it to the Admiralty for presentation to the Astronomical Society.—I am, &c. "T. Henderson."

III. Positions of 78 Fixed Stars contained in the A. S. C., represented by Mr. Baily as not determined with sufficient accuracy, deduced from Observations made with the Meridian Circle of the Observatory of Kremsmünster. By M. Köller, Director of the Observatory.

IV. Observations of Falling Stars made at Hereford on the night of Nov. 12, 1841. By Henry Lawson, Esq.

Three observers were employed in watching for these phenomena, from seven o'clock in the evening till half-past four o'clock of the following morning, each taking a distinct portion of the heavens. The whole number observed was 79, and the greatest number observed in any one hour was 20, between the hours of three and four in the morning. The result the author considers to be so far satisfactory, that it tends to confirm the fact of the appearance, at about this period, of a greater number of meteors than usual.

V. A List of Falling Stars observed Nov. 12, 1841, at St. Helena. By J. H. Lefroy, Esq. R.A. Director of the Magnetic Observatory at Longwood.

The whole number observed was 102, between the hours of eight in the evening and five of the following morning. The Greenwich mean solar time of the appearance of each is noted to the nearest second, and the place of its appearance as referred to the bright stars nearest it. The direction of the motion of each is also given, with remarks on its appearance, rapidity, and other circumstances connected with the phenomenon.

VI. Path of the Moon's Shadow over the Southern Part of France, the North of Italy, and part of Germany, during the Total Eclipse of the Sun on July 7, 1842 (July 8, *Civil Time*). By Lieut. W. S. Stratford, R.N.

During the total eclipse of the sun on July 7, 1842, the moon's shadow will pass over Spain, the south of France, the north of Italy, and part of Germany; and it may induce travellers and others in those countries to prepare for the observation of this important phenomenon, if the means of so doing be furnished.

The following table has, therefore, been computed to enable them to trace the path of the moon's shadow on a large scale, and with very considerable accuracy.

It contains for each minute, from $17^h\ 34^m\ 39^s$ to $17^h\ 46^m\ 39^s$, mean astronomical time at Greenwich, the geographical positions (the longitudes being reckoned from Greenwich) of points on the earth's surface, where the following phenomena occur.

1. Contact of the upper limbs of the moon and sun.
2. Contact of the centre of the moon and sun.
3. Contact of the lower limbs of the moon and sun.

Greenwich Mean Time.	Northern Line.		Central Line.		Southern Line.	
	Long.	N. Lat.	Long.	N. Lat.	Long.	N. Lat.
h m s 17 34 39	° ′ 3 44 W.	° ′ 40 57	° ′ 0 15 W.	° ′ 41 26	° ′ 2 3 E.	° ′ 41 29
35 39	0 5 W.	42 30	2 49 E.	42 39	4 36 E.	42 31
36 39	2 43 E.	43 37	5 5 E.	43 34	6 48 E.	43 22
37 39	5 2 E.	44 31	7 4 E.	44 22	8 46 E.	44 6
38 39	7 5 E.	45 18	8 59 E.	45 3	10 33 E.	44 45
39 39	8 58 E.	45 59	10 44 E.	45 42	12 12 E.	45 21
40 39	10 40 E.	46 36	12 22 E.	46 16	13 46 E.	45 53
41 39	12 17 E.	47 10	13 53 E.	46 47	15 14 E.	46 23
42 39	13 50 E.	47 41	15 21 E.	47 17	16 38 E.	46 51
43 39	15 16 E.	48 10	16 43 E.	47 43	18 0 E.	47 16
44 39	16 40 E.	48 36	18 3 E.	48 9	19 19 E.	47 40
45 39	18 2 E.	49 1	19 24 E.	48 32	20 35 E.	48 2
17 46 39	19 20 E.	49 25	20 38 E.	48 55	21 49 E.	48 23

The connexion of the several points in (1) will trace out the northern limiting line of total eclipse; those in (2) the central line; and those in (3) the southern limiting line of total eclipse.

To indicate some of the principal places over, or near to, which the shadow will pass, a small map, on Mercator's projection, is annexed; representing the table from 43° to 49° of north latitude, and from 1° to 21° of east longitude. The transverse curve-lines represent arcs of vertical circles; and for the same moment of time, inserted above them, their intersections with the north, central, and south lines respectively, represent the phenomena numbered 1, 2, 3, previously explained.

The central line will pass

North of Marseilles	° ′ 0 28	or	32·2	miles.
South of Turin	0 27	or	31·0	—
North of Genoa	0 38	or	43·7	—
South of Milan	0 20	or	23·0	—
North of Verona	0 24	or	27·6	—
South of Vienna	0 32	or	36·9	—
North of Buda	0 58	or	66·8	—

The beginning of the total eclipse at a place on the central line being the contact of the eastern limbs of the moon and sun, and the ending the contact of the western limbs, the interval representing the duration of the total eclipse at any point will be about $2^{m}\cdot4$.

Should the darkness be sufficiently intense, as has been sometimes the case during total eclipses of the sun, to render some of the planets and brighter stars visible, the planet *Mercury* may be looked for about 5° south of the sun and moon. The planet *Mars* about west by north, *Mars* being 15^{m} of right ascension to the west, and 1° 16′ of declination to the north of the sun and moon.

The planet *Venus* is below the horizon until the shadow has passed Vienna, and will scarcely be visible at the eastern limit of the map. *Jupiter* and *Saturn* are invisible, being below the horizon during the whole interval. The *Georgian* is $7^h 13^m$ of right ascension to the west, and 23° 53′ to the south of the sun and moon.

The sun and moon are in the constellation *Gemini*, and will have *Castor* and *Pollux* not far distant in a N.N.E. direction; *Ursa Major* to the northward and eastward; *Procyon* to the south and east; *Orion* to the south and west; *Taurus* to the west; and *Auriga* and *Perseus* to the north and west.

Those persons who wish for more detailed information regarding the circumstances and phenomena of solar eclipses, will be amply gratified by consulting *A Memoir relative to the Annular Eclipse of the Sun, which will happen on Sept.* 7, 1820, by Francis Baily (London, 1818); the works therein referred to, viz. *Phil. Trans.* vol. xxix. p. 245, vol. xl. p. 177, vol. xlv. p. 582, &c. &c.; and a paper by the same author, in the tenth volume of the *Memoirs of the Royal Astronomical Society*, "On a remarkable Phenomenon that occurs in Total and Annular Eclipses of the Sun."

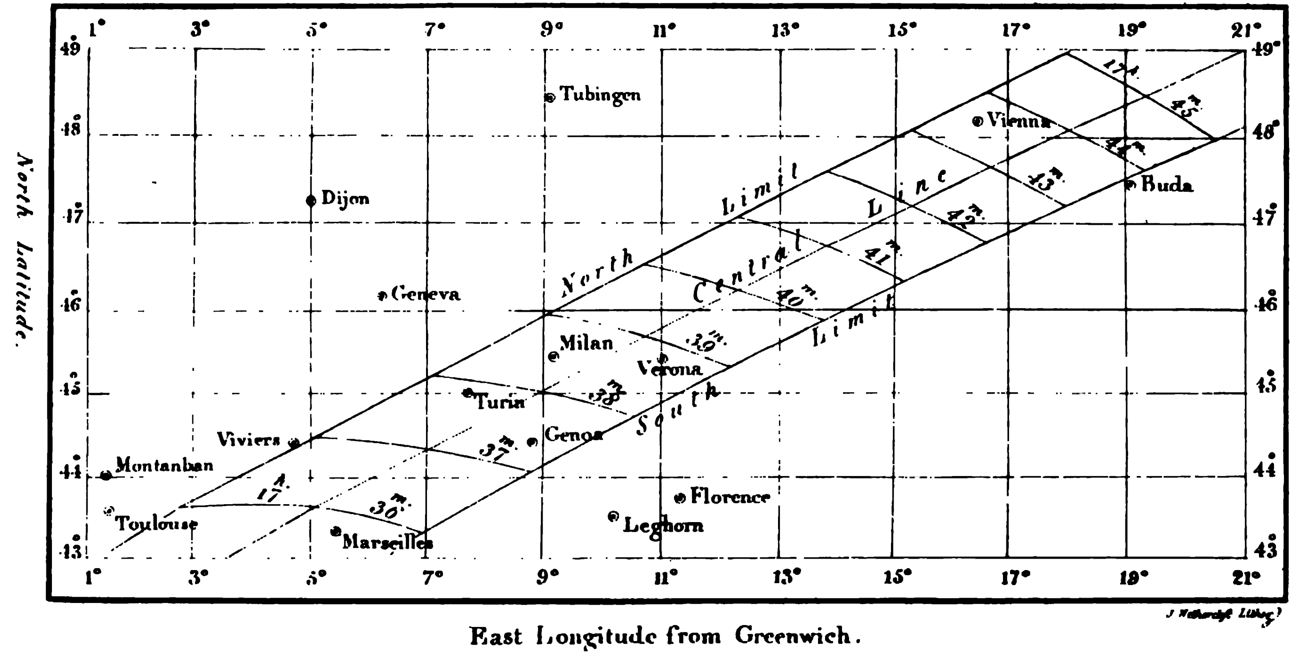
The memoir alluded to, though unfortunately not printed for sale, was circulated with the author's known liberality so widely, that there is little doubt of its being to be found on the shelves of the libraries of persons who feel interested in these matters.

Since the preceding matter was in type, a copy of Professor Silliman's *Journal of Science and Arts*, for January 1842, has arrived from America, containing an article "On the Solar Eclipse of July 8, 1842" (*civil reckoning*), from which the following is extracted, as meriting particular attention:

"As the approaching eclipse will excite great interest throughout Europe, and especially in those places where it will be total, it is earnestly hoped that particular attention will be paid by those favourably situated, and in possession of suitable instruments, to the determination of the correctness of a recent suggestion, that the irregularities so frequently noticed at the second and third contacts of nearly central eclipses, and at all the contacts of the transits of *Venus*, may be seen or not at the pleasure of the observer, according as the colour of the dark glass he applies to his telescope is red or green. These irregularities, as seen by many, have been minutely described by Francis Baily, Esq. of London, in an article in the tenth volume of the *Memoirs of the Astronomical Society*, although it particularly relates to the appearances, observed by himself, in the south part of Scotland, during the eclipse of May 15th, 1836, which was annular there. Many of the appearances described by Mr. Baily were seen through a *red* glass at the second and third contacts of the eclipse of Feb. 12th, 1831, which was annular in the south-eastern part of this State. Shortly afterwards, however, it having been ascertained that a double screen, composed of one light red and one light green glass, would not only render the light of the sun very

pleasant to the eye, but would far better define the limbs, and would sometimes even enable me to see a small spot, that was invisible through the dark red alone, a screen of that kind was adapted to the telescope, and was used for the partial eclipses of 1832 and 1836, and those that were central in 1834 and 1838. Through this screen no one of the irregularities described by Mr. Baily has ever been perceived, although carefully looked for. Indeed, so remarkable was the difference between the observed and expected appearances of the sun's limbs at the second and third contacts at Beaufort, S. C. on Nov. 30th, 1834, that even then a suspicion was excited that the entire absence of all distortion or irregularity in the cusps, just before and after the total obscuration, was to be attributed to the colour of the screen, especially since other observers in the vicinity of Beaufort saw through red screens many or most of the usual phenomena. This suspicion was strengthened by the observations on the large, but not central, eclipse of May 1836; it was, therefore, communicated to several of our astronomers, who paid particular attention to it, at the formation and rupture of the ring on Sept. 18th, 1838. In Philadelphia and its vicinity there were many observers, provided with telescopes of nearly equal optical capacity, but protected by screens of different colours. The result appears to be, that in every, or nearly every instance, in which the red glass was used, many or all of the usual irregularities were seen, whilst those observers who used yellow or green screens saw these appearances either greatly modified or not at all. At Princeton, near the northern boundary of the ring, two skilful astronomers, provided with 3½-feet telescopes by Dollond and Fraunhofer, were enabled distinctly to see some of these appearances through the red eyepiece of the former, though none was visible through the green screen of the latter instrument. At Washington, where the eclipse was nearly central, no distortion of the limb of the moon could be seen through the double screen above mentioned, and the cusps of the sun, just before and after the ring, were as pointed as needles. The Committee of the Philosophical Society of Philadelphia, in their report on this eclipse, say, 'This suggestion is one of great importance, as it seems to furnish evidence of the existence of a lunar atmosphere, through which, as through our own, the red rays have the greatest penetrative power. It also leads to new views concerning the cause of the remarkable appearances of the beads of light and the dark lines frequently noticed; since it shews that their appearance may be completely modified by a change in the colour, and, consequently, in the absorbing power of the screen glass through which they are observed.' It is believed that on another account will this suggestion, if well founded, be of great importance, viz. in its obvious tendency to diminish, if not wholly remove, the discordancies not unfrequently found in the best observations on solar eclipses and transits of *Venus*, and which, with regard to the latter in 1761 and 1769, were so great as

PATH OF THE MOON'S SHADOW OVER THE SOUTH OF FRANCE, NORTH OF ITALY, AND PART OF GERMANY, DURING THE TOTAL ECLIPSE OF THE SUN ON JULY 7. 1842.



materially to diminish the value of this method of determining the distance between the earth and the sun.

"Phases of the Eclipse at some of the principal Cities of Europe." The longitudes are reckoned from Greenwich, and the times indicate *mean civil times*, at each place respectively, on July 8, 1842.

	Latitude, North.	Longitude.	Beginning of Eclipse.		Ending of Eclipse.	
			Partial.	Total.	Total.	Partial.
	° ′	° ′	h m	h m s	h m s	h m
Brescia	45 32	10 13 E.	5 24	6 19 18	6 21 44	7 21
Genoa	44 24	8 54 E.	5 18	6 12 53	6 14 31	7 14
Gratz	47 4	15 27 E.	5 46	6 43 14	6 45 44	7 46
Lemberg	49 52	24 3 E.	6 25	7 24 33	7 27 24	8 33
Madrid	40 25	3 42 W.	bef. ⊙ rise	5 18 45	5 20 30	6 16
Marseilles......	43 18	5 22 E.	5 3	5 57 3	5 59 5	6 57
Milan	45 28	9 12 E.	5 20	6 15 4	6 17 18	7 17
Nice	43 42	7 17 E.	5 11	6 5 36	6 6 52	7 6
Padua	45 24	11 52 E.	5 30	6 26 28	6 27 56	7 29
Pavia	45 11	9 9 E.	5 20	6 14 28	6 16 52	7 17
Presburg	48 8	17 6 E.	5 54	6 51 44	6 54 14	7 57
Turin	45 4	7 42 E.	5 14	6 8 35	6 10 34	7 10
Venice	45 26	12 20 E.	5 32	6 28 49	6 29 33	7 31
Verona.........	45 26	10 59 E.	5 27	6 22 26	6 24 40	7 25
Vienna.........	48 13	16 23 E.	5 51	6 48 58	6 50 55	7 54

VII. A letter from Professor Hansen, dated March 1, 1842, in acknowledgment of the communication of the Foreign Secretary, announcing the award of the Society's Gold Medal at the last Annual General Meeting.

"Sir,—I have just now received your letter, by which you announce to me that the Royal Astronomical Society have honoured me with their Gold Medal. I recognise in it a valuable sign of the kind attention of this Society towards me and my labours; and I beg you to present to them my sincerest thanks.

"Pray have the goodness to allow the medal to be sent to M. Prætorius, Secretary and Librarian of His Royal Highness Prince Albert, who will undertake to send it to me.

"Of late my labours in the lunar theory have been considerably advanced. The calculation of the perturbations is finished, and I am now engaged on the calculation of provisional tables for the purpose of comparing my results with observations, and of determining the correction of the elliptic elements which result from them. I am now giving to these tables the necessary extension,

that they may afterwards serve as definite tables, after having applied to them the necessary corrections which are required by the new determination of the elliptic elements. To combine with exactness in these tables the most convenient mode of calculating the places of the moon, I have chosen the form that M. Carlini has given to the tables of the sun, as much as it is possible to do so. However, the labour of calculation of the tables themselves is much increased by this arrangement.

"Repeating my request that you will present my respects to the Royal Astronomical Society, I beg you will accept the sentiment of high consideration with which I have the honour to be, &c.

"P. A. Hansen."

The Chairman announced to the Meeting that Mr. T. Jones has signified his intention of immediately returning to the Society the *Beaufoy* Transit, which is at present in his possession.

London: Moyes and Barclay, Castle Street Leicester Square

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *April* 8, 1842. No. 22.

The President, Lord Wrottesley, in the Chair.

John Jenkins, Esq. of Wind Street, Swansea; and James Sweetman Eiffe, Esq. of South Crescent, Bedford Square, London, were balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. On the Aggregate Mass of the Binary Star 61 *Cygni*. By S. M. Drach, Esq.

The truth of universal gravitation having been confirmed by the elliptic form of the orbits of binary stars, it follows that knowing the absolute distances of the component members and their period of revolution round each other, we are able to deduce their aggregate mass compared with that of our sun and a planet, by exactly the same process which acquaints us with the various masses of the planets which are attended with satellites.

The ratio of the sums of the masses of the component bodies in two such systems being then that of the cubes of the mean distances of the components, multiplied into that of the inverse squares of their periods of revolution round each other, we may assume that one system is composed of the earth and sun, and we have then two cases to consider; 1st, when this binary star is of very small mass compared with the sun, in which case the system would revolve about the sun, the centre of gravity being near the sun's centre: and, 2dly, when the star's mass is much superior to that of the sun, in which case the orbital motion of the star would be only apparent, and owing to the real revolution of the solar system round it.

Applying these remarks to the case of the star 61 *Cygni*, and assuming Bessel's value of the parallax, and the usually assumed elements of the orbit of this binary system, it appears evident that this system is unconnected with the solar system. It does not, however, appear impossible that both systems revolve round a third at an immensely greater distance than that of the sun from the earth.

The author, in conclusion, adverts to the great importance, in the present advanced state of practical Astronomy, of noting the positions of the stars having the greatest proper motions with all possible accuracy, and of rigorously comparing the deduced proper motions at equal intervals of time, for the purpose of discovering whether the motions are performed in one plane, and whether they are uniform; and also to the importance of having a catalogue of stars accurately arranged in order of brilliancy by means of photometrical observations, as an essentially requisite element in the determination of their relative distances from the earth.

II. Second Note on the Mass of *Venus*. By R. W. Rothman, Esq.

In a Note on the Masses of *Mercury* and *Venus*, read at the Meeting of this Society on the 14th of January, I stated that a consideration of the motion of the perihelion of *Venus* had led me to conclude, that it was necessary to diminish the mass of *Mercury* by a quantity estimated approximately at $\frac{4}{10}$. This would make the mass in question $\frac{1}{3182843}$. I may observe in passing, that in the notice of the meeting of the 14th of January, page 132, there is a misprint in the algebraical formula for the motion of the perihelion; but this is merely a typographical error, and the calculations are correct. At the same meeting there was read an extract of a letter from Professor Encke to the Astronomer Royal, from which it appears that Professor Encke, guided by very different considerations has been led to fix the mass of *Mercury* in the first instance at $\frac{1}{3091947}$, and subsequently at $\frac{1}{4865751}$.

At the end of my Note, I stated that the secular equations affecting the orbit of *Mercury* appeared to confirm the necessity of an augmentation of the mass of *Venus*, to which I had been led by an examination of the secular motion of the node of the latter planet. But, in fact, this deserves somewhat further developement.

I have calculated the secular equation of the node of *Mercury* with the same planetary masses as those assumed in my first node, excepting that of *Mercury*, which I have supposed equal to $\frac{1}{3182843}$.

I have used the following values of the greater axes which are slightly different from those employed before:—

$$
\begin{aligned}
☿ &= 0·38709888\\
♀ &= 0·72333228\\
♁ &= 1·\\
♂ &= 1·52369210\\
♃ &= 5·20115524\\
♄ &= 9·53797320\\
♅ &= 19·18251740
\end{aligned}
$$

With these data I obtain for the annual sidereal motion of the node of *Mercury* :—

$$\frac{d\Omega_0}{dt} = -7''\!\cdot\!264 \quad -0''\!\cdot\!0621\,\mu_0 - 3''\!\cdot\!8665\,\mu_1 - 0''\!\cdot\!8915\,\mu_2$$
$$-0\cdot0991\,\mu_3 - 2\cdot2292\,\mu_4 - 0\cdot1129\,\mu_5 - 0\cdot0022\,\mu_6$$

If we assume Encke's second value of the mass of *Mercury*, namely $\frac{1}{4865751}$, and suppose μ_2, μ_3, μ_4, μ_5, μ_6 each $= 0$

then $\frac{d\Omega_0}{dt} = -7''\!\cdot\!242 - 3''\!\cdot\!867\,\mu_1$.

Now, according to Lindenau, the tropical motion of the node from 1631 to 1802 is 42″·534 annually; hence, with a precession of 50″·21, the annual sidereal motion is 7″·676

$$\therefore -0''\!\cdot\!434 \quad = -3''\!\cdot\!867\,\mu_1$$
$$\mu_1 = +0\cdot11$$

With the same data as before I have calculated the motion of the perihelion of *Mercury*, for which I find the following expression,

$$\frac{d\varpi_0}{dt} = +5''\!\cdot\!44335 \quad + 2''\!\cdot\!88796\,\mu_1 + 0''\!\cdot\!86028\,\mu_2$$
$$+ 0\cdot02881\,\mu_3 + 1\cdot59026\,\mu_4 + 0\cdot07604\,\mu_5$$

The mass of *Mercury* does not enter into this expression. The coefficient of μ_6 is insensible. Supposing now μ_2, μ_3, μ_4, μ_5 each $= 0$,

$$\frac{d\varpi_0}{dt} = +5''\!\cdot\!44335 + 2''\!\cdot\!8876\,\mu_1.$$

Now Lindenau gives for the tropical motion of the perihelion 56″·354; or, with a precession of 50″·21, an annual sidereal motion $= +6''\!\cdot\!144$.

$$\therefore 6''\!\cdot\!144 = 5''\!\cdot\!443 + 2''\!\cdot\!888\,\mu_1$$
$$\therefore \mu_1 = \frac{0\cdot701}{2\cdot888} = 0\cdot25.$$

The node of *Venus*, as given in my first note, furnishes us, assuming Encke's second mass of *Mercury*, and neglecting the terms which contain μ_2, μ_3, μ_4, μ_5, μ_6, with the equation

$$-1''\!\cdot\!60 = -5''\!\cdot\!174\,\mu_1$$
$$\therefore \mu_1 = +0\cdot31.$$

The three values of μ_1 are then

$$\mu_1 = +0''\!\cdot\!11$$
$$\mu_1 = +0\cdot25$$
$$\mu_1 = +0\cdot31$$

or, taking the mean $\mu_1 = 0''\!\cdot\!22$.

This, of course, is only given as an approximate estimation; but it seems difficult to resist the conclusion that the mass of *Venus* should be augmented by a quantity which cannot be put lower than one-tenth, and is probably considerably larger. An augmentation of one-tenth would make this mass $\frac{1}{365308}$, of two-tenths, $\frac{1}{334866}$.

III. On a Method of Determining the Latitude at Sea. By M. C. L. von Littrow, Adjoint-Astronomer at the Imperial Observatory at Vienna. Communicated by the Rev. W. Whewell, Master of Trinity College, Cambridge.

IV. On the Rectification of Equatoreals by Observations of Stars on the Meridian and at an Hour-Angle of Six Hours. By M. C. L. von Littrow. Communicated by the Rev. W. Whewell.

V. The Parallax of *α Centauri* deduced from Mr. Maclear's Observations at the Cape of Good Hope in the years 1839 and 1840. By Professor Henderson.

An abstract of the principal contents of this paper will be found in Professor Henderson's letter, contained in the last *Monthly Notice*, viz. that for March 1842. In addition, the author gives the following facts relating to the history of the observations of the star *α Centauri*. The earliest recorded observations which he has found are those of Richer, at Cayenne, in 1673, and of Halley, at St. Helena, in 1677; but neither of these astronomers mentions it as being double. Feuillée appears to have been the first person who observed it to be double, his observations being made at Conception, in Chili, in July 1709, with a telescope of 18-feet focal length. He estimates their magnitudes as being of the third and fourth, the smaller star being the more westerly, and their distance as equal to the apparent diameter of the smaller star (*Journal des Observations Physiques*, &c., par Louis Feuillée, tome i. p. 425; Paris, 1714).

La Condamine observed the star during the expedition to Peru for measuring an arc of the meridian (see *Philosophical Transactions for* 1749, p. 142). He estimated it as being of the first magnitude, and recognised its duplicity; and he remarked that the larger star was northward of the other, and to the east of it. From La Caille's observations in 1751–2, the distance of the two stars appears to have been 22″·5. Maskelyne observed them at St. Helena in 1761 (see *Philosophical Transactions for* 1764, p. 383), and estimated them as being of the second and fourth magnitudes. Their distance, as observed with a divided object-glass micrometer, he found to be from 15″ to 16″. From this time to the time of the institution of the Paramatta Observatory, the author has met with no observations of the distance of the stars. Mr. Dunlop, in the years 1825–6, found the distance to be 23″

(see *Memoirs of the Royal Astronomical Society*, vol. iii. p. 265), since which time it has been decreasing at the rate of more than half a second *per annum*. The angle of position scarcely appears to have changed since the time of La Caille; whence it may be inferred that the relative orbit is seen projected into a straight line, or a very eccentric ellipse; that an apparent maximum of distance was attained in the end of the last or the beginning of the present century; and that, about twenty years hence, the stars will probably be seen very near each other, or in apparent contact; but the data are at present insufficient to give even an approximation to the major axis of the orbit and time of revolution.

VI. Observations of the beginning and end of the Solar Eclipse of July 18, 1841. By Dr. Cruikshank. Communicated by G. Innes, Esq.

The eclipse was observed at Fyvie Castle, in latitude 57° 26′ 40″·7 north, and longitude $9^m\ 32^s{\cdot}6$ west, where there is a good clock by Hardy and a fine transit instrument. The magnifying power of the telescope used was about thirty.

	h	m	s		s
Time of the beginning of the eclipse...	2	15	4	; uncertain to	10
Time of the end	2	57	30	———	2.

The President announced that, at the next ordinary Meeting, it was the intention of the Astronomer Royal to give an account, illustrated by a model, of a zenith sector of peculiar construction, to be used in the Ordnance Survey.

London: Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

VOL. V. *May* 13, 1842. No. 23.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

The following communications were read:—

I. Extract of a Letter from Professor Encke to F. Baily, Esq., on the Periodical Comet of Encke.

"I have been very happy in the observations of the Comet of short period, which Mr. Airy, as he had the kindness to mention it to me, has likewise found and observed. Since February 8, my assistant Mr. Galle, and I, have observed it on thirteen evenings. The first five observations have been made by placing the comet in the centre of the dark field of the telescope (perhaps 18′ in diameter), reading the divisions of the instrument, and comparing these determinations with the determination of a fixed star made in the like manner. The next two evenings (March 5 and 7) the comet could be directly compared with a star by illuminated wires in the dark field. But the positions of these stars have not yet been determined. The last six evenings the compared stars were found in the *Histoire Céleste* and Bessel's *Zones*.

1842.	Mean Time, Berlin.	Observed R.A.	Observed Decl.	Diff. (Ephem.-Obs.)	
				R.A.	Decl.
	h m s	° ′ ″	° ′ ″	″	″
Feb. 8	7 30 4·3	356 40 58·0	+ 6 46 58·7	+ 17·1	+ 52·2
9	7 6 38·2	357 6 46·2	6 57 38·1	− 1·6	+ 16·5
11	7 5 37·4	357 59 23·4	7 18 53·0	+ 17·5	− 3·4
12	7 30 39·1	358 26 56·1	7 29 40·3	+ 16·1	+ 2·1
Mar. 3	7 45 55·6	8 25 46·5	11 21 45·3	− 16·3	− 10·7
11	7 27 34·1	13 36 34·2	13 12 31·8	− 12·2	− 3·5
20	7 47 33·2	20 23 49·8	15 18 55·0	− 18·8	− 9·9
23	7 34 6·9	22 53 24·8	15 57 43·3	− 15·0	− 11·9
24	7 44 7·9	23 45 32·0	16 9 46·3	− 24·6	+ 0·1
Apr. 6	7 49 6·8	35 4 6·6	16 57 16·4	+ 11·2	+ 30·9
7	8 2 36·9	35 45 26·4	16 43 10·3	+ 13·9	+ 45·4

"At the last observations the comet had a very distinct round figure, and was earlier found than the small stars in its neighbourhood. The first observations of the last evenings could be made without illuminating the wires, the twilight being clear enough. These last observations are according to the difference of the single observations as good as the observations of a fixed star.

"It will be very interesting to compare the observations made before the perihelion with those to be hoped after the perihelion. It is for the first time that both observations are made. If the circumstances should, in the next appearances of the comet, be equally favourable, it would be possible to learn something about the law of the resistance of the medium by comparing the two sets of observations with another. For the law which has hitherto been hypothetically adopted has the effect that the whole influence of the resistance is enclosed in the time of perhaps two months, before and after the passage through the perihelion.

"If you think proper to communicate these observations to the Royal Astronomical Society, I beg you to present to this Society my humble respect. — I remain, dear Sir, yours very faithfully,

"J. F. Encke,

"Dir. of the Berlin Observ."

II. A Letter from Professor Chevallier, on some Phenomena observable in Total and Annular Solar Eclipses.

"The extract from Professor Silliman's *Journal of Science and Arts*, for January 1842, contained in the *Monthly Notice* of the Astronomical Society for March, gives a most interesting account of the different appearance of the cusps formed at the second and third contacts of total and annular eclipses, as modified by differently coloured glasses.

"As the total eclipse in July, in the south of Europe, may give an opportunity of examining this curious phenomenon under various circumstances, I should be glad to direct the attention of observers to the appearance where the sun is not seen directly, but its image is formed upon a screen, by throwing the telescope slightly out of focus, in the usual way.

"If the colour of the glass has any remarkable effect, or if, as I suspect, the eye of the observer is liable to derangement in looking intently at a fine bright object, this method of examining the cusps is free from either of those sources of error. I observed in that manner the annular eclipse of the sun of May 15, 1836. The image upon the screen was perfectly well formed, so as to shew very distinctly the spots upon the sun's disc. I was not then aware of the bead-like appearance commonly noticed, so that my attention was not so closely drawn to the circumstance as it might have been. But the formation of the annulus and its termination were both carefully watched; and neither myself, nor any one of several other persons who were attentively marking the phenomena, perceived any such jagged appearance. The cusps came to a fine clear edge. I do not speak simply from general recollection;

since I had occasion, almost immediately after the eclipse, to compare the results of my own observation with those of others who had observed the eclipse through a coloured glass, and noticed the serrated appearance of the fine rim of light, and especially an apparent separation of one of the edges of the cusps, at the breaking up of the annulus.

"It occurred to me some time since, that this phenomenon might be studied at leisure, by creating an artificial eclipse of the sun. But it is only within the last few days that I could think of a practicable way of trying the experiment. In order to do so, I procured a finely turned disc, screwed upon a thin bar attached to the sides of a circular rim, which can itself be screwed into the tube of the eye-piece of a telescope, so that the disc is in the focus of the eye-glass, in the same manner as a ring-micrometer. The disc which I have used subtends an angle a very little greater than that subtended by the sun's diameter; and the apparatus is so contrived, that discs of different sizes can be employed. The figure represents the arrangement of the disc.

"By causing the sun to pass behind this disc, an excellent imitation of an eclipse is produced. I have examined the formation of the cusps with differently coloured glasses; and a combination of purple and green glasses, giving the sun the appearance of the moon, makes the cusps the most distinct. But I have not succeeded in obtaining with a red glass any appearance similar to the broken line of light which has been observed in solar eclipses. The disc, however, which I have employed is not very finely polished; and as the success of the experiment depends upon the accuracy with which the magnified disc is brought into comparison with the edge of the sun, a more careful set of experiments may be successful, if the appearance arises from any cause not originating in the moon.

"With reference to annular eclipses, I may be permitted to mention a singular circumstance which was noticed in the neighbourhood of Durham, during the continuance of the annular phase of the eclipse in May 1836. Some young persons coming down stairs, in a house in which a chequered light fell upon the floor through some trees, observed the stairs and floor to *be covered with little bright circles.* Of course, this must necessarily take place, since each spot of light through a small aperture forms an image of the sun. But as, on so rare an occasion as an annular eclipse, lasting for so few minutes, all eyes are usually directed to the skies, it may be doubted whether there is another instance on record of such an appearance ever having been actually observed.

"I am, &c.

"TEMPLE CHEVALLIER."

III. An Account of some Experiments with the Torsion-rod, for determining the Mean Density of the Earth. By F. Baily, Esq.

This paper was in part read, and will be resumed at the next ordinary meeting.

Mr. Airy gave a verbal account, illustrated by a full-sized model, of a zenith sector, now in the course of construction by Messrs. Troughton and Simms, under his direction, for the use of the Trigonometrical Survey. The instrument itself he considered to deserve attention in no remarkable degree except as illustrating principles of construction, which, as speculative principles, possessed no particular novelty, but whose practical importance he had felt so strongly as to have determined, at the first opportunity, on departing from the modes of construction usual among English instrument-makers. The construction of this sector originated in a request of Col. Colby to Mr. Airy to furnish him with a sketch of an instrument by which absolute zenith-distances might be determined with great accuracy by a single night's observations. The necessity for securing the last condition has been frequently felt in the course of the survey (the sectors arranged on the received construction requiring observations with the instrument in reversed positions on successive nights; and the unsettled weather, especially of hilly countries, rendering this a source of most vexatious delay): and in the event of extending the determination of astronomical latitudes in the survey, it would be indispensable that means should be provided for obtaining zenith-distances in a single night. The construction of the instrument was actually begun when the fire at the Tower occurred, in which Ramsden's zenith sector was destroyed. There was now urgent necessity for the completion of the new instrument.

The first principle in the new instrument is the arrangement for obtaining in one night zenith-distances cleared of instrumental reductions. For this purpose it is necessary to have the means either of making observations equally inclined on opposite sides to a horizontal surface, or of making observations equally inclined on opposite sides (as regards the graduations of the instrument) to a vertical axis, or to an axis whose inclination to the vertical could be accurately obtained. The former method was practically introduced by Mr. Airy (at the suggestion of Mr. Sheepshanks, though Dr. Robinson had independently proposed the same plan) at the Cambridge Observatory, and is now continued at the Royal Observatory of Greenwich: the practical process is, to set the circle properly for the observation by reflexion in mercury, to clamp it firmly, and to read the microscopes, before the object enters the field of view: then the reflexion observation is completed in a very short time by bringing upon it the micrometer wire of the telescope; and there is ample time for making the direct observation in the usual way. The latter method had been adopted by Dr. Brinkley in the use of the Dublin Circle: it was

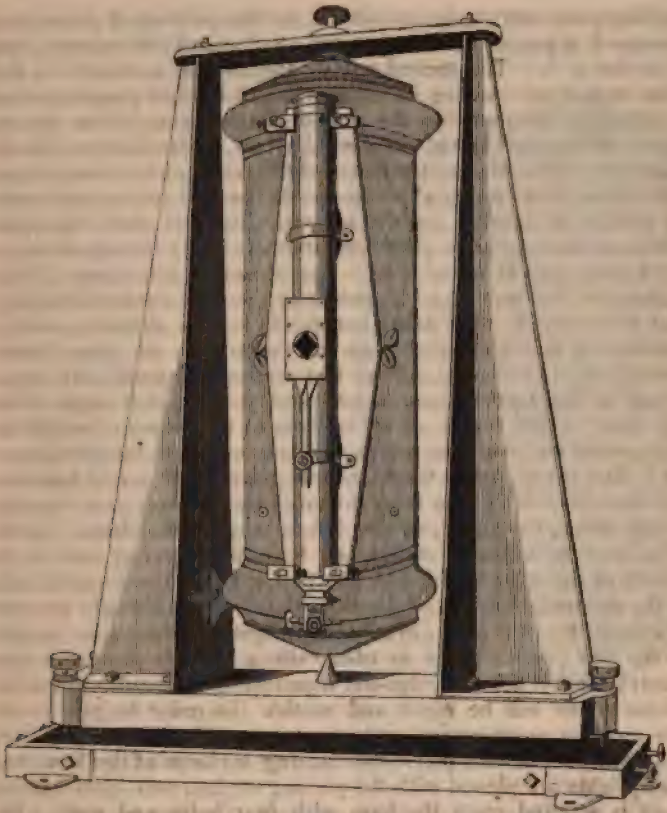
also applied (in a somewhat different manner) in the use of the repeating circle, which probably owed much of its value to this circumstance; and Mr. Airy himself had lately made some small modifications in the 25-feet zenith-tube of the Royal Observatory for the same purpose. The practical application of it consists in using every possible contrivance to make the first observation (before reversing the instrument round its vertical axis) as rapid as possible, as, for instance, by reading previously any levels, verniers, or plumb-line microscopes, by which the position of the telescope for the moment is determined, and then making the completion of the observation to depend only on the turning of a micrometer or other screw through a very small space (in the Greenwich zenith-tube an additional wire has been inserted, so that one wire is used for γ *Draconis* with the instrument in one position, and another for the same star with the instrument in the opposite position), and by making the reversion, by means of definite stops, &c., as rapid as possible.

The second principle in the new instrument is, the abandonment of plumb-lines (which have been used in all former zenith sectors) and the reliance upon levels in their stead. Mr. Airy pointed out as a very serious objection to the use of plumb-lines in all instruments which are reversed rapidly, that the bob of the plumb-line will not revolve in the same manner as the instrument, and therefore the lower part of the wire will be twisted relatively to the instrument. And as no wire is ever quite straight, an error is introduced by this twist (the wire not being observed in the same manner, with relation to the parts of the instrument, before and after reversion). In the zenith-tube of the Royal Observatory, to which allusion has been already made, this danger is removed by suspending the plumb-bob to a horizontal bar several inches long, and supporting the two ends of this bar on two plumb-lines, of which one is that which is observed with the microscopes; but this method is not wholly free from objection. In good levels, Mr. Airy expressed great confidence generally: and these it is evident are exposed to no particular error in rapid reversion.

The third principle is, to have each portion of the instrument cast in one piece, as far as possible, and not composed of a great number of pieces connected together by screws and stop-pins. Mr. Airy insisted strongly upon this, as the most important departure from the practice of London instrument-makers. The experience of the engineer-officers employed in our surveys, as well as that of observing astronomers, has shewn that the usual failure of instruments does not arise from want of accuracy of graduation (a few instances of extreme carelessness excepted), but from want of firmness of frame. In some instances (for example, in a part of the East Equatoreal at the Royal Observatory) a partial inaccuracy of division has been produced by the weakness of frame, which required extraordinary precautions to enable it to bear properly the pressure of the dividing apparatus. But the mere process of division, in Troughton's method, appears to be as nearly

perfect as it can be made by human powers. Mr. Airy, therefore, had not thought it necessary to attempt any new arrangement for increasing the accuracy of the divisions, or the accuracy of the methods of reading them (the micrometer-microscope scarcely admitting of improvement), but had given much attention to the subject of firmness. The usual plan of construction, he remarked, might be considered as one of *trussing*. This plan possesses the advantage of lightness: it is also strong for supporting a pressure always in one direction, but it can scarcely be trusted when the pressure is sometimes in one direction and sometimes in another. It was adopted by Mr. Airy in the large equatoreal frame of the 20-feet Northumberland telescope at the Cambridge Observatory; but even there it was thought necessary to have powerful screw-bolts, acting in various ways, so as to put it in the power of the astronomer to insure the firmest contact at every joint. Trussing also, in general, has respect to one plane only. For instruments which never change their position, or which are held by hold-fasts in the direction perpendicular to that plane, this principle may be exceedingly good; an excellent instance of this is in Graham's and Bird's mural quadrants, in which such trussing is properly applied, because they are held by proper bolts acting perpendicularly to the plane of the truss, and because the direction of the pressure is always the same. But it fails sometimes in its proper direction from weakness in the perpendicular direction An instance of this occurred several years ago in the failure of the trussed iron roof of the Brunswick Theatre. The truss was amply strong enough to bear a weight considerably greater than that under which it failed, had it been maintained firmly in its own plane; but the ruin occurred in consequence of the rafters first bending sideways. Mr. Airy then called attention to the construction of the circle called (improperly perhaps) a mural circle. He pointed out that it consisted of thirty-three pieces, entirely in one plane, fitted together with very great manual labour, yet weakly connected; and he inferred that it would be extremely weak without some other principle of strength. Upon examination it was obvious that the strength depended entirely upon the external rings, which were in one piece. The suggestion immediately followed,—Why not make the whole circle in one piece? So strongly had Mr. Airy felt this, that, in mounting the great Northumberland equatoreal, he had caused its hour-circle (five feet in diameter) to be cast in one piece; and Mr. Simms, who had the immediate charge of the further operations, division, &c., was fully satisfied with it. The same remarks as to weakness apply to the zenith sector.

Mr. Airy then proceeded to describe the instrument; a representation of which, in its observing state, is given in the following view:—



The instrument essentially consists of three portions:—1. The Stand; 2. The Frame, revolving in azimuth; 3. The Telescope-Frame. These were described in order.

1. The Stand.—In conformity with the spirit of Mr. Airy's remarks, the Stand ought, if possible, to have been made in one piece. This was found, however, to be very difficult. Partly for providing for the coarse adjustments necessary in the erection of an instrument, and partly for the sake of portability, it was made of the following parts. The lowest part of all is a large tray of cast-iron, with upright sides. This tray is intended to be fixed upon the pier which will be built up at each station, and is provided with several projecting ears in the plane of its base, pierced with holes, through which screws or bolts may be passed for the purpose of fixing it to such pier. Each of the sides of the tray has tapped in it two strong screws, in a horizontal position, which pass through the sides, and are intended to press the widened edges of

the flat piece next to be mentioned, for the purpose of giving it, if necessary, a part of a revolution (of small extent) in azimuth, and there fixing it. The second part is the flat piece, which is a thin plate of cast iron, having its edges widened in eight places (two on each edge), for receiving the action of the points of the screws, which are tapped in the sides of the tray. The width of the flat piece is less than that of the inside of the tray by about two inches, and its length is less by about one inch; and to this extent it can be turned in azimuth by the screws. The flat piece has three bosses on its lower side, and three others (exactly corresponding to the former) on its upper side. The bosses on the upper side are channelled for receiving the points of the foot-screws of the next part of the stand. The lower bosses are to give definite points of bearing on the horizontal plate of the tray. As these bosses correspond, the bearing of the foot-screws is immediately transmitted to the tray, without any dependence on the strength of the thin plate, which serves merely to connect the bosses on which the foot-screws rest with the widened edges on which the side-screws act. The third part is the Stand proper, consisting of a base and two uprights. The base is an inverted tray of cast iron, strongly ribbed below, the sides and ribs being three inches deep. At the sides, near to one end, it has two brass foot-screws; and at the middle of the other end it has one foot-screw. These screws rest in the channelled bosses of the thin plate. Each upright consists of a broad bearing piece; of a broad upright diminishing in breadth to the top (whose plane in observations will be east and west nearly); and of a broad edge-bar diminishing in breadth to the top (whose plane will be north and south). In order to give to the two uprights a connexion with the base nearly as firm as if they had been cast in one piece, the bearing surfaces of the base and the uprights are planed with the planing engine, and each bearing piece is screwed upon the base with four bolts and nuts. Mr. Airy here took occasion to observe, that he considered the tools of instrument-makers to be extremely defective in regard to their total want of a planing-engine; no other operation giving plane surfaces admitting of the same solidity of contact which is given by that machine: and no instrument having contributed so much, in the opinion of all engineers, to the perfection of modern workmanship, as the planing-engine. For completing the description of the Stand, it is only necessary to add that a cone fixed on the base gives the lower bearing for the Revolving-Frame: and that a cross bar at the top is fastened by nuts upon the two uprights (whose ends are cut as screws), and that a screw in the centre of the bar, with a conical point directed downwards, and a tightening nut, gives the upper bearing for the Revolving-Frame.

2. The Revolving Frame.—This part is of bell-metal, cast entirely in one piece. Its front is represented in the first figure, and its back (with its levels mounted) is shewn in the second figure. It consists of a hexagonal tray, with sides and ribs about

two inches deep. In the central part of its front (on that face shewn in the first figure, but concealed by the Telescope-Frame) is a ring of about eight inches diameter, turned on the lathe. This ring serves as the bearing by which the position of the Telescope-Frame is determined; the Telescope-Frame being drawn to contact by a spring-clutch. The parts at which the Revolving-Frame turns upon the two conical points, mentioned in the description of the Stand, are simply two conical holes, with cylinders of small diameter drilled in continuation of the vertices of the cones. In order to make the workmanship true, the centres of these holes ought to have been marked while the frame was on the lathe for turning the bearing ring; but, from some confusion in transmitting directions through two persons (no instrument-maker having any lathe except of very small dimensions, and the turning of this frame having consequently been intrusted to Messrs. Maudslay and Co., by whom the castings were made) it was forgotten. It is believed, however, that by a subsequent process they are correctly marked; this will be discovered in the use of the instrument. In the second figure are seen the three levels carried by the Revolving-Frame. These can be removed by slipping their ends (which have oblong holes) from the supporting pieces (which are cast with the Revolving-Frame). When in use, the levels are held firmly upon the supporting pieces by screws tapped through the lower sides of the oblong holes, which by pressing with their points the lower sides of the projecting pieces enforce the bearing of the levels upon the upper sides. It is necessary to make the levels moveable, because, in order to determine from observation the want of parallelism between the azimuthal-axis and the bearing-ring, it is necessary to have the power of reversing the Revolving-Frame. The revolution of the Frame in azimuth is checked by a stop attached to one of the uprights of the stand, to which the Frame approaches without much of jar by being compelled to rub along a spring. Near each end of the Revolving-Frame, on the face seen in the first figure, is a divided limb, viewed by the micrometer-microscopes of the Telescope-Frame: and near each end there is also a clamping-limb, for attaching the clamp of the Telescope-Frame; only one of these clamping-limbs (namely, that which happens to be lowest) is used at a time.

3. The Telescope-Frame.—This consists of a ribbed frame of considerable strength, widest in the middle (where its surface is turned to bear properly upon the bearing-ring of the Revolving

Frame), and having a pivot of large size which passes through a corresponding hole in the centre of the Revolving-Frame, and is drawn up by a spring-clutch to give the proper contact of the Telescope-Frame with the bearing ring. The telescope-tube is merely a protection from dust, and carries no essential part of the telescope. Near the object glass are two micrometer-microscopes, and near the eye-end are two opposite to the former. In considering what parts ought to be cast in one piece, it was laid down as absolutely necessary, that the screw for holding the object-glass-cell and its neighbouring microscopes should be in one cast; and that the eye-piece and its microscopes should be in one cast. It was then found easiest to cast the whole in one piece. The microscopes were bored. The following parts are therefore entirely in one cast; the screw for the object-glass-cell, the screws for the object-glasses of the four microscopes, the bearing-parts for the micrometer-screws of the four microscopes, and the bearing-part for the micrometer-screw of the eyepiece. The field of view is to be illuminated by a lamp at a distance; the same lamp throws light upon two reflectors on each side of the Telescope-Frame, which reflect the light to the microscope-reflectors.

General Remarks, and Method of Observing.—It will be perceived, from this description, that very little power of adjustment is left to the observer. The telescope may not describe a great circle; the plane of that circle may not be parallel to the azimuthal axis, the microscopes may have great error of runs, the levels may be not centrally adjusted; but for these errors the observer has no remedy, except in numerical calculation. Mr. Airy stated that he considered this to be a most important principle; that firmness was thereby insured; and that, whether there was or was not a power of adjustment, every instruction for observation, and every skeleton form for calculation, ought to be drawn out on the supposition that there may be, and usually will be, such errors. It is only necessary in the construction of the instrument that means should be provided for measuring the amount of the various errors; and in the instrument in question sufficient means existed, either by the observations of transits in different positions of the revolving frame (both as regards up and down, and as regards face east and face west), or by ordinary instrumental observations. The only adjustment to which these remarks do not apply is that for focal length; and this must be left to the instrument-maker. A small error in this point does not much affect the accuracy of observations which are confined to stars.

The consideration which determined the magnitude of the instrument was this. It was thought that it would be convenient to choose such a length of telescope, that a person of ordinary stature might be able, sitting upon a bench, to observe at the eye-end with a reflecting eyepiece; and that he might be able, standing upon the same bench, to read the upper micrometer-microscopes. It was found that the length of forty-six inches satisfied this condition pretty well; and, as Mr. Simms had by him several good object-

glasses of this focal length, one was selected, after trial, by Mr. Airy, and the instrument was adapted to it.

In describing the method of observing, it is to be premised that, though the divisions of the graduated limbs are read as if those limbs were interrupted parts of a complete circle, in which the degrees are read in continued sequence from 0 (at the beginning of one limb) to 359 (which does not appear, being upon the imaginary completion of the circle), yet for convenience a coarse graduation was provided on each end of the revolving frame, upon the clamping limb, giving zenith-distance both ways from the zenith, and used in conjunction with a small indicating mark carried by the telescope-frame. A small setting-piece was also provided, sliding on the opposite side of the clamping limb, and having upon it a strong mark; the use of this is, to be previously set to a zenith distance, in order that the observer may in an instant bring the telescope-frame to its proper position without the trouble of reading the divisions.

The method of observing then is this. There must be two observers, one on each side of the instrument. No. 1 (who is supposed to be on the same side as the Telescope-Frame at commencing) must set the telescope as nearly as possible for the star which is to be observed, and must also fix the small setting-piece for the same zenith distance on the opposite side; he must then read the four microscopes. No. 2, at the same time, must read the three levels. As soon as the star has entered the field, and has arrived at that part at which it is thought proper to observe it (before reaching the centre of the field), No. 1 must bisect it with the micrometer wire; and it will be prudent, though not necessary, for him to read the micrometer. He must immediately turn the Revolving-Frame in azimuth: No. 2 must then loosen the clamp and bring the indicating mark of the Telescope-Frame to the mark on the small setting-piece, when he will find the star in the field of view: he must then bisect it, either by means of the tangent-screw, or by the micrometer (if it has been read); then he must read the four microscopes, and No. 1 in the meantime must read the levels. This completes the double observation. The method of reduction will be, for each position of the instrument, to add together the mean of microscopes, the correction for runs, the mean of equivalents for the three levels, and the equivalent for the micrometer reading (the whole of these ought to be so arranged that there will never be negative quantities); the mean between these two sums for the opposite positions will be the quantity corresponding to a zenithal observation; and it will be prudent to deduce this separately from each star observed in the same evening, and to compare all. The difference between the adopted zenith-point and the sum for each observation, is the true zenith-distance from that observation. The whole of the preceding process requires no fixed wire in the field of view; and it is thought best that there should be no fixed wire (except wires parallel to the meridian, to indicate the part of the field in which the star may be observed).

Mr. Airy then stated that he had no improvement to suggest in the construction of the instrument, except that it might, perhaps, have been a little wider at the base (in the east and west direction), and that the whole might have been made of cast iron. He had been induced to use bell-metal on the belief that, with sufficient soundness, it might be cast thinner than cast iron; but he now had reason to think that he had been misinformed. He stated his intention, if he should again superintend the construction of an important instrument, of constructing it entirely of cast iron, unless some inconvenience at present unknown to him should present itself.

Mr. Airy concluded with some remarks on the tools and methods of instrument-makers, which, though well adapted to work on the small scale, are, in his opinion, inefficient for the construction of large instruments. He instanced particularly the want of powerful lathes with sliding rests, of planing machines, and of fixed drill-frames: and he expressed his opinion that the method of cutting micrometer-screws by dies held in the hand, adopted by instrument-makers, is inferior to the method of cutting them by machinery without reference to manual skill, adopted in the cutting of larger screws in the establishments of modern engineers.

Erratum in last Monthly Notice (*April* 8, 1842).

Page 180, line 11 from the bottom, *for* node, *read* note.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *June* 10, 1842. No. 24.

The Right Honourable Lord Wrottesley, President, in the Chair.

The Rev. H. A. Plow, of Queen's College, Cambridge, was elected a Fellow of the Society.

The following paper, which was in part read at the last meeting, was concluded, viz.

An Account of some Experiments with the Torsion-rod, for Determining the Mean Density of the Earth. By Francis Baily, Esq. Vice-President of the Society.

The author commences his account with a short preliminary history of the subject, and a reference to the previous labours of Maskelyne and Cavendish. He considers the experiments of Maskelyne, on the attraction of the Schehallien mountain, by no means decisive of the question; and with respect to those of Cavendish, by means of the torsion-rod, he is of opinion that Cavendish's object in drawing up his Memoir was more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important inquiry, than of deducing a result, at that time, that should lay claim to the full confidence of the scientific world. For, Cavendish himself, (who made only 23 experiments), in allusion to this very point, expresses a doubt on the subject, and hints at some further experiments which he had in view, for clearing up some of the irregularities which he had met with. But, as no further account of any subsequent experiments is on record, and as no trace of any new light on this subject can be found amongst Cavendish's papers, the propriety and advantage of repeating the experiments, under new circumstances, and with all the improvements of modern artists, had consequently been frequently discussed amongst scientific persons: and in the year 1835 the Council of this Society appointed a Committee for the express purpose of considering the subject. No effective steps, however, were taken even by this body for carrying the measure into execution till the autumn of the year 1837, when Mr. Airy,

the Astronomer Royal (one of the Vice-Presidents of this Society) applied for, and obtained from his late Majesty's Government, a grant of £500, to defray the expenses of this object.

Mr. Baily having offered to undertake the laborious task of making the proposed experiments, and of computing the results, the whole arrangement of the plan, and the entire execution of the work, was placed at his disposal and under his control.

It is somewhat singular that, whilst this plan was in agitation in this country, a similar course of experiments had been actually undertaken and accomplished by M. Reich, Professor of Natural Philosophy in the Academy of Mines, at Freyberg in Saxony; an account of which was read before the German Scientific Association, which met at Prague in September 1837; and an abstract of the results was printed in the *Monthly Notices* of this Society, for December following. Though the experiments are, on the whole, in good accordance with the general result obtained by Cavendish, yet they do not interfere with the plan that this Society had in contemplation: which was not merely to repeat the original experiments of Cavendish in a somewhat similar manner, but also to extend the investigation by varying the magnitude and substance of the attracted balls—by trying the effect of different modes of suspension—by adopting considerable difference of temperature—and by other variations that might be suggested during the progress of the inquiry. Reich made use of *one* mass only, and that much inferior in weight to the *two* adopted by Cavendish. The weight of Reich's large ball was little more than 99 pounds avoirdupois: whilst the two spheres, used by Cavendish, weighed nearly 700 pounds. Reich's experiments also were (like Cavendish's) too few in number; 57 only having been made, from which 14 results have been deduced; the mean of which makes the Density of the Earth equal to 5·44, almost identical with that of Cavendish.

As a great portion of the apparatus, which had been ordered, was at this time actually completed, and the remainder of it in considerable progress, Mr. Baily resolved to proceed in the inquiry, notwithstanding this apparent confirmation of Cavendish's results. Various places were suggested, by different persons, as the most suitable and fit for performing experiments of this kind: but, after inspecting several situations that were proposed, and considering all the circumstances of the case, Mr. Baily at length decided to carry them on at his own house, which he considers to be not only the most convenient that he could have selected, but which he has since found to be as suitable and fit as any that could have been specially erected for the express purpose. This house stands detached from any other building, in a large garden, some distance from the street, and consists of one story only.

The author then proceeds to give a description of the room in which the experiments were made, and likewise of the apparatus that was constructed for this special purpose. Although the apparatus was in a general view similar to that of Cavendish, yet in some respects it was essentially different. The great balls (or

masses, as they are called) were suspended from the ceiling by Cavendish and Reich: but Mr. Baily supported them, from the floor, on a plank which turned on a pivot; and suspended the small balls from the ceiling: thus reversing the mode of operations. This method of moving the masses he considers to be a great improvement: for he says, "Nothing can exceed the ease, the steadi-" ness, and the facility with which these large bodies are moved: " and during the many thousands of times that they have been " turned backwards and forwards, I have never observed the least " deviation from the most perfect accuracy. At the final close of " all the experiments, the pivot turns as steadily, as freely, and as " accurately as at the commencement of the operations." The small balls were also by Cavendish and Reich, suspended by a fine wire from the ends of the torsion-rod; whereas Mr. Baily screwed them to the ends of the torsion-rod, of which they thus formed an integral and solid portion. The motion of the torsion-rod was observed by means of a reflected image of the scale, from a small mirror attached to the centre of the torsion-rod, in the manner proposed by Gauss in magnetical experiments, and adopted by Reich. Some other alterations were likewise made in the construction and arrangement of the apparatus, to which it is unnecessary to allude more minutely, on the present occasion.

Mr. Baily made use occasionally of several small balls, of different sizes, and formed of different substances, with a view of ascertaining whether the results would be affected by such a variation: these were platina, lead, zinc, glass, ivory, and *hollow* brass, varying from 1½ inch to 2½ inches in diameter. The mode of suspension was also diversified, with a similar view: iron, copper, brass, and silk were successively used, not only single, but also double, similar to the bifilar mode suggested by Gauss for certain magnetical experiments. The mean weight of *each* of the great balls (or masses) was 2,663,282 grains, or about 380½ pounds avoirdupois; as determined by the accurate weights and scales of the Bank of England. And the weight of each of the small balls varied from 1950 to 23,742 grains. The length of the suspension-line was 60 inches; and the length of the torsion-rod (between the centres of the two balls, affixed thereto) was nearly 80 inches. The torsion-rod was made of fine deal, of an uniform shape throughout its whole length, and weighed only about 2300 grains. Another torsion-rod was afterwards made, for some special experiments, the weight of which was nearly ten times as great: it consisted of a solid brass rod, and was occasionally used without any balls attached to the ends.

The torsion-rod and the suspension-lines were screened by a mahogany box, constructed exactly similar in form to that used by Cavendish, but supported from the ceiling, in a very firm manner; and unconnected with the floor or any other part of the surrounding apparatus. Every precaution was taken to secure the torsion-rod from the influence of any sudden or partial change of temperature; and also to insure the stability and firmness of the

support to which it was attached. The author's remarks on this subject are worthy of notice: for he says, "In order to satisfy "myself on this point, at the time of the original construction of "the apparatus, I made various attempts to create a sensible dis- "turbance in the motion of the torsion-rod, by causing the doors "to be frequently and violently slammed—by jumping heavily on "the floor of the room—and also *above* the ceiling—and in other "different ways, having a similar tendency: but, in no instance "could I observe the least effect upon the lateral motion of the "rod. I have also frequently tried the same experiment, when "different visitors were present, since the apparatus has been com- "pleted: and have moreover many times not only accidentally, "but also designedly, made a regular series of experiments for "determining the Density of the Earth, during the most violent "storms that I have ever witnessed, when the wind has been so "boisterous, and blowing in such gusts, that the house has been "shaken to its centre. But in no instance have I ever seen the "least disturbance in the lateral motion of the torsion-rod, nor any "difference produced in the results of the experiments. I have "thought it proper to make these remarks and thus to place them "on record, because some persons at first hazarded an opinion that "the place which I had selected might not be quite adapted for "experiments of so delicate a nature. But a moment's considera- "tion will convince a person conversant with the subject, that no "*dancing* motion of the suspension line (even if it did exist) would "tend to produce an irregular *lateral* or *angular* motion in the "torsion-rod; and this is the only anomalous motion we need "guard against.

"There is also another remarkable circumstance connected with "this subject, which I think it requisite likewise here to place on "record. When the torsion-rod has been in a state of repose, I "have frequently shaken the torsion-box, by rapidly moving the "ends backward and forward from side to side 50 or 60 times, and "even more: but I could never discover that this disturbance of "the box caused the least motion in the torsion-rod, which still "retained its stationary position. This experiment has been wit- "nessed at various times by several scientific persons. Yet, not- "withstanding this torpid state of the torsion-rod, if the slightest "change of temperature be applied near the *side* of the torsion-box, "or if either *side* near the balls be sprinkled with a little spirit of "wine, the torsion-rod is immediately put in motion and the rest- "ing point undergoes a rapid change."

Notwithstanding these favourable circumstances the author at first met with certain irregularities and discordances, which he found it difficult to remove; and which appear to have been experienced also by Cavendish and Reich,—caused, as it is presumed, by variations in the temperature of the room in which the experiments were carried on. Cavendish chose an out-house in his garden at Clapham Common; and, having constructed his apparatus *within* the building, he moved the masses by means of ropes passing through a hole

in the wall, and observed the torsion-rod, by means of a telescope fixed in an ante-room on the *outside*. The general temperature of the interior was therefore probably uniform during the time that he was occupied in any one set of experiments: but it is scarcely to be expected that a building of this kind, and in such a situation, would preserve the same uniform temperature for twenty-four successive hours; especially at the season which he selected for his operations. Reich pursued a similar plan, but under circumstances apparently more favourable; for he selected a dark cellar, where the temperature was not so likely to be disturbed: and, having closed up the door, he adopted Cavendish's plan of observing the motions of the torsion-rod, on the outside. But, even in a situation like this, we must not expect a constant uniformity of temperature for a long period. Neither of these authors, however, has given any information on this subject; both of them, however, met with anomalies for which they could not satisfactorily account: and, although Cavendish suspected the cause of some of those anomalies, yet he does not appear to have applied any remedy for the evil, in any of his subsequent experiments.

Mr. Baily remarks, that his first experiments were tolerably regular, although the results were generally greater than those obtained either by Cavendish or Reich; but that he soon observed discrepancies which convinced him that some disturbing force was in operation, which he had not yet contemplated, and which he could not discover. One of the most striking evidences of such anomaly was the remarkable circumstance that the arc of vibration, during one and the same experiment, would seldom decrease in the regular manner which it ought to pursue, if the torsion-rod were guided by an uniform influence; and moreover, that in fact it would frequently *increase*, contrary to all the known laws of bodies so circumstanced. Notwithstanding these interruptions, he not only considered it proper to continue the experiments, for some time, in the usual manner, in the hope that he might thereby eventually throw some light on the probable cause of the anomalies, and perhaps be enabled to apply a correction for the effect of their influence; but also was induced to institute several new courses of experiments, as circumstances and suggestions occurred, for the express purpose of elucidating the subject. The theories of electricity, magnetism, temperature, and currents of air—the influence of different modes of suspension by single and double wires and by double silk lines—the trial of balls composed of different substances and magnitudes—were successively and frequently appealed to, and various experiments made to discover their probable effect on the results. The mode of conducting the experiments was also varied in different ways, with a view of eliciting information on the point in question. Some of them were carried on like those of Cavendish, and others like those of Reich (for the methods of these two experimentalists were very different from each other), whilst many more were conducted on a plan essentially different from either of them. Heated balls, and powerful lamps, were occasion-

ally applied near the torsion-box, with a view to raise an artificial temperature, and thus create a powerful influence: and, on the other hand, masses of ice have been employed for a similar purpose. The manner likewise of putting the masses in motion was frequently diversified, under the hope of being enabled thereby to obtain a clue to the object of research. But, the author has considered it needless to proceed with a detail of these fruitless operations, which were carried on, without much interruption, for upwards of eighteen months, and amounted in number to nearly 1300 experiments. Many of these were of a mere speculative nature, with a view to discover the cause of the anomalies here alluded to: but a thousand of them, at least, were more specially made for the purpose of determining the Density of the Earth, and were eventually reduced. But the results, although in many cases very consistent amongst themselves, were upon the whole so discordant and unsatisfactory, that no confidence could be placed on the general result, as a correct value of the true object of inquiry. And, as he had predetermined not to select merely those experiments which might appear to be the most favourable specimens, or supporting any particular theory, and to keep out of view, and reject the rest, he consequently abandoned the *whole*.

During these investigations the author was frequently visited by several scientific persons who took a lively interest in the pursuit in which he was engaged, and who kindly offered him their opinion and advice on several occasions. But he remarks, that he was principally indebted to Professor Forbes of Edinburgh, for the most satisfactory removal of the principal anomalies that he had met with. This gentleman's intimate acquaintance with the theory of heat, and its various operations, effects, and influence, led him to agree with Cavendish in opinion that one source, at least, of the anomalies might arise from the *radiation of heat* from the masses, when they were brought up to the sides of the torsion-box: and that this might even still operate notwithstanding the interposition of the sides of the box, and the precautions already taken. As a remedy for this influence he suggested the propriety of having the masses *gilt*, and also of procuring a *gilt case*, as a cover to the torsion-box, for the purpose of preventing the effect of radiation, from whatever source it might arise. Acting upon this advice, Mr. Baily not only caused a gilt case to be made in the manner here proposed, but also caused the torsion-box itself to be previously covered, all over, with thick flannel. These and other alterations and improvements having been completed, the author resolved to commence a *new series* of experiments, that were likely to be thus made under more favourable auspices, for the correct determination of the mean Density of the Earth: and it appears that the results soon convinced him that the proper mode had been taken for the removal of the principal source of discordance. For, although, in some cases, slight discrepancies may still appear to exist, as might be expected in any inquiry that involves so delicate a system of operations; yet where the discordances are of greater

magnitude they seem to be confined to one class of experiments, and to depend principally on the nature and construction of the material of which the suspension-line or torsion-rod is composed, and do not materially affect the general result of the whole. In fact, Mr. Baily states that he has since met with very few experiments, made in the regular mode of proceeding, that are objectionable, or that need be rejected. Every experiment therefore that has been made, under this new arrangement of the apparatus (whether good, bad, or indifferent) has been recorded and preserved; and they are all given without any reserve whatever: it being left to the reader himself to reject or retain, at his pleasure, such as he may think fit.

After these introductory remarks, the author proceeds to the several modes of carrying on the regular system of operations which he had undertaken. With respect to the torsion-rod, he states that it is never at absolute rest, but is constantly in a state of vibration on its centre; and consequently when the end of it is viewed at a distance with the telescope, it appears to oscillate on each side of a mean point, called the *resting point*. For, even when it is apparently in a state of complete repose, minute vibrations are always perceptible with the telescope; and the times of performing such infinitesimal arcs correspond, in most cases, very nearly with the mean time of vibration that takes place when the torsion rod is in full action. Mr. Baily however observes, that this resting point is by no means permanent or stationary, and seldom remains in the same position for any length of time, even when the torsion-rod is not influenced by the approach of the masses. The extent and direction of its disturbance, as well as its rate of motion when so disturbed, are very variable, and seem to depend on causes which have not been sufficiently accounted for, but which may in some measure arise either from slight changes of temperature, or some latent alteration in the component parts of the suspension-line. These vibratory motions of the resting point (which must be carefully distinguished from the regular vibratory changes in the position of the torsion-rod itself, caused by the near approach of the masses) do not materially affect the mean results in a series of experiments: more especially if their march be regular. It is only when any sudden and considerable transition takes place, that a sensible and material error is likely to occur: but this seldom happens if due precaution has been taken to screen the torsion box effectually. Yet the author is still of opinion that discordances sometimes arise which cannot wholly be attributed to change of temperature, but to some other occult influence with which we are at present unacquainted. The regular march of the resting point of the torsion-rod is one of the most important objects of attention; since any considerable deviation therefrom is the source of great discordance, and therefore requires to be watched with care.

The torsion force comes next under consideration. Mr. Baily justly remarks that the torsion force of a wire is that elastic power

in the body, by means of which it is enabled to return to its original position, after being drawn aside by any external impulse. It varies with the substance, magnitude, and length of the wire; but it is generally considered to be constant for the same wire, whatever be the weight suspended thereto. This, however, must be taken within certain limits, since the time of vibration (which is one of the elements for determining the force of torsion) will frequently differ very considerably without any apparent or sensible alteration in the component parts of the apparatus. For, the author states that we frequently have, in the *same hour*, very considerable variations in the time of vibration, which evidently shew that the force of torsion has undergone some sensible change. But, this alteration in the torsion force does not appear to affect the results of the experiments, since we find that, when the time increases, the deviation is also increased in due proportion. The magnitude, therefore, of the force of torsion is not a necessary object of inquiry in these investigations.

The only two objects requiring close attention, for the purpose of obtaining results from any of the experiments, are the determination of the *mean resting point* of the torsion-rod, and the *time* of its vibration. Now, it fortunately happens that these two objects can, in all cases, be observed with the greatest ease and accuracy, however anomalous they may be; and they are never accompanied with any doubt or difficulty. There is however another subject that is required also to be accurately ascertained in every experiment; namely, the exact distance of the centre of the masses from the centre of the balls. This has been effected by means of plumb-lines, which abut against the masses, and the distances between which are measured, at every experiment, by means of a microscopical apparatus, carefully adjusted.

From the results of the several experiments that the author has made, it would appear that *single* wires, of different diameters, give slight differences in the results. But, he states that the most discordant results occur where the *double* suspension-lines are formed of silk; and he apprehends that these anomalies have arisen from the circumstance that *all* the fibres, of which the skein is composed, are not *equally* stretched by the different balls as they are successively attached to the torsion-rod: and that they are thus severally operated on by different forces, which consequently produces a discordancy in the results. These discordances, however, appear to be generally confined within certain limits.

The author then gives a detailed account of the various experiments that he has made, under the improved form of apparatus, which amount in the whole to 2153; and which were pursued and conducted in different ways, for the purpose of throwing some light on the slight discrepancies that, in spite of his care and caution, would occasionally intrude themselves. It would be impossible in an abstract, like this, to give a minute detail of the several modes that were adopted in carrying on these operations; and which must therefore be left unexplained till the work itself is pub-

lished. But the following short synoptical view will enable the reader to form an estimate of the general results obtained from the different balls, according to the manner in which they have been successively suspended. The seven different balls employed are arranged, in the first column, in the order of their weight; and the number of experiments made therewith, together with the mean resulting density therefrom, is classed in the three collateral columns, according as the suspension was formed of double silk lines, double metal wire, or single copper wire. The three detached series, at the bottom of the table, containing 149 experiments, will be presently explained.

Balls.	Double silk.		Double wire.		Single wire.	
	No.	Density.	No.	Density.	No.	Density.
2½-inch Lead......	148	5·60	130	5·62	57	5·58
2-inch Lead	218	5·65	145	5·66	162	5·59
1½-inch Platina...	89	5·66	...	...	86	5·56
2½-inch Brass.....	46	5·72	...	...	92	5·60
2-inch Zinc......	162	5·73	20	5·68	40	5·61
2-inch Glass.....	158	5·78	170	5·71	...	...
2-inch Ivory.....	99	5·82	162	5·70	20	5·79
2½-inch Lead, with Brass rod ...			44	5·62		
2-inch Lead, with Brass rod			49	5·68		
Brass rod, alone....................			56	5·97		

It cannot be supposed, amongst such a number of experiments, prosecuted in such a variety of ways, and with such different materials, that the several mean results, obtained from the individual classifications, can be of equal weight. In fact, the author himself has, in his investigations of the subject, clearly shown that some of them are entitled to more confidence than others: and moreover that, in a few instances, there may be a fair cause for rejection. On these points however there is no room for explanation in this place: and it may be sufficient here to state that, assuming every experiment to be of equal weight, the mean result of the whole 2004 experiments is 5·67. Nor is there much probability that the result of this immense number of experiments will be materially altered, even if those few experiments, which may appear to be affected with some source of error or discordance, should be wholly omitted.

The author remarks that it cannot escape observation that the general mean result, obtained from these experiments, is much greater (equal to $\frac{1}{43}$ part) than that deduced either by Cavendish or Reich, who both agreed in the very same quantity, namely, 5·44: but he does not assign any probable cause for this dis-

cordance. It is evident, however, from the detail which he has given of his own experiments, that perceptible differences not only arose according to the mode in which the torsion-rod was suspended, but also depended on the materials of which the suspension-lines were formed: but it is somewhat singular that none of the mean results, in any of these classifications, are so low as that obtained by the two experimentalists above mentioned.

In these remarks, no notice has yet been taken of the remaining 149 experiments that have been made with the brass torsion-rod; a class of experiments that were undertaken for the express purpose of ascertaining the effect of such a measure on the general result. This torsion-rod was nearly of the same weight as the two 2-inch lead balls, and about half the weight of the two $2\frac{1}{2}$-inch lead balls. The experiments were made not only with each of these balls, successively attached to the rod, but also with the rod alone, without any thing attached thereto. The results show that the attraction of the masses on the rod should be diminished about $\frac{1}{70}$ part, in order to render these three several results consistent with each other, and also accordant with the same balls and the same mode of suspension, attached to the lighter wooden torsion-rods.

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ROYAL ASTRONOMICAL SOCIETY.

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The President, Lord Wrottesley, in the chair.

Lieutenant Henry Charles Otter, R.N., and William F. Donkin, Esq. M.A. Savilian Professor of Astronomy at Oxford, were balloted for and duly elected Fellows of the Society.

The following communications were read:—

I. A few Remarks on the Total Eclipse of the Sun observed at Nice, July 8th, 1842. By Captain John Grover.

I went this morning an hour before sunrise to a position I had selected on a height between Nice and Villa Franca. I had an open horizon to the southward, bounded on the east by Monaco, and on the west by Antibes. Shortly before sunrise, the atmosphere became hazy. I could not see Jupiter, which had been shining brilliantly all night. I made use of a telescope with a 3-inch object-glass, made by Tulley out of some glass I procured from Thibaudeau et Bontemps. The sun appeared at $4^h\ 27^m$, bearing 9° 8′ to the northward of east.

I had suspended a thermometer to my telescope stand, three feet above the ground: this, at the commencement of the eclipse, marked 68° F.; at the greatest obscuration, it fell to 59°; and at the close, stood at 73°. Every one complained of the cold. The thermometer was constantly exposed to the sun.

I observed nothing particular at the first contact: the moon's edge was sharply defined throughout, and perfectly even. No remarkable diminution of light occurred until half the moon's disc was obscured: then the light diminished very gradually, until about only four minutes of the sun's disc were left; then the change was rapid, and it became as dark as it was half an hour before sunrise. Some persons told me they could not see the Château de Nice, a remarkable object on a hill distant less than a mile.

At the semi-obscuration, objects became tinged a *slate* colour, which became darker and darker until lost to view.

Just before the total obscuration, when about three minutes of the sun's disc remained visible, a most splendid sight presented itself; this crescent of light was suddenly changed to luminous

points, which appeared to wave from the extremities to the centre of the crescent; presenting precisely the appearance I have witnessed at an illumination when a *device in gas* has been swept over by a strong breeze.

The obscuration being complete, the upper edge of the two luminaries (about 40°) was blazing with flickering flames, having the appearance of spirits of wine burning. The rest of the circumference was surrounded by a band of light of about 4′, which did not flash or waver.

The total obscuration lasted $1^m\ 35^s$. When the sun's disc reappeared, it was perfectly true and well defined: no luminous points. All the appearances I have described above vanished the instant the sun's edge appeared.

I observed nothing on the moon's disc. I could not see Venus or any star owing to the haze.

Some persons inform me they saw some stars in the zenith.

I send this hasty account before taking any rest, and may, perhaps, have something further to communicate on my arrival in London.

II. Observation of the Time of the Termination of the Solar Eclipse of July 8, 1842. By Arthur Utting, Esq. Communicated by E. Riddle, Esq.

The observation was made at Yarmouth, in long. $6^m\ 58^s$, and lat. 52° 36′ 12″. The state of the atmosphere was very unfavourable, which rendered the time of the separation of the limbs of the sun and moon doubtful to the amount of 10^s. The telescope used was a 3-feet Gregorian, with a power 65°. No spots were seen on the sun's disc.

The mean solar time of the termination from the observation was $18^h\ 52^m\ 29^s$.

Barometer	$29^{in}\cdot749$
Att. Therm.	$61^{\circ}\cdot5$
Ext. Therm.	$58^{\circ}\cdot0$

III. Some Remarks on the Total Eclipse of the Sun, on July 8th, 1842. By Francis Baily, Esq. Vice-President of this Society.

It is well known to many members of this Society that I proposed to proceed to the Continent, during the last summer, for the express purpose of observing the total eclipse of the Sun which was to take place on the morning of July 8th, civil reckoning. This object has been accomplished; and I flatter myself that an account of that rare phenomenon, by an eye-witness, may be acceptable to this meeting. A statement of the principal observations that I made, was communicated by me, to one of the Vice-Presidents of this Society, in a letter written at Milan within 48 hours after the eclipse, whilst the circumstances were still fresh in my memory; and they do not differ from those that I am now about to relate more in detail, and which I am desirous here to place on record.

A total eclipse of the sun, in any particular portion of the

globe, is an event of very rare occurrence, since only four or five of these remarkable phenomena are recorded as having been seen in Europe during the last century: to which we may add another that was fortunately seen *at sea*, by Don Ulloa. But, the accounts of these several eclipses are by no means satisfactory, since they are discordant in many particulars; which probably has arisen not only from the sudden and unexpected appearances that occurred, but also from the loose description that has been given of them, either by the observers themselves, or by those who drew up the accounts, and perhaps did not fully comprehend the intention and meaning of the authors. The difficulty also is very much increased from the want of drawings to represent the exact appearances seen; which are always more readily understood by this method, than by any verbal description.

During the present century another eclipse of this kind has taken place in the United States of America, which was observed by Mr. Ferrer; and a minute account of the same, together with a drawing of its appearance, has been published in the sixth volume of the Transactions of the American Philosophical Society. These are the only cases of interest that are on record since the invention of the telescope, within which period we must necessarily limit our attempt to acquire any useful information relative to this remarkable phenomenon. But, I must proceed with my narrative.

My original intention was to have taken up my station, for observing the eclipse, at Digne, in the south of France; and I had proceeded on my way thither till I arrived near Lyons, when I found that I had a few days to spare: and, as I had proposed to visit Venice before my return home, I altered my route and resolved to proceed in an easterly direction, along the line of the moon's shadow, till the day before the eclipse, when I proposed to halt at the most convenient place that might offer. I therefore turned off towards Chambery, and crossing the Alps at Mount Cenis, passed through Turin, Asti, and Alessandria, and arrived at Pavia about noon on July 7th.

As this place was directly on the central line of the moon's shadow, I resolved at once to make it my head quarters. I had intended to apply to the director of the university there, for the use of a convenient place where I might observe the eclipse: but I was agreeably anticipated in this respect, by a visit from one of the Professors, who having heard of my arrival and my object, immediately and obligingly came to offer me the use of any one of the apartments in the university that might be considered most adapted for my purpose. On accompanying him to the university, with this object, I selected one of the upper rooms of the building, which was admirably adapted for making the observations that I had in view. He then very kindly expressed his readiness to furnish me with any instruments at the university, that I might require for my use. But, I had taken with me from London the same 3½-feet telescope by Dollond, that I had formerly used in the

annular eclipse of May 15, 1836, as already described in the tenth volume of the *Memoirs* of this Society: and I therefore informed him that all I wanted was to be left *alone* during the whole time of the eclipse, being fully persuaded that nothing is so injurious to the making of accurate observations, as the intrusion of unnecessary company. Acting upon this hint, he immediately took the key from the outside of the door, and placed it in the inside, and told me that I might lock myself in: but, there was no occasion for this precaution, for although I heard numerous footsteps pass the door, in their way to an adjoining apartment, which was also used as an observatory on this occasion, no one attempted to enter the room in which I was located.

At four o'clock in the morning of the eventful day I went to the university, in order to prepare for the observation: and at that early hour I found many of the students, and official persons, walking about. At sunrise a thin stratum of clouds was seen in the east near the horizon, but the sun soon got above this obstruction, and the remainder of the day was beautifully clear and serene: not a cloud was to be seen in any part of the heavens, visible from my window, during the whole time of the eclipse. It was as fine a day as that which I had fortunately witnessed in Scotland, at the annular eclipse of 1836.

I had a very good observation of the commencement, and the end of the eclipse; but I did not pay any great atttention to these secondary objects, and, as my chronometer was not adjusted to correct mean time, these obseivations can be of no use, except as indicating the duration of the eclipse, which, according to my reckoning, was $1^h\ 56^m\ 39^s{\cdot}6$ mean time.

As the moon advanced towards her central conjunction with the sun, I watched very carefully and with much anxiety, the approach of the border of the moon towards the still illuminated portion of the sun, which was now rapidly assuming a fine crescent shape, the precursor of total obscuration. I used a *red* coloured glass, in order to observe the phenomenon, notwithstanding the remarks and advice to the contrary by an American observer: and the power of the eye glass was about 40. When the total obscuration took place, the coloured glass was removed.

I at first looked out very narrowly for the *black lines* which were seen in the annular eclipse of 1836; as they would probably precede the *string of beads.* These lines however did not make their appearance; or, at least, they were not seen by me. But, the *beads* were distinctly visible; and on their first appearance I had noted down, on paper, the time of my chronometer, and was in the act of counting the seconds in order to ascertain the time of their duration, when I was astounded by a tremendous burst of applause from the streets below, and at the *same moment* was electrified at the sight of one of the most brilliant and splendid phenomena that can well be imagined. For, at that instant, the dark body of the moon was *suddenly* surrounded with a *corona,* or

kind of bright *glory*, similar in shape and relative magnitude to that which painters draw round the heads of saints, and which by the French is designated an *auréole*.

Pavia contains many thousand inhabitants, the major part of whom were at this early hour, walking about the streets and squares, or looking out of windows, in order to witness this long talked-of phenomenon: and when the total obscuration took place, which was *instantaneous*, there was an universal shout from every observer, which "made the welkin ring;" and, for the moment, withdrew my attention from the object with which I was immediately occupied. I had indeed anticipated the appearance of a luminous circle round the moon during the time of total obscurity: but I did not expect, from any of the accounts of preceding eclipses that I had read, to witness so magnificent an exhibition as that which took place. I had imagined (erroneously as it seems) that the *corona*, as to its brilliant or luminous appearance, would not be greater than that faint crepuscular light which sometimes takes place on a summer's evening, and that it would encircle the moon like a *ring*. I was therefore somewhat surprised and astonished at the splendid scene which now so suddenly burst upon my view. It rivetted my attention so effectually that I quite lost sight of the string of *beads*, which however were not completely closed when this phenomenon first appeared. I apprehend that only a few seconds of time (perhaps 3 or 4) were wanting to complete the perfect obscuration of the sun: but I cannot speak on this point with much certainty.

I had previously noted down some of the principal objects to which I was desirous of directing my attention during the time of total obscuration, and which seem to have given rise to much discussion on former occasions. These, as far as the *corona* is concerned, had reference principally to its colour, its lustre or paleness, its magnitude and extent, its state of motion or repose, and its encircling the sun or the moon as its centre: then, as to the moon, whether any holes were discernible, or any coruscations of light on the dark side: next, as to the amount of darkness in the atmosphere, the change of colour in surrounding objects, and some other points not requisite here to enumerate further. The time however for making accurate observations of this kind is always so short in total eclipses (in the present case being less than $2\frac{1}{2}$ minutes) that one individual can scarcely attend to all the objects that are requisite to be noticed; more especially if his attention is called away (as in this instance) by any new phenomenon which had not been previously observed, nor even anticipated. It is therefore desirable, in any future occurrences of this nature, that a *division of labour* should be made between 2 or 3 observers at the same place; each attending solely to the part which he has selected for his particular object.

The breadth of the *corona*, measured from the circumference of the moon, appeared to me to be nearly equal to half the moon's diameter. It had the appearance of brilliant rays. The light was

most dense (indeed, I may say quite dense) close to the border of the moon, and became gradually and uniformly more attenuate as its distance therefrom increased, assuming the form of diverging rays, in a rectilinear line, and at the extremity were more divided and of unequal length: so that in no part of the *corona* could I discover the regular and well defined shape of a *ring* at its *outer* margin. It appeared to me to have the sun for its centre, but I had no means of taking any accurate measures for determining this point. Its colour was quite white, not pearl colour, nor yellow, nor red; and the rays had a vivid and *flickering* appearance, somewhat like that which a gas-light illumination might be supposed to assume, if formed into a similar shape. I should think it not impossible to give a tolerable representation of this phenomenon by some artificial contrivance. I have seen something like it, in miniature, by the reflection of the sun's light from a piece of broken glass; and on a larger scale by viewing the sun through a grove of trees: but in both these cases it is necessary to obscure the central portion of the rays. The brilliancy of the *corona* was however quite as great as that which is produced by either of the methods here alluded to. I have annexed hereto a drawing of the *corona*, representing, as nearly as I can preserve in my recollection, the appearance of its shape and extent, and the ramification of the rays, at the time of the middle of the total obscuration. I had no time or opportunity for ascertaining the deviation of the moon from the central position of the *corona*, at any other point of its progress. (See the copper-plate accompanying this paper.)

Splendid and astonishing however as this remarkable phenomenon really was, and although it could not fail to call forth the admiration and applause of every beholder, yet I must confess that there was at the same time something in its singular and wonderful appearance that was appalling: and I can readily imagine that uncivilized nations may occasionally have become alarmed and terrified at such an object, more especially in times when the true cause of the occurrence may have been but faintly understood, and the phenomenon itself wholly unexpected.

But the most remarkable circumstance attending this phenomenon (at least, that which most engaged my observation during the short interval of total obscuration, and drew my attention from other objects of interest) was the appearance of *three large protuberances* apparently emanating from the circumference of the moon, but evidently forming a portion of the *corona*. They had the appearance of mountains, of a prodigious elevation: their colour was red, tinged with lilac or purple; perhaps the colour of the peach blossom would more nearly represent it. They somewhat resembled the snowy tops of the Alpine mountains, when coloured by the rising or setting sun. They resembled the Alpine mountains also in another respect, inasmuch as their light was perfectly steady, and had none of that flickering or sparkling motion so visible in other parts of the *corona*. All the three projections were of the same roseate cast of colour, and *very distinct* from the brilliant

Appearance of th

as seen at Paris, in

vivid white light that formed the *corona*: but they differed from each other in magnitude. I have endeavoured to represent the appearance of the shape, size, and position of these several protuberances, in the accompanying drawing; and have numbered them in the order in which they were first seen by me. My attention was drawn, first of all, to No. 1, which is situate considerably to the right of the vertical point in the circumference; and on looking round the moon, I observed the other two. The largest of them was No. 2, which appeared to be bifurcated, and the separation of the parts was discernible even to the base, so that they might be taken for two distinct projections, one overlaying the other. No. 3 was not quite so large as No. 1. The whole of these three protuberances were visible even to the last moment of total obscuration, at least, I never lost sight of them, when looking in that direction; and, when the first ray of light was admitted from the sun, they vanished with the *corona*, altogether, and day-light was *instantaneously* restored.

I should mention that this drawing represents the appearances as seen in a telescope that *inverts*: and it may be interesting to know that the moon made the first impression on the sun's disc very near the point No. 3, and left it very near the point No. 1. My attention was so constantly taken up by these remarkable and unexpected appearances, that I omitted to watch for the re-appearance of the *beads*, and therefore cannot add my testimony to the re-occurrence of that phenomenon.

The darkness, during the time of total obscuration, was not so great as I had anticipated. I had caused a lighted candle to be prepared, in order to be ready in case of need; but I eventually extinguished it; as I found I could read very small print, and note the time by my chronometer, without its assistance. Prior to the commencement of the eclipse I had observed a great number of swallows flying about; but towards the middle of the eclipse they had all vanished, and did not make their appearance again till a few minutes after the first ray of light emanated from the sun, when they were as active, and soon became as numerous, as ever.

During the time of total obscuration, I examined carefully with the telescope the body of the moon, but could not discern any bright spot that might be mistaken for a hole; nor could I discover any coruscations issuing from the dark side of the moon. These, however, were only momentary observations. I was told that several stars were seen, but I could not spare the time to look about for them myself: every moment was occupied with more important matter.

Having thus given a detail of all the principal circumstances that occurred, and precisely in the manner in which they presented themselves to my view, as far as my recollection (committed to paper, immediately after the event) will assist me, I had intended to have subjoined to this communication an account of the several phenomena that had been noted on former occasions of this kind, and to have compared the various descriptions with each other, in

order to see how far any differences that were observed, might be reconciled with present appearances. Or, in other words, to have presented a sort of historical view of the subject, somewhat similar to the plan which I adopted in my Memoir relative to the annular eclipse in 1836. But, I fear that I may already have encroached too much on the time of the meeting: and I am moreover of opinion that a review of this kind can be taken with greater advantage at a more advanced period of time, when we may be in possession also of the several observations that have been made on the present eclipse, at different places on the continent, and which might thus be introduced into the comparison. Should such a measure be thought desirable and useful to future observers, I may probably intrude again upon the time and attention of the Society.

IV. Observations of the Total Solar Eclipse of 1842, July 7 (July 8, civil reckoning). By G. B. Airy, Esq. Astronomer Royal.

In the past summer, I made a journey to Turin, principally for the purpose of observing the solar eclipse at a place where it would be total. My intention was rather to observe the nature and succession of the general phenomena of a physical character than to make any precise observations of absolute time or absolute measure, or to attempt to deduce corrections of the elements of the moon's motion. I carried with me a small telescope by Simms, mounted on a short tripod stand, of 1·9 inches clear aperture, and about 14 inches foeal length. For the use of this, I am indebted to the kindness of Mr. Simms. I had carefully tried it on the sun's disc, and had satisfied myself that it was abundantly competent for the observation of those phenomena of eclipses which have excited so much interest: it is, indeed, a very good telescope of its size. I had also a duplex pocket-watch.

Having crossed the Alps by the pass of the Little St. Bernard, I had the good fortune, at Cormayeur, to meet Professor Forbes. We made arrangements at once for journeying together to Turin, and for observing the eclipse in concert. We reached Turin late in the evening of July 5.

The next day was spent in the examination of the instruments and localities of the Observatory of Turin, under the auspices of M. Plana, and in the inspection of the hill and church of the Superga. M. Plana was extremely anxious that we should observe the eclipse at the Observatory, where every facility depending on an ample supply of instruments, and an accurate determination of time, could be afforded us. I had, however, long before fixed on the Superga as a station from which I should desire to see, if possible, the grand phenomena of a total eclipse as exhibited on a large tract of country. It may be proper here to mention that the Superga is the highest point of an insulated cluster of hills, perhaps 800 feet above the Po, and five miles from Turin. It is completely surrounded by the plain of Piedmont, and commands a most remarkable view: on the east over the plain of Lombardy; on the north-east, of Monte Rosa and the neighbouring high Alps; on the

Appearances of the Total Eclipse of the Sun 1842. July 8. (civil reckoning) as seen from the Superga near Turin

(The relative positions are those seen with the naked eye or with an erecting telescope.)

1.—Appearance of the Sun a short time before the totality.

2.—Appearance of the Sun and Moon one or two seconds of time before totality.

north, of the mountains of the Val d'Aosta (Mont Blanc itself is hidden by them); on the west, of Monte Viso and the Dauphiny Alps; and on the south and south-east, of the Maritime Alps and the Appennines; with the plain extending from the foot of the hill to the bases of these mountains in every direction. As the eclipse was to be total for the Superga itself, and for the western and southern mountains, and partial for the northern mountains, I had thought it possible that I might see in great perfection the different phenomena in the different parts of the view. The sequel will shew that in this respect I was disappointed, but that (by chance) a most important advantage was obtained by my adherence to this plan. Finally, it was arranged that Professor Forbes should make his observations at the Observatory, and that I should go to the Superga.

On the 7th, the observations to be made were fully discussed, and the series of observations intrusted to each person were drawn out in the form of written instructions. The observations bearing upon physical optics were principally consigned to Professor Forbes: those of astronomical character to myself. The unfortunate circumstances of weather, however, rendered the former impossible, and in some degree abridged the latter.

The morning of the 8th, at one or two o'clock (civil reckoning), was very dark and lowering: scarcely a star was visible. I proceeded, however, with a companion to the Superga, and reached it at a short time before five o'clock. Every facility for viewing the eclipse from any part of the church or convent of the Superga that I might select was offered me by the fathers of that establishment; and, had my object been simply to view the country during the eclipse, I should undoubtedly have stationed myself in the upper gallery of the dome of the Superga. But the platform in front of the portico offered far greater facilities for the placing of my telescope, and more of general convenience; and, by moving a few steps, I could at any time command the whole plain. I therefore adopted the platform as my station. I think it due to the courtesy of the Italians to remark that, though many persons were present, I did not receive the smallest interruption of any kind from any one. The sun was clear, and I saw the beginning of the eclipse very well.

The power which I used throughout the eclipse was about 27 (as I have since found by the well-known method of comparing a distant object seen in the telescope with a near one seen without it). This power was found very convenient. I had intended occasionally to use a higher power, but was prevented from doing so by the following circumstance. The dark glasses adapted to the higher powers were made a little darker than was necessary at Greenwich; not, however, so dark as to prevent the most delicate observation. But, in consequence of the cloudiness of the day of the eclipse, the sun was, for the most part, so faint, that he could scarcely be seen when these higher powers, with their corresponding dark glasses, were employed; and as the glass for the lowest power was necessarily still darker, it was useless to attempt to com-

bine the eye-pieces for the higher powers with the dark glass for the lowest power. I was therefore compelled to lay aside the higher powers. It is certain, however, that the power which I used was sufficient for the nicest observations which the state of the air permitted, as it shewed very well the atmospheric undulations on the limbs of the sun and moon; and nothing smaller, of course, could be seen with certainty.

The dark glass which was used on the power actually employed was a combination of a purple and a green glass. It gave to the sun's disc a faint yellow-greenish tinge.

As the eclipse advanced (the sun continuing unclouded), I observed a circumstance which I have remarked in every solar eclipse that I have seen. It is, that the limb of the moon is very much more sharply defined than the limb of the sun. This is clearly owing to the difference of intensity of light on different parts of the sun's disc, the intensity near the centre of the disc being much greater than that near the limb, and the degradation very near the limb though rapid, being gradual. I speak of this as a fact of which I have not the smallest doubt; having long ago observed it in my daily practice as an observer with the transit instrument and the circle; and having also frequently remarked it as an experimenter when I have thrown the image of a portion of the sun's disc upon a small screen, in which case I have always been able to determine whether the limb was approaching to the edge of the fully illuminated screen, simply by the change of the intensity of illumination. And I allude specially to this fact at present, first, because in contemplating the probable phenomena of the eclipse it had been a particular subject of conversation between Professor Forbes and myself; secondly, because it may perhaps assist to explain a very strange observation which I shall shortly have to mention.

I had carried with me a wax-taper in a lantern, which I lighted about the time of the commencement of the eclipse. Whenever I looked round over the country, I also looked at the flame of the taper. I cannot, however, say that I remarked any peculiarity in the colour either of terrestrial objects or of the candle-flame. The flame, as the eclipse advanced, appeared much brighter (and must have been visible to a great distance at the totality), and its colour seemed somewhat redder: but I believe that this change takes place in just the same degree when the general light is diminished from any other cause. The surrounding objects did not receive the greenish hue which I have remarked in other eclipses of nine or ten digits: I know not whether this was due to the cloudiness of the day, or to the continued correction of my eye by reference to the light of the candle.

I saw no spots whatever on the sun's disc.

About six minutes before the totality I specially recorded the remark that there was a slight undulation on the limbs, but that the cusps were perfectly sharp. I cite this particular observation, I find it noticed in pencil at the time; but I am quite

certain that the cusps were seen perfectly sharp at every other time. The general aspect of objects was now very gloomy.

As the totality approached, the gloom increased very rapidly. About two minutes (or perhaps more) before the totality, my companion exclaimed that it was darker towards the Val d' Aosta. I immediately looked in that direction, and am satisfied that it was not darker there. The country, however, looked blacker on that side, partly, I think, because it is more encumbered with wood, and partly (perhaps) because the mountains are nearer than on the south side, and therefore have less of the whitish atmospheric tinge.

At this time, and to the totality, the appearances were very awful. The gloom increased every moment; the candle seemed to blaze with unnatural brilliancy; a large cloud over our heads, whose appearance I had not particularly remarked, but which, I think, was of cumulo-stratus character, became converted into a black nimbus, blacker, if possible, than pitch, and seemed to be descending rapidly; its aspect became horribly menacing, and I could almost imagine that it appeared animated. Of all the appearances of the eclipse, there is none which has dwelt more powerfully upon my imagination than the sight of that terrible cloud. The sun was very little clouded; his narrow crescent form could be seen with the naked eye when the eyelids were partially closed; there was, however, a dark cloud immediately above him, and fainter clouds about him. Immediately before applying my eye to the telescope to view the completion of the obscuration, I imagined that the light which the sun cast upon the ground was of a reddish colour. But the light was so very faint that I cannot at all vouch for this observation.

I have now to mention a very strange observation. I was viewing the sun most carefully with the dark glass upon the eye-piece, while the small illuminated ring was closing rapidly; my watch was lying on the parapet on which the short telescope-stand was placed, and I was counting its beats, with the intention of observing the time which might elapse between the appearance of Mr. Baily's beads and the total obscurity. I saw the moon's limb advance to the sun's, and cover it completely. I withdrew my eye for a moment from the eye-piece, when I heard my companion remark that the sun was nearly gone. I said firmly, "It is out." On being assured that it was not, I again applied my eye to the telescope, and to my infinite surprise I again saw the narrow ring of the sun's disc, not quite so bright as before. I again saw the moon's limb advance to the sun's limb, and cover it. In other words, I saw the totality completed twice. With regard to the *fact*, I can only say that I was at the time most fully alive to every thing which occurred, and that I was specially prepared for an observation which I expected to be one of the most important in the whole eclipse; and I have not the smallest doubt that the thing occurred, under the circumstances in which it was viewed with my telescope, precisely as I have stated. The *explanation* I cannot offer with great confidence, but I conceive that it may be

the following. I have already remarked, that the light of the sun's disc, very near to its limb, is considerably less than in those parts of the disc which are a little farther from the limb. This being assumed, it is evident that the interference of a cloud, which was sufficiently dense to hide the faintest part of the disc (at the limb), but not sufficiently dense to hide the brighter parts, would sensibly diminish the sun's diameter. Now I was assured by my companion, that there was a cloud upon the sun at the time when I first saw its extinction; and this cloud, though not sufficient to conceal the edge of the sun's disk from the naked eye, might be sufficient to conceal it as viewed in a telescope, in which the specific brightness of any surface is much less than to the naked eye, and which also was armed with a dark glass. But if this explanation is valid, it may apply to many other phenomena. I have frequently seen the sun's limb deeply notched, and I have conceived that this was due to irregularity of refraction: it may have been due to irregularity in the transparency of the atmosphere. Mr. Baily's beads may themselves have depended on this circumstance. I now return to my narrative.

I saw nothing whatever of beads, or other irregularity, in either of the extinctions of the sun's limb. The cusps were perfectly well defined till they met.

I quitted the telescope and looked round the horizon. The outlines of the mountains could with great difficulty be seen. But every thing, though not black, appeared horribly gloomy. My companion believed that there was a dark green tinge on every object. I did not remark it. I endeavoured to ascertain whether the darkness could be seen sensibly to travel over the great plain, but could not satisfy myself that it was so; the whole seemed to me to become dark at once. Professors Plana and Forbes, however, on the Turin observatory, (from which the mountains to the north and west are visible, but not the plain), were confident that they saw the darkness travel gradually. It is possible that from my elevated position I saw the country too much in detail to observe this; it is possible, also, that my eye was applied to the telescope at the critical time. The illumination was so small, that I could with difficulty read the divisions on the watch-plate, which was within eight inches of my eye; I did not try a printed book. The clouds were much less distinct than before, but as far as they could be seen they appeared terribly threatening. But the appearance of the moon can never be forgotten. It was like a black patch fixed in the sky, surrounded by a ring of faint light, whose breadth I estimated at $\frac{1}{8}$ of the moon's diameter (or probably four minutes). The colour of this ring was nearly white, inclining (as I thought) to peach-colour, but its illuminating power was very small. It was brightest at the lower part and to the left; this brightness travelled a little to the right. (It will be remarked, that a point to the left of the lowest point was the part last covered by the moon.) The clouds, however, were so near to the moon on all sides, and a dense cloud was so nearly in contact with it at the top, that it seems

exceedingly probable that some of these appearances might depend upon them.

I gazed earnestly at this remarkable ring, and I could not divest myself of the idea that it was produced by the sun's light shining past the moon's body through a portion of our own atmosphere. I wish it to be understood clearly that I do not offer this as an explanation of the ring (indeed, considering the number of miles by which the moon's limb overpassed the line drawn from the place of observation to the sun's limb, I cannot now consider such an explanation feasible). I wish merely to convey the impression which was given to me at the time of viewing the phenomenon. Indeed, I remarked at the time, that the appearance was almost exactly similar to that produced by a brilliant street-lamp, when its direct rays are just prevented from reaching the eye by a post, or by the corner of a building. I think it possible that there might be a very slight radial appearance in the light of the ring, but I do not recollect it with certainty, and I am perfectly certain that it was not sufficiently marked to interfere sensibly with the general appearance of annular structure. The moon appeared to be extemely near; her distance might have been estimated at a few hundred yards. The whole appearance of things was very unnatural and frightening. No stars were seen from the Superga, the sky being covered with clouds: but I found, from the reports of MM. Plana and Forbes, as well as from the conversation of many persons whom I met I general society, that many stars were seen at Turin, and at other places in the neighbourhood. I may take this opportunity of stating that I heard of distinct instances in which horses exhibited signs of very great terror when the totality came on.

I took off the dark glasses and carefully examined the moon with the telescope. Her disc was distinctly visible as having independent light, and I think that if it had been stronger, I might have seen the large tracts of different brightness on her disc. I could not, however, see the smallest inequality of light, of the nature either of broad dark tract, or dark spot, or bright spot. I looked carefully, for a long time (in proportion to the whole duration of darkness), and am confident that there was nothing of this kind to be seen.

While thus looking at the moon I saw, to my great surprise, some small red flames at the apparent bottom of the disc (the top as seen with the naked eye). The number of flames, as I have them impressed on my memory and as I find them drawn on a small pencil sketch made a few minutes after their appearance, was three; their form was nearly that of saw-teeth in the position proper for a circular saw turned round in the same direction in which the hands of a watch turn: their height was certainly not greater than one-fourth of the breadth of the ring, or probably a minute: the distance between the first and third was perhaps forty degrees or more on the moon's limb: their colour was a full lake-red: and their brilliancy greater than that of any other part of the ring. On my calling attention to these, my companion saw them

with the naked eye. It will be remarked that the part of the limb in which the red flames appeared was immediately in contact with the dark cloud of which I have spoken; and we attributed them to some irregularity in the density of the cloud's edge.

While engaged in watching the reappearance of the sun, I lost the opportunity of observing how the ring and the flames disappeared. Every luminous appearance, however, and every trace of the remainder of the moon's limb, vanished as soon as the smallest portion of the sun was uncovered. No beads or irregularity of any kind could be observed. The general illumination of the earth and sky was restored with very great rapidity. The clouds soon began to cover the sun, and in a short time it was invisible. In less than half an hour, a few drops of rain fell.

My companion, who had better opportunities than I had of observing the formation of the ring, &c., has given me the following account:—

"A bright line seemed to form round the right side of the moon before the disappearance, but not quite round, so that the ring was not complete: but, at the moment of the total disappearance, the ends seemed suddenly to join and form the complete ring, brightest on the left side, and as if beams of light came out. It continued brightest below, and at one time disappeared on the upper side, but a heavy black cloud was touching it there. Shortly before the reappearance, the brightness increased on the upper side; and immediately before the reappearance there were little beams of flame colour starting out. There was no defined edge to the ring: it changed sensibly, being brightest first on the left side where the sun had gone in, then below, and then on the right side; the light coming out at each place successively like little beams from the moon's edge. There was no remarkable change in the colour of the light till the little flame-coloured beams shot out for a few seconds before the reappearance. The general appearance of the country during the totality was very frightful; but every object, all the distant hills, &c. &c. were distinctly visible: it was like looking at objects through a very dark greenish glass. The sky in every part except that in which the sun was, was covered with thick clouds. The sun also was covered by the clouds very soon after his reappearance."

The numerous persons who watched the eclipse from the Superga appeared to notice with great interest the progress of the phases to the totality; and when the sun was actually hidden there burst forth from them, first a low murmur and then loud sounds of applause. Immediately after the restoration of the sun, the whole crowd dispersed, and nobody seemed to regard with the smallest interest the phases of decrease of the eclipse.

The meteorological circumstances of the atmosphere were evidently much affected by the concealment of the sun. The air after the totality appeared transparent (as it usually does when the ground is cold); and there rested upon or near to the flanks of the

mountains a series of stratified or cumulo-stratified cloud, at an elevation of less than 2000 feet; with a lower surface very sharply defined and (as far as I could judge) most truly horizontal through the whole extent to which I could see them; and with a less regular upper surface: the depth of these clouds could not be more than 200 or 300 feet. They had not the smallest resemblance to the ill-defined fog-clouds which hang about the mountains at all elevations in rainy weather; nor to any other clouds that I have seen during the day in these countries: I think that I have seen clouds in the evening which resembled them more nearly than any others.

After the termination of the eclipse, the day became very hot, and the aspect of the country became very similar to that which it usually presents at this season of the year.

It was not till some hours after my return to Turin that I found that MM. Plana and Forbes had not seen the moon at all during the totality. The region in which the sun and moon ought to have been seen was covered with an opaque cloud: I have little doubt that it was the same cloud which I from the Superga saw just above the moon (the azimuths of Turin and the moon being almost exactly opposed), and to which I was inclined to attribute some of the peculiar phenomena of the eclipse.

I fear that I have greatly trespassed upon the time of the Royal Astronomical Society; and I can offer only this apology, that in describing a phenomenon of such strange character, of which so few authentic accounts exist, there appears to be no possible way of including all those points which are really of scientific interest, except by narrating every thing which was seen, and leaving to others the power of selecting from the mass those circumstances which may possess some real value.

V. Mr. Baily communicated the substance of a circular letter, which he had received from Professor Shumacher, announcing the discovery of a Comet by M. Laugier at Paris, on the 28th of October. At $10^h\ 10^m$ mean time at Paris its right ascension was $16^h\ 41^m$ and its declination $+ 68^\circ\ 44'$. The right ascension increased, in six hours, $3^m\ 34^s$, and the declination diminished $20'$ in the same interval.

VI. Before the close of the meeting, an explanation was given by the Astronomer Royal, Mr. Airy, of the principle of an escapement recently invented by him, and intended to be applied to a clock made by Mr. Dent for the Observatory of Pulkowa. Mr. Airy observed that the severity of the Russian climate rendered the going of clocks furnished with the ordinary dead-beat escapement very imperfect; and he dwelt at some length on the defects of the remontoir escapement, which is the only one applicable to the case in question. These defects chiefly arise from the circumstance that, in all the remontoir escapements hitherto made, a retarding force is made to act upon the pendulum near the extremity of its swing, and an impulse is communicated to it on its return, thus, in both cases, tending to diminish the time of its vibration, and making

the clock gain to a considerable amount. The unavoidable inequalities even of the force given by a remontoir escapement produce, therefore, serious irregularities in the going of the clock.

Mr. Airy had met the difficulty of the case by devising an escapement which combines the properties both of the dead-beat and the remontoir escapements. Instead of the usual scape wheel he employs two, of which one is fixed upon the spindle connected with the pinion that is driven by the train of wheels, and the other moves freely upon that spindle, being connected with the fixed wheel by a long spiral spring, but having no other constraint on its freedom of motion. There are also two anchors (both firmly connected with the crutch), with pallets; the pallets which are acted on by the moveable wheel, are sloped in the usual way; those acted on by the fixed wheel are sloped in the opposite way. Thus the action of the moveable wheel (as driven by the spiral spring) is the action which really maintains the vibration of the pendulum; the fixed wheel only presses slightly on the curved sides of its pallets, and, at every swing of the pendulum, escapes like the other wheel, but without running down a sloping face of the pallet, and therefore merely winds up the spiral spring without exerting any maintaining power on the pendulum. To increase the maintaining power, it is only necessary to stop the moveable wheel, allowing the fixed wheel to go on, during a few beats of the pendulum: the fixed wheel then winds up the spring at one end, and it cannot (so long as the moveable wheel is stopped) expand itself at the other end. To diminish the maintaining power, it is necessary to stop the fixed wheel, allowing the moveable wheel to go on, for a few beats of the pendulum; the spring then expands itself at one end, and is not wound up at the other end. Small detents are provided, by means of which these wheels can be stopped without the need of touching them with the fingers.

Mr. Dent, with the permission of the Astronomer Royal, has applied the principles to chronometer escapements, and has constructed a chronometer with such an escapement attached. A large model of the escapement was exhibited in the meeting-room of the Society, and a clock having the escapement above described.

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ROYAL ASTRONOMICAL SOCIETY.

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The President, Lord Wrottesley, in the chair.

John Lane, Esq. R.N., of Keppel Street, Russell Square, was balloted for and duly elected a Fellow of the Society.

The following communications were read:—

I. A Letter from Professor Henderson to the Secretary, on the Parallaxes of certain Southern Stars:—

Edinburgh, October 5, 1842.

My dear Sir,—I applied to Mr. Maclear for the continuation of his observations of *α Centauri*; but I have received his answer, saying that he has no more at present to communicate. Two years ago, the greater part of the astronomical work at the Observatory was suspended by the operations connected with the measurement of the arc of the meridian. The observations of *α Centauri* have just been resumed, and also of some other stars in the same region of the heavens, which Sir John Herschel recommended as deserving investigation for parallax. Twelve months must, however, elapse before results can be drawn from these observations.

It occurred to me that it might be proper to ascertain what were the parallaxes that appeared from my observations at the Cape, of such stars as had been observed sufficiently often to reduce within reasonable limits the errors of observation; and although my observations were not made with a view of eliciting parallax, I reduced in the ordinary manner the observations of declination of twenty stars, comprehending most of those pointed out by Sir John. The following are the results which were communicated several months ago to Sir John and Mr. Maclear:—

No. of A. S. C.	Star.	Parallax.	No. of Observations.	Weight.
27	β Hydri	+0″·63	33	15·10
32	α Phœnicis	+0·90	33	8·73
182	α Eridani	+0·47	74	45·67
699	α Columbæ	+0·53	141	54·12
807	α Argûs	−0·08	89	46·44
869	ι Canis Majoris	+0·01	30	12·58
1008	γ² Argûs	+0·10	31	13·17
1032	ι Argûs	+0·28	32	15·90
1133	β Argûs	−0·04	32	15·26
1137	ι Argûs	+0·08	32	12·97
1276	ρ Argûs	−0·22	30	21·94
1281	η Argûs	−0·05	34	18·26
1427	α¹ Crucis	+0·48	17	8·91
	α² Crucis	+0·37	18	9·29
1439	γ Crucis	+0·25	20	7·57
1473	β Crucis	+0·12	45	12·57
1596	β Centauri	+0·28	43	13·16
1906	α Trianguli Aust.	+1·00	46	7·33
2398	α Pavonis	+0·15	32	5·70
2623	α Gruis	+0·62	31	4·01

Although considerable parallaxes appear in *α Phœnicis*, *α Trianguli Australis*, and *α Gruis*, yet, as the weights to be given to the results are small (the observations not having been made at the best times for parallax), I place little reliance on them. Nevertheless, I have recommended these stars to Mr. Maclear for re-observation.

Considerable parallaxes also appear in *β Hydri*, *α Eridani*, *α Columbæ*, and *α Crucis* (double star); and as the weights for these stars are greater, there may be a chance of some parallax being detected in one of them. I have therefore recommended them also to Mr. Maclear.

The parallaxes of the remaining twelve stars (amongst them is *Canopus*, which, after *Sirius*, is the brightest star in the heavens) appear to be so small, that we may conclude them to be beyond the power of the mural circle.

The mean of the 20 parallaxes is +0″·29; and we may, perhaps, suppose that this quantity is a nearer approximation to the true value of the mean of the parallaxes of these stars than any one of the individual results is to the true parallax of the star.

Consequently, if the parallaxes of some of these stars be greater than $+0''\cdot 29$, those of others (and probably the greater number) are less.

The number of positive results is remarkable; but it is possible that they are produced by some other cause than parallax affecting the observations.

In a conversation which I had with M. Bessel, he expressed his wish that *α Centauri* were observed with a heliometer, or good equatoreal, capable of precise micrometrical measurement; and he said that he had doubts of the results derived from meridian instruments. He mentioned the case of Dr. Brinkley's parallaxes, and stated that, in his own observatory, two excellent meridian circles, placed beside each other, gave, at certain seasons, places of the pole-star that differed from each other; the reason of which disagreement he had not found out.

This makes me very desirous that a good heliometer or equatoreal should be got for the Cape. Maclear reminds me that his portable 4-feet achromatic, though provided with a good micrometer, does not from its construction, permit the equatoreal adjustments to be employed between the zenith and the pole.

I am, my dear Sir,
Yours very faithfully,
T. Henderson.

The Rev. R. Main.

II. Observations of the Beginning and End of the Solar Eclipse of July 7, 1842, communicated by C. Rumker, Esq. in a letter to Dr. Lee.

At Hamburg, the beginning of the eclipse was not visible, but the end was observed by Mr. Rumker at $19^h\ 30^m\ 23^s\cdot 8$ mean solar time.

At Lubeck, the times of the beginning and end were observed by Mr. Franck:

Mean solar time of beginning	$17^h\ 36^m\ 38^s\cdot 9$
Ditto ditto end....................................	19 33 47 ·2

At Leipsic, the beginning was observed at $17^h\ 40^m\ 34^s$, mean solar time.

At Papenburg, the end was observed by Mr. Eglert at $19^h\ 12^m\ 15^s$, mean solar time.

At Presburg, Professor Marting observed,—

The beginning at	17^h	59^m	5^s	Mean solar time.
Beginning of total obscurity at .	18	56	29	,,
End of total obscurity at.........	18	58	57	,,
End of eclipse at	20	1	25	,,

At Presburg, *Capella*, *α Orionis*, *γ Orionis*, *α Persei*, *α Lyræ*, and *Polaris*, were visible. The obscuration was not, however, so complete but that the small print of a newspaper might have been read, and the part of the sky where the sun was had a lumi-

nous appearance; a dampness, however, was felt, and Reaumur's thermometer fell from 20° to 11°½.

III. Occultations observed at Yarmouth, by Arthur Utting, Esq., communicated in a letter to E. Riddle, Esq.

The weather was generally unfavourable at the time of the occultation of Jupiter by the moon on November 7, 1842; but, at Yarmouth, the clouds dispersed a few minutes before the time of the emersion, and a good observation was made with a two-feet Gregorian telescope:—

First appearance of Jupiter's west limb	$5^h\ 6^m\ 52^s{\cdot}5$	Mean solar time.
East limb in contact with the moon's limb...	5 8 1 ·5	„

The night of November 12th was very fine, and the immersions of κ^1 and κ^2 Piscium were observed as follows:

κ^2 *Piscium* at	$9^h\ 33^m\ 34^s{\cdot}7$	Mean solar time.
κ^1 *Piscium* at	9 57 53 ·7	„

Both stars disappeared simultaneously, as observed with a three-feet telescope.

IV. Sequel to a paper "On a new Method for greatly facilitating the Computation of the Moon's Co-ordinates." By S. M. Drach, Esq.

The object of this paper is to present, in a practical shape, the transformation of the lunar equations which had been suggested by the author in his former paper, for facilitating the computation of the moon's co-ordinates. Though the facilitation did not reach the extent at first anticipated, still it is hoped by the author that much labour will be saved to the computer of the places of the moon by the use of the method proposed.

The paper is accompanied by two skeleton forms, representing the details of the computations necessary for computing the co-ordinates by the use of the tables proposed by the author.

There was exhibited in the meeting-room an instrument which has been recently presented to the Society by Admiral Greig, a detailed account of which (accompanying it) will be read at the next meeting.

*** *The Twelfth Volume of the Memoirs of the Society is now published, and may be had of the Assistant Secretary, at the Apartments of the Society. Price to Fellows* 12*s*. 6*d*.

London:—Printed by Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *Jan.* 13, 1843. No. 27.

Francis Baily, Esq. Vice-President, in the chair.

John Eyre Ashby, Esq. B.A., of Homerton College, University of London, was balloted for and duly elected a Fellow of the Society.

The following communications were read:—

I. Translation of a Letter from Professor Hansen to G. B. Airy, Esq. the Astronomer Royal, on a New Method of Computing the Perturbations of Planets, whose Eccentricities and Inclinations are not small. Communicated by G. B. Airy, Esq.

"Sir,—I hasten to communicate to you a piece of astronomical intelligence of some importance. You are aware that all the methods that we possess for calculating the perturbations of the planets suppose that the eccentricities and inclinations are small, and that for those of the celestial bodies, which move in orbits very eccentric and very much inclined, we have been hitherto obliged to calculate the differentials of the perturbations for a great number of points of the orbits, and to integrate them by mechanical quadratures. I have just now discovered a method by which we can calculate the absolute perturbations,—that is to say, the perturbations for any time whatever, whatever be the eccentricity of the ellipse and the inclination of the orbit. For a first example of this method, I have calculated the perturbations of the comet of Encke produced by *Saturn*. The series to which my method leads are of such rapid convergence, that the perturbations of the longitude contain only forty-six terms, and the perturbations of the radius vector and of the latitude, somewhat fewer than this. I have reason to believe that it is impossible to reduce them to a less number of terms. Instead of writing here all the terms explicitly, allow me to represent generally the value for the time of perihelion passage.

"Here then is the first result of this kind, in which $n \delta t$ represents the perturbations of the mean longitude; u those of the hyperbolic logarithm of the radius vector, expressed in seconds, of

the above-mentioned comet; g' the mean anomaly of *Saturn;* and t the time, of which the unit is a Julian year.

$$
\begin{aligned}
n\delta t = -0''.06 - 1''.7152\,t &+ 1''.56 \sin g' - 14''.23 \cos g' \\
&+ 23.41 \sin 2g' + 20.65 \cos 2g' \\
&- 6.39 \sin 3g' + 8.52 \cos 3g' \\
&- 2.65 \sin 4g' - 2.89 \cos 4g' \\
&+ 1.43 \sin 5g' - 0.96 \cos 5g' \\
&+ 0.32 \sin 6g' + 0.55 \cos 6g'
\end{aligned}
$$

$$
\begin{aligned}
u = -1''.05 - 0''.1611\,t &+ 0''.61 \cos g' + 8''.86 \sin g' \\
&+ 33.10 \cos 2g' - 29.85 \sin 2g' \\
&- 9.01 \cos 3g' - 11.47 \sin 3g' \\
&- 3.41 \cos 4g' + 3.90 \sin 4g' \\
&+ 1.74 \cos 5g' + 1.11 \sin 5g' \\
&+ 0.32 \cos 6g' - 0.82 \sin 6g'
\end{aligned}
$$

"In the *Astronomiche Nachrichten*, Vol. IX. No. 211, M. Encke has published for three periods the separate perturbations of this comet for each planet, and for the time of perihelion passage. We are, therefore, able to compare these perturbations with their preceding general value. But in this comparison it is necessary to remark, that in the calculation of the perturbations by mechanical quadratures, there arises in the perturbations of the epoch of the mean anomaly a term proportional to the time, which does not exist in the absolute perturbations, and that it is, consequently, necessary to determine the value of and to subtract this term. This being premised, x being the value of this term for a whole revolution of the comet; $\varkappa$ the number of revolutions; Δm the perturbations of the epoch of the mean anomaly; $\Delta \pi$ those of the longitude of the perihelion; $\Delta \Omega$ those of the longitude of the ascending node; $\Delta \phi$ those of the angle of the eccentricity ($e = \sin \phi$); $\Delta \mu$ those of the mean motion; i the inclination; we have

$$n\delta t = \Delta m + \frac{(1-e)^2}{\sqrt{1-e^2}} \Delta \pi - 2 \sin^2 \frac{1}{2} i \,.\, \frac{(1-e)^2}{\sqrt{1-e^2}} \Delta \Omega - \varkappa x$$

$$u = -\frac{2}{3} \frac{\Delta \mu}{\mu} - \Delta \phi \sqrt{\frac{1+e}{1-e}}$$

"By substituting in these expressions the numerical values, which M. Encke has given at the place above quoted, we find for the period

Of 1819, Jan. 27·25 to	1822, May 24·0	$n\delta t = -67''.81 - x$;	$u = +94''.11$
———	1825, Sept. 16·3	$= -79.30 - 2x$	$= +100.78$
———	1829, Jan. 9·72	$= -124.42 - 3x$	$= +54.54$

"For these four times we have the mean anomaly of *Saturn*, augmented for the sake of greater correctness by the great inequality; thus

$$
\begin{aligned}
g' &= 266^\circ\ 8' \\
&= 306\ 42 \\
&= 347\ 13 \\
&= 27\ 44
\end{aligned}
$$

"If we substitute these values, as well as the values of t, 0; 3·322; 6·636; 9·952, in the preceding expressions of the absolute perturbations, we find for these four times,

$$\begin{array}{ll} n\,\delta t = -25''\!\cdot\!97 & u = -66''\!\cdot\!38 \\ = -46\cdot59 & = +28\cdot94 \\ = -8\cdot12 & = +34\cdot81 \\ = -3\cdot57 & = -12\cdot52 \end{array}$$

"If we subtract from these the first-mentioned values, we obtain the perturbations for the three above-mentioned revolutions of the comet. Thus

$$\begin{array}{ll} n\,\delta t = -20''\!\cdot\!62 & u = +95''\!\cdot\!32 \\ = +17\cdot85 & = +101\cdot19 \\ = +22\cdot40 & = +53\cdot86 \end{array}$$

"By comparing these values of u with those given before as found by M. Encke, we obtain the following differences:—

$$\begin{array}{l} +1''\!\cdot\!21 \\ +0\cdot41 \\ -0\cdot68 \end{array}$$

"By comparing in the same way the values of $n\,\delta t$, we find immediately

$$\begin{array}{lll} 0 = +20''\!\cdot\!62 - 67''\!\cdot\!81 - x & \text{or} & 0 = -47''\!\cdot\!19 - x \\ 0 = -17\cdot85 - 79\cdot30 - 2x & & 0 = -97\cdot15 - 2x \\ 0 = -22\cdot40 - 124\cdot42 - 3x & & 0 = -146\cdot82 - 3x \end{array}$$

"Hence we get $x = -48''\!\cdot\!711$, by the substitution of which there result the following differences:—

$$\begin{array}{l} +1''\!\cdot\!52 \\ +0\cdot27 \\ -0\cdot69 \end{array}$$

of the perturbations of longitude. These differences, as well as those of the perturbations of the radius vector, are smaller than might have been expected, when we reflect on the total diversity of the methods employed, and the long calculations which the method of mechanical quadratures requires. Besides, my method is so simple that I am astonished at not having discovered it long ago; I have employed only eight days for the calculation of the preceding perturbations, the general expression of which belongs to every point of the orbit of the comet. I have thus succeeded in solving this problem, of which we till the present time possessed no solution.

"I beg you to communicate this letter to the Royal Astronomical Society, and to the Royal Society of Sciences, and to accept the expressions of high consideration, with which I am, Sir,

"Your very obedient servant,

"P. A. Hansen.

"*Gotha*, 14*th Dec.* 1842."

II. On a new Arrangement of a Vertical Collimator attached to the Altitude and Azimuth Instrument. By W. Simms, Esq.

The only essential respect in which the altitude and azimuth instrument now before the Society* differs from similar instruments by which it has been preceded is this. The azimuth or vertical axis is perforated and fitted with an achromatic object-glass having a diaphragm in its focus, so as to serve, in conjunction with the spirit-level upon the instrument, as a vertical collimator.

At present the spider lines in the diaphragm of the collimator form an acute cross, subtending an angle of about 30°; but the preference of this arrangement over two parallel lines placed very nearly together, so as to present a narrow space for bisection, admits perhaps of question; my own habit being that of bisecting an angle by a line, leads me to give to it the preference, although I have found by experiment that very satisfactory results may be obtained by the other arrangement.

In this state of things, if the telescope be directed vertically downwards, the image of the cross in the collimator will be seen upon the diaphragm of the telescope; and the adjustment, independently of verticality which must be effected by the spirit levels attached to the instrument, consists in so rectifying the optical axes, that the centres, or intersecting points in the telescope and collimator, remain coincident during an azimuthal revolution.

The mode of adjustment, described in order, may be as follows, admitting, however, of variation at the pleasure of the observer.

1. It will be found convenient that the instrument be first generally levelled:—the azimuth axis by turning 180° in azimuth, and correcting by the feet screws of the tripod and the adjusting screws of the spirit levels; but, in all cases, if the error be not beyond the range of the scales, it is far better to leave these screws untouched, and to apply the correction by reference to the divisions upon the scales. The axis of the altitude circle must be rectified by the striding level, exactly in the same manner as in the transit instrument; all which, however, is too well understood to need a particular description in this place.

2. To adjust the line of collimation, bring the vernier marked A to 90° or 270° upon the azimuth circle, and, by means of the adjusting screws at the eye end of the telescope, make the middle vertical, or meridian line, bisect the angles of the collimator cross, turn 180° in azimuth, and correct half the error by the above-mentioned screws, and the remaining half by moving the object-glass of the collimator.

3. To correct the nadir point, set the vernier A to 0° or 180° upon the azimuth circle, 90° distant from its former position, and make the middle horizontal line bisect the angles of the collimator

* An instrument with the collimator attached was exhibited in the meeting room of the Society.

cross; turn 180° in azimuth, and correct half the error by giving motion to the telescope by means of the tangent screw, and half by moving the object-glass of the collimator. The micrometers should now be set to the zero points upon the altitude circle. By those, however, who prefer numerical corrections to mechanical adjustments, which, when extreme accuracy is aimed at, are always tedious and difficult to execute satisfactorily, the nadir point may be readily determined by reading the altitude micrometers with the circle in the reversed positions. The indications of the spirit-level fixed to the micrometer-bar must, of course, be carefully attended to in such a determination.

I shall conclude this notice by observing that the new application of the collimator does not deprive it of any uses and conveniences which it has in any other form, while at the same time it possesses advantages peculiar to itself. Its property, in common with the vertical floating collimator, of enabling the observer to set the axis of the altitude circle perfectly horizontal, irrespective of the riding level, is one which the level collimator does not possess. In common with the others, however, it affords a ready means of verifying and correcting the essential adjustments of the instrument without reference to any external object. An object adapted for such purposes should be both distant and well defined, conditions which imply a clearness of atmosphere perhaps not generally met with in any climate, and much less in that of our own country; moreover the collimator is equally available by night and by day: the light of a small lamp or taper being sufficient to render the lines visible.

But it has greatly the superiority, particularly in operations out of doors, over the vertical floating, and also over the level collimator, because the latter requires supports independent of, and equally steady with, that upon which the instrument itself is placed; things by no means easy of attainment under any circumstances, and to the scientific traveller often perfectly impracticable.

Neither is it a small advantage to dispense altogether with an additional instrument, which, to say the least, lessens the number of the traveller's cases, and with them his cares also. An extra instrument may by accident be injured, or through forgetfulness left behind, or for want of time to set up, or a steady support when set up, prove useless when it is most needed. But, by the new arrangement, the collimator becomes part and parcel of the instrument itself, and is so completely protected from injury that an accident could hardly impair or destroy it without at the same time destroying the entire instrument. Its introduction, too, into the perforated axis adds so little to the original cost of an instrument, that it may make a final claim on the score of economy.

III. Description of a Universal Instrument made by M. Ertel, of Munich, and presented to the Society by Alexis Greig, Esq. Vice-Admiral in the Imperial Russian Navy. By M. Ertel.

Translated from the German by Mr. Charles Knorre, and communicated by Admiral Greig.

This paper commences with a detailed statement of the cautions to be used in taking the instrument out of its cases, and of fitting it up for observation; and gives minute directions for rectifying and using it. It is accompanied by two drawings, the first of which is that of a projection parallel to the plane of the horizontal circle of the instrument; and the second exhibits in detail some of the essential parts of it.

IV. Occultations observed chiefly at Ashurst in the Year 1842. By R. Snow, Esq.

Date.	Star's Name.	Corrected Time of Immersion.	Moon's Limb.	Corrected Time of Emersion.	Moon's Limb.	
1842.		h m s		h m s		
March 22	o' Cancri........	8 10 45·22 [Sid.	dark.	....	....	Very well observed.
May 14	63 (p) Geminorum	....	....	12 3 11·5 [Sid.	bright.	Uncertain to 2s.
July 12	e Leonis........	....	....	15 54 31·0 [Sid.	bright.	
July 24	θ Aquarii......	23 35 19·33 [Sid.	bright.	0 41 52·83 [Sid.	dark.	Emersion uncertain. Daybreak.
July 31	9 Tauri........	20 48 ± [Sid.	bright.	21 31 2·94 [Sid.	dark.	At imm. Moon's limb low and undulating. Em. very good.
Sept. 26	B Tauri........	....	....	22 59 4·0 [Sid.	dark.	Clouds about the Moon, and Moon low.
Oct. 23	B Tauri........	6 48 42·56 [Sid.	bright.	....	....	Immersion beautiful; cloudy at emersion.
Nov. 12	$\varkappa^2$ Piscium	9 23 41·3 [M. Time.	dark.	....	....	Observed at Somerset House.
ditto.	$\varkappa^1$ Piscium	9 46 42·4 [M. Time.	dark.	....	....	
ditto.	$\varkappa^1$ Piscium	....	...	10 18 3·3 [M. Time.	bright.	Beautiful.
Nov. 22	ζ Cancri........	....	....	1 56 56·0 [Sid.	dark.	
ditto.	d^2 Cancri........	8 19 16·6 [Sid.	bright.	....	....	

The Long. of Ashurst is........ 0h 1m 10s·1 W.; the Lat. 51° 15′ 58″ N.
——— Somerset House.. 0 0 27·0 W.; —— 51 30 34 N.

V. Observations on the (apparently periodical) Variations in the Lustre of certain Stars of the First Magnitude. By T. Forster, Esq.

The author has had his attention directed to the subject of the apparently periodical variations of brightness in certain of the stars, since the year 1824; and he has at every opportunity continued to multiply observations for the determination of this point in Belgium as well as in Italy and England. He has been induced to lay before the Astronomical Society the results of his inquiry, by the additional interest which has been lately given to the subject by the researches of Sir John Herschel.

Under the conviction that all recorded facts are useful, especially in the infancy of any particular inquiry, he has extracted

from his note-books the observations which he has at different times made on several stars, notwithstanding their rudeness and the imperfections of their arrangement, with the hope of aiding an investigation into the nature and variations of star-light.

After trials of several photometrical methods of arranging the stars in order of brightness, he has of late adhered to the simple and natural comparison made with the unassisted eyes, from a conviction of its superiority to all other methods, and he trusts that habit has conferred thereon a sufficient degree of accuracy to render the observations useful.

The first stars which attracted particular attention, on account of the striking variations observed in their apparent lustre, were *α Orionis*, *Aldebaran*, and *Procyon*, and to these were afterwards added *Capella*, *Rigel*, and *α Lyræ*.

From a series of corresponding observations in April and May 1824, the following results were obtained, *Sirius* being added to the list on account of the varying difference between his lustre and that of the others.

The order of apparent brightness of the above stars, from more than twenty observations, in April and May 1824, was as follows for that epoch, the degrees of difference being marked by the number of vertical lines between the stars:*—

Sirius |||| Capella | α Lyræ | Procyon | Rigel = Aldebaran | α Orionis.

The correctness of the above comparison the author thinks can be relied on with great confidence, as nearly two months were devoted to it, the observations being continued nearly throughout every night, yet the arrangement is not in accordance with present observations. *Rigel* is, for example, equal to *Aldebaran*, and *Capella* (after *Sirius*) is at the head of a list of which *α Orionis* is at the bottom, while the magnitude of *Sirius*, by the most accurate measurement, was about four times that of *Capella*. The difference of colour between *α Lyræ* and *Aldebaran* renders their comparison subject to some uncertainty.

The relative lustre of the same stars with *Pollux* in November 1824, from many observations, was as follows:—

Capella | α Lyræ | Procyon | Rigel | Aldebaran | α Orionis || Pollux;

the most striking difference being between the last two, and there being a difference between the lustres of *Rigel* and *Aldebaran*, which were before equal. Now, as the sum of the differences between *Capella* and *α Orionis* in May is equal to 4, and in November is equal to 5, the whole difference from *Capella* to *α Orionis* must have increased one-fourth; and this the author takes to have happened from the decreasing lights of both *α Orionis* and *Aldebaran* at once; and he suggests the propriety of remarking in future whether in general the respective lustres of any given

* Where no lines occur it is indicated that the stars are *nearly* of the same lustre.

number of changing stars vary contemporaneously or alternately, the latter being rendered improbable by the distance of the phenomena in question.

The author obtained some anomalous results from a comparison of brightness of some stars in the summer of 1824, by means of the time of their first becoming visible after sunset; this test sometimes, with stars of different colours, seeming to reverse the order obtained by the former method of direct comparison by the eye.

In November 1827, the order of lustre, as observed at Boreham, in Essex, was as follows:—

Capella | Procyon = Rigel | α Orionis = Aldebaran.

In 1834, at Naples, the order was

Capella, Arcturus, α Lyræ || Rigel, Procyon | α Orionis, Aldebaran | α Aquilæ.

(The precise date of the above observations was omitted in the note-book, though the author believes it to be in the declining summer).

In September 1834, at Rome, the order was, from many observations,

Arcturus, Capella, α Lyræ | Rigel, Procyon | Aldebaran, α Orionis.

In November 1835, at Aix-la-Chapelle, the order was

Capella | Rigel = Procyon | α Orionis, Aldebaran.

(The observation of *Aldebaran* was bad).

In February 1837, the order was

Procyon = Rigel | Aldebaran || Pollux | α Orionis.

The author suspects some error in the above, finding, in November 1837, the order to be

Rigel = Procyon | Aldebaran = α Orionis.

January 16th, 1838,

Rigel = Procyon, α Orionis | Aldebaran | Pollux = Castor.

January 6th, 7th, 16th, and 17th, 1839,

Sirius |||| Capella || Rigel | Procyon | Aldebaran | α Orionis | Pollux = Castor = Regulus = Bellatrix.

The above observations were frequently repeated with the same results.

January 22d, 1839,

Sirius ||| Capella = α Lyræ | Rigel = Procyon, Aldebaran | α Orionis || Arided, Pollux = Regulus.

August 10th, 1839, while registering the numerous and brilliant meteors which the author has annually seen on this night since the year 1810, the following order of lustre was observed;—

Arcturus | α Lyræ || α Aquilæ || Arided.

November 11th, 1839, the order, as observed at Schäarbeck in Belgium, was

α Orionis = Rigel | Procyon | Aldebaran.

December 11th, 1839 (the night brilliant),

Rigel | α Orionis = Procyon || Aldebaran.

July 6th, 1840, the comparative lustre (observed by various instrumental means) was

α Lyræ || α Aquilæ || Arided.

July 29th, 1840,

Arcturus = α Lyræ || α Aquilæ || Arided.

January 9th, 1841, (a remarkably fine night),

Sirius |||| Procyon = Rigel | Aldebaran = α Orionis.

The observations were at this period suspended by the illness of the author, and were again resumed early in the year 1842.

March 1842, at Tunbridge Wells, the order was

Capella | Rigel | α Orionis | Aldebaran.

December 28th, 1842, at Bruges,

Sirius |||| Rigel, Procyon | α Orionis, Aldebaran.

January 1st, 2d, 3d, and 4th, 1843, at Bruges,

Capella, α Lyræ | Procyon, Rigel | α Orionis, Aldebaran.

The author hopes to be able to offer to the Society, at a future time, some more observations put together in a tabular form, and to accompany the observations with some historical notes respecting the changes in the colour and magnitudes of the stars.

Vol. XIV. of the Memoirs is now published, and may be had of the Assistant Secretary, at the Apartments of the Society. Price to Fellows Ten Shillings.

Vol. XIII. is now in the Press, and will be published in the course of the present year.

London :—Printed by Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *February* 10, 1843. No. 28.

Report of the Council of the Society to the Twenty-third Annual General Meeting, held this day.

In making this, the twenty-third, annual Report to the Members, the Council are happy in again announcing the progressive improvement of the Society, and the active steps taken to preserve its importance and utility: also in being able to announce that the expenses of the Society have been kept within the annual income, notwithstanding the cost of paper and printing, for the 12th volume of the *Memoirs* recently published, has been defrayed during the past year. Another volume (the 14th) is also now ready for delivery, and lies on the table; but the cost of printing this volume will not press so much on the funds of the Society, on account of the government having undertaken a portion of the expense, as will be more specially alluded to in the sequel of this Report. The 13th volume (which in point of order should have preceded this last-mentioned volume) is in a state of great forwardness, and will be finished in the course of the current year. So that within the space of 12 months no less than *three* volumes will have made their appearance, without encroaching much on the funds of the Society. The Report of the Auditors, which is here subjoined, will shew the state of the finances of the Society:

RECEIPTS.

	£.	*s.*	*d.*
Balance of last year's account	271	8	11
1 year's dividend on £900 Consols	26	4	4
1 year's ditto on £2000 3*s.* 1*d.* New 3½ per Cents	67	19	2
On account of arrears of contributions	48	6	0
70 annual contributions (1842–1843)	147	0	0
2 ditto (1843–1844)	4	4	0
3 compositions	63	0	0
Carried over	£628	2	5

RECEIPTS (*continued*).

	£.	s.	d.
Brought forward	628	2	5
10 admission fees	21	0	0
7 first year's contributions	11	11	0
Sale of Memoirs, by the Assistant-Secretary, from Jan. 29, 1842, to Jan. 27, 1843	45	3	0
Ditto by J. Weale, from March 1840, to Dec. 1842	42	5	8
Balance of the fund, granted by the Treasury, for the Cavendish experiment	98	5	8
	£846	7	9

EXPENDITURE.

				£.	s.	d.
J. Basire, for engraving and printing plates 1–6, Vol. XII.				34	18	9
Purchase of £20 18*s*. New 3½ per Cents				21	0	0
W. Magnay, for 50 reams of paper				45	2	0
J. Bagg for drawings and engravings for Cavendish experiment				10	15	0
J. H. Thompson for ditto				2	0	0
Moyes and Barclay, for printing Monthly Notices, Vol. V. Nos. 17 to 24				38	7	0
Ditto, for printing Memoirs, Vol. XII.				239	11	0
1 year's salary to the assistant-secretary				80	0	0
Harrison and Co. for stationery				10	3	10
Assistant-Secretary, for commission on collecting £277 4*s*.				13	17	3
W. Rumfit, for binding books, &c.				1	4	3
Charges on books, and carriage of parcels				3	10	0
Postage of letters				15	5	10
Porter's and charwoman's work, &c.				7	13	6
Tea, sugar, cakes, &c. for the evening meetings				13	13	0
Coals, candles, &c.				12	2	0
Sundry disbursements by the treasurer				16	13	3
Taxes: Poor's rate	6	3	3			
Church and rector's rate	3	16	6			
Sewer's rate	0	12	9			
Land tax	3	2	6			
Window duty	5	4	9			
				18	19	9
Balance in the hands of the treasurer (Jan. 28, 1843)				261	11	4
				£846	7	9

The assets and present property of the Society are as follow:

				£.	s.	d.
Balance in the hands of the Treasurer				261	11	4
3 contributions of four years' standing	£25	4	0			
5 ——— of three ditto	31	10	0			
17 ——— of two ditto	71	8	0			
39 ——— of one ditto	81	18	0			
				210	0	0
£2000 3*s*. 1*d*. 3½ per Cent Annuities; £900 3 per Cent Consols — valued at				£2850	0	0

2 Gold Medals unappropriated.
Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year, viz.:

	Compounded.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1842	93	131	84	6	314	35	349
Since elected	3	6	...	...	9	1	10
Deceased	−2	−6	−3	...	−11	...	−11
Resigned	...	−4	...	...	−4	...	−4
Removals	...	+3	−3	...	...	...	...
Expelled	...	−3	...	...	−3	...	−3
February 1843	94	127	78	6	305	36	341

With respect to the instruments of the Society, it may be proper to place on record the present state of them.

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* quadruple portable sextant,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-feet reflector,
The *Greig* universal instrument,

are in the apartments of the Society.

The brass quadrant, said to be *Lacaille's*,

is in the apartments of the Royal Society, for safe custody.

The Standard Scale

is in the possession, and under the care of the Astronomer Royal.

The *Cavendish* apparatus

at present remains in the possession of Mr. Baily.

The remainder of the instruments are lent, during the pleasure of the Society, to the several parties undermentioned: viz.

The *Fuller* theodolite, to the Lords of the Admiralty.
The *Beaufoy* clock, The two invariable pendulums, } to the Royal Society.
The *Lee* circle, to Lord Wrottesley.
The *Wollaston* telescope to Professor Schumacher.
The other *Beaufoy* clock, to Major-General Pasley, R.E.

The Council consider it here proper to announce to the Fellows, that a new arrangement has been made with respect to the sale of the Memoirs of the Society. Hitherto the sale has, from time to time, been undertaken by different publishers, whilst a

portion of them might also be obtained at the apartments of the Society; which has caused some confusion of accounts, and an uncertainty as to the place where they could be procured by the public. In order to avoid these difficulties, the Council have resolved that the only place for the sale of the Memoirs, both to the public and to the Fellows, shall in future be at the apartments of the Society, similarly to the plan adopted by many other societies; and the assistant-secretary is consequently empowered to conduct the sale, and to receive the produce of the same, agreeably to a plan laid down for his guidance. The Council trust that this arrangement will be economical as well as useful.

The Council regret that they have to lay before the meeting a list of a much greater number of deceased Members than on any former occasion: no less than eleven, during the past year, being removed by the hand of death; amongst whom are some of the earliest members of the Society. Although these sad events must always be a source of regret to the members, yet the Council trust that it will act as a stimulus to those who survive, to repair the loss which has been thus occasioned, and lead to new efforts for the promotion of the objects which it has in view.

The Earl of Macclesfield was one of the early members of this Society, and continued his connexion with us till the time of his death. He was the great-grandson of the first Earl of Macclesfield, who was President of the Royal Society in 1752; and who had established an astronomical observatory, fitted up with excellent instruments, at Shirburn Castle, near Oxford, which Bradley frequently visited. The observations made at Shirburn are now in the Savilian library at Oxford; and Bradley acknowledged his obligations to them in enabling him to complete his researches on nutation and refraction. The observatory does not now exist: but in the library, which is still in great preservation, and contains many valuable printed works, there is a large collection of original letters, from men of science, in the last century; amongst which the names of Oughtred, Flamsteed, the Gregorys, Barrow, Wallis, and Newton, frequently occur. The late Earl of Macclesfield, whose decease we are now recording, permitted the late Professor Rigaud to make a selection of such letters as he thought might be most interesting to the public, which have since been printed at the University press of Oxford, and at the expense of the delegates, in two volumes. Amongst these documents is the first letter which Flamsteed wrote to the President of the Royal Society, on Nov. 24, 1669, and which was not known to be still in existence prior to this discovery. Only a portion of it was printed in the *Philosophical Transactions;* and in the present publication there are yet certain portions omitted, which are not now considered to be interesting.

Mr. Smeaton was a civil engineer, and was descended from a brother of his celebrated namesake: he had been but a short time a fellow of the Society when he was removed by death.

Mr. Thomas Tulley was the second son of Charles Tulley, the

optician, in whose workshop he was brought up, and to whose business he succeeded, first in partnership with an elder brother, and afterwards alone, on the death of the latter.

Rear-admiral d'Urban has been dead some years, but the Council did not receive any news of his decease till very recently.

The Rev. Michael Ward had been a fellow of the Society for a long period. He was fond of astronomy, and possessed a small observatory.

Major-General Shrapnell was well known to military men as the inventor of the destructive shell which bears his name. He was for many years a fellow of the Society, though his period of active exertion was almost past before the Society was established.

Mr. James Moore French, chronometer-maker, at the Royal Exchange, was a zealous and successful artist, and on several occasions gained the prize given to the best of the chronometers which were tried at Greenwich.

Captain William Tucker, R.N. was introduced to the Society by his uncle, the late Mr. Frend, of whom an account appears in the last annual Report. He perished in November last, in the 48th year of his age, on the wreck of the unfortunate East Indiaman Reliance, which was lost off Boulogne. The circumstances of his death, and the devotion of his last moments, as narrated by survivors, to the performance of an act of humanity, created the strongest public sympathy. He had been almost all his life in active service, and particularly in cruising against the slave-trade, in which he had been remarkably successful; and he gained his commander's commission by a daring and prosperous attack upon a slaver of twice his force. He had been, previously to his death, in command of the Iris frigate, and senior officer on the Cape Coast station, and the failure of his health, which obliged him to return to England in a merchant-vessel, led to his unfortunate catastrophe.

Commander Michael Atwell Slater, R.N., was an officer who had gained distinguished reputation in the scientific branch of his profession; and was well known to many members of this Society for his zeal in the extensive surveys in which he was occasionally engaged. It was in one of these useful labours that he was unfortunately cut off in the prime of life, on the 2d of February in last year, by falling into the sea over the cliff called Holburn Head, on the eastern extremity of Scotland.

Mr. Innes of Aberdeen was well known to astronomers as a zealous calculator of eclipses, occultations, and tides; which occupied a considerable portion of his time. He was brought up as a watch and clock-maker: and although his professional gains were but small, yet by living very economically, he was enabled to collect together a valuable collection of books, which he has left behind him. He was a man of very mild temper and unassuming manners: and, after a very slow decay of health, died on the 22d of May, 1842.

The Council feel sure of the approbation of the Society at large

in the award of a Gold Medal to Mr. Baily, for his persevering and skilful management of, and complete success in, the repetition of the Cavendish experiment. The President has undertaken to explain in detail the grounds of this resolution, and to state to the meeting the more than usual obligation under which the Society has been laid by Mr. Baily's patient and sagacious proceedings. The publication of the 14th volume of the *Memoirs*, which is wholly devoted to an account of this experiment, renders any description, even of its general features, unnecessary in this place: but the Council cannot here refuse themselves the pleasure of recording their opinion, that in no instance whatever, since the foundation of the Society, has its medal been more worthily won, whether the result be looked at with respect to the skill and industry by which it was attained, or to the complete sufficiency of the Memoir in which it is promulgated.

While on this subject, it may further be stated that the 13th volume of the *Memoirs*, referred to in the last annual report as about to be presented to the Society by Mr. Baily, and containing the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, and Hevelius, is now nearly completed, and nothing but the attention requisite for the Cavendish experiment has prevented it from being actually ready. Thus the Society receives, in the course of one year, two of the most valuable volumes of its *Memoirs*, both from the labour, and one at the expense, of the same Fellow, and that Fellow the one of all to whom the Society is most indebted, independently of these rich contributions.

In the course of the last year, the question has been started, whether it would not be advisable to alter the numerical typography now in use, and to return to the old method of forming the Arabic figures, in the manner still usually practised in handwriting. A committee appointed to consider this subject reported unanimously in favour of the alteration, and the Council have accordingly given directions that it shall be carried into effect in all the future publications of the Society. The printers have met the proposition with a readiness which deserves the thanks of the Society, the change involving, as it does, some trouble and expense. Fortunately, however, it has been found that though the old type has been almost entirely disused for many years, the punches necessary to recast it are still, of every size which the Society wants, in the hands of the type-founders. The Council strongly recommend the alteration to the fellows in their own private publications, as they are sure that the form now in use bears no comparison, as to distinctness and legibility, with that which it is proposed to restore.

During the past year, the trustees of the Radcliffe Observatory at Oxford have, for the first time, published the observations made at that establishment, in an octavo volume, containing the observations made in the year 1840. The director of that observatory, Mr. Johnson, is one of our most active members, and well known to us as the author of the excellent catalogue of southern stars, printed at the expense of the East India Company, and rewarded

by this Society with its Gold Medal in the year 1835. Mr. Johnson, conceiving that it would be desirable to confine his attention principally to a selected class of observations, determined to re-observe those stars in Groombridge's catalogue that are situate to the north of the zenith of his place. The volume, here mentioned, contains the first attempt of this kind: and in it we see the same marks of minute accuracy and scrupulous integrity that were so evident in his former publication. The Council trust that the publication will be continued in like manner from year to year, as it is only in this way that the progress of discovery can be rendered of essential and permanent advantage.

It has been mentioned at the preceding anniversaries of this Society that the British Association had appropriated funds for three very useful and important catalogues, which the Council are now happy to state have been completed. The first is an extension of the catalogue of this Society, published in the second volume of its *Memoirs*, with certain additional columns that will render it of greater value to the practical astronomer. The British Association have granted the funds requisite for the publication of this work, which will soon be sent to the press. The next two catalogues contain the reduction of the stars observed by Lacaille at the Cape of Good Hope, and of those observed by Lalande at the *Ecole Militaire* at Paris. These catalogues are finished, and the British Association propose to apply to government for a grant of the requisite funds for printing them.

The Council, while reviewing the subjects connected with astronomy which have been brought before the Society since the last anniversary meeting, beg particularly to call the attention of the meeting to the labours of Professor Henderson and of M. Hansen. It will be remembered that in the President's address, on delivering the Gold Medal to M. Bessel for his researches on the parallax of the double star 61 *Cygni*, honourable mention was made of the labours of Professor Henderson in a similar inquiry with respect to α *Centauri*, founded on the reduction of his own observations made with the mural circle at the Cape of Good Hope. This indefatigable astronomer has within the present year presented us with the result of a series of observations made at his request by Mr. Maclear expressly for the purpose; and which, extending as they do considerably beyond a year, or the time during which the parallax goes through all its changes, and averaging from eight to ten observations of the double altitude of each star in every month, will at least afford good ground for determining whether the problem of the parallax of this remarkable star is likely to be solved by meridian observations. But without entering into the question of the evidence offered by the observations, our thanks are certainly due to the untiring zeal of Professor Henderson in prosecuting this most important but too much neglected branch of astronomy, and in this expression the Council feel sure that the meeting will join.

The meeting will scarcely need to be reminded of the discovery of M. Hansen, to which allusion has been made, his letter having

been so recently read before the Society. The want of some general method of expressing the perturbations of a body moving in an ellipse, whose eccentricity and whose inclination to the ecliptic are not small, has been, as it were, the opprobrium of modern physical astronomy. The method which has hitherto been employed of dividing the orbit into several portions, calculating the differentials of the perturbations for the points of the orbit thus decided on, and then integrating them by mechanical quadratures, seems scarcely worthy of the present state of analytical science; and has put the patience of astronomers to the severest trials as often as the return of an interesting comet, such as Halley's or Encke's, has made necessary the rigorous computation of its disturbances by the larger planets. Still, such has been the difficulty of the problem, that up to the present time no person since Lagrange seems to have suggested or hoped for any means of removing it. M. Hansen has laid before us the result of his first trial in the case of the comet of Encke, and the comparison of his computed perturbations with those made by Encke by mechanical quadratures, proves the accuracy of the method as well as the easy application of it. We may be allowed to express a hope that we shall be soon in possession of the method itself, which we are thus far entitled to regard as a most brilliant conquest over one of the residual difficulties of physical astronomy. The Council cannot refrain from congratulating the meeting on the above proofs that the science which we especially cultivate is still advancing; that each year adds something to our stock of previous knowledge of the constitution of the universe, and that, too, of an importance that marks the zeal and the talent with which, both at home and abroad, preparations are making for the complete solution of the few most interesting problems which yet remain to us.

The Council have great pleasure in drawing the attention of the meeting to the very valuable present which the Society has recently received from Admiral Greig, one of our members, and a distinguished officer in the Russian navy. This consists of an altitude and azimuth instrument, by M. Reichenbach* of Munich. The diameter of the azimuth circle is 15, and of the altitude circle 12 inches. The divisions, which are upon silver, read to 4″ of space by 4 verniers upon each circle. The instrument is one of the kind which admits of repetition both in the horizontal and vertical planes, and is furnished with two telescopes; the principal one resting in Ys attached to the azimuth index, and the other placed below the azimuth circle, according to the ordinary arrangement. But a peculiarity deserving especial notice is the manner in which the usual difficulty of observing near the zenith is obviated. A diagonal reflector, in this case a prism, directs the rays through one of the pivots of the transit axis, in which the diaphragm and eye-piece are consequently placed, and thus the observer remains

* In the *Notice* for January 1843, Article III. the instrument is erroneously stated to have been made by M. Ertel.

in an easy, unaltered position, whatever may be the altitude of the object observed. It is almost superfluous to add, that the graduation, axes, tangent screws, and other delicate parts of this instrument, exhibit all the proofs of care and skill for which the maker was so long celebrated.

The Council feel confident that the meeting will unite with them in a warm expression of their thanks to Admiral Greig, not only for his munificent present, but also for the proof he has thus given that, though at a distance, he still looks upon us and our proceedings with that interest and regard which are doubly grateful as coming from an absent friend.

Titles of Papers read before the Society, between February 1842 *and February* 1843.

1842.

Mar. 11. On an Instrument adapted for Observing Right Ascensions and Declinations of Stars independently of Time, accompanied by Drawings made with the Camera Lucida by Capt. Basil Hall, R.N. By M. Wettinger. Communicated, with a Letter of Description, to Sir J. F. W. Herschel, Bart., by Capt. Basil Hall, R.N.

Letter from Professor Henderson to the Secretary, dated Edinburgh, Jan. 31, 1842, on the Determination of the Parallax of *α Centauri*, by recent Observations made by Mr. Maclear at the Cape of Good Hope.

Positions of 78 Fixed Stars contained in the A. S. C., represented by Mr. Baily as not determined with sufficient accuracy, deduced from Observations made with the Meridian Circle of the Observatory of Kremsmünster. By M. Köller, Director of the Observatory.

Observations of Falling Stars made at Hereford on the night of Nov. 12, 1841. By Henry Lawson, Esq.

A List of Falling Stars observed Nov. 12, 1841, at St. Helena. By J. H. Lefroy, Esq. R.A. Director of the Magnetic Observatory at Longwood.

Path of the Moon's Shadow over the Southern part of France, the North of Italy, and part of Germany, during the Total Eclipse of the Sun on July 7, 1842. By Lieut. W. S. Stratford, R.N.

April 8. On the Aggregate Mass of the Binary Star 61 *Cygni*. By S. M. Drach, Esq.

Second Note on the Mass of *Venus*. By R. W. Rothman, Esq.

On a Method of determining the Latitude at Sea. By M. C. L. von Littrow, adjoint-Astronomer at the Imperial Observatory at Vienna. Communicated by the Rev. W. Whewell, Master of Trinity College, Cambridge.

1842.

April 8. On the Rectification of Equatoreals by Observations of Stars on the Meridian, and at an Hour-Angle of Six Hours. By M. C. L. von Littrow. Communicated by the Rev. W. Whewell.

The Parallax of α *Centauri*, deduced from Mr. Maclear's Observations at the Cape of Good Hope, in the years 1839 and 1840. By Professor Henderson.

Observations of the Beginning and End of the Solar Eclipse of July 18, 1841. By Dr. Cruikshank. Communicated by G. Innes, Esq.

May 13. Extract of a Letter from Professor Encke to F. Baily, Esq. on the Periodical Comet of Encke.

A Letter from Professor Chevallier, on some Phenomena observable in Total and Annular Solar Eclipses.

A verbal Account, by G. B. Airy, Esq., illustrated by a full-sized Model, of a Zenith Sector, then in the course of construction by Messrs. Troughton and Simms, under his direction, for the use of the Trigonometrical Survey.

June 10. An Account of some Experiments with the Torsion Rod, for determining the Mean Density of the Earth. By Francis Baily, Esq. Vice-President of the Society.

Nov. 11. A few Remarks on the Total Eclipse of the Sun, observed at Nice, July 8, 1842. By Capt. John Grover.

Observation of the Time of the Termination of the Solar Eclipse of July 8, 1842. By Arthur Utting, Esq. Communicated by E. Riddle, Esq.

Some Remarks on the Total Eclipse of the Sun, on July 8, 1842. By Francis Baily, Esq. Vice-President of this Society.

Observations of the Total Solar Eclipse of 1842, July 7 (July 8, civil reckoning). By G. B. Airy, Esq. Astronomer Royal.

An Explanation, by the Astronomer Royal, G. B. Airy, Esq., of the principle of an Escapement recently invented by him, and intended to be applied to a Clock made by Mr. Dent for the Observatory of Pulkowa.

Dec. 9. A Letter from Professor Henderson to the Secretary, on the Parallaxes of certain Southern Stars.

Observations of the Beginning and End of the Solar Eclipse of July 7, 1842. Communicated by C. Rumker, Esq., in a letter to Dr. Lee.

Occultations observed at Yarmouth. By Arthur Utting, Esq. Communicated in a letter to E. Riddle, Esq.

Sequel to a paper "On a new Method for greatly Facilitating the Computation of the Moon's Co-ordinates." By S. M. Drach, Esq.

1843.

Jan. 13. Translation of a Letter from Professor Hansen to G. B. Airy, Esq., the Astronomer Royal, on a New Method of Computing the Perturbations of Planets whose Eccentricities and Inclinations are not small. Communicated by G. B. Airy, Esq.

1843.

Jan. 13. On a New Arrangement of a Vertical Collimator attached to the Altitude and Azimuth Instrument. By W. Simms, Esq.

Description of a Universal Instrument made by M. Reichenbach of Munich, and presented to the Society by Alexis Greig, Esq. Vice-Admiral in the Imperial Russian Navy. By M. Ertel.

Occultations observed, chiefly at Ashurst, in the year 1842. Ry R. Snow, Esq.

Observations on the (apparently periodical) Variations in the Lustre of certain Stars of the First Magnitude. By T. Forster, Esq.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

American Philosophical Society.
Society of Arts.
Royal Asiatic Society.
The Lords Commissioners of the Admiralty.
Royal Academy of Berlin.
British Association.
Royal Academy of Sciences, Brussels.
Cambridge Philosophical Society.
Editor of the Athenæum Journal.
Royal Society of Edinburgh.
L'Académie Royale des Sciences de l'Institut de France.
Royal Geographical Society.
Geological Society of London.
Imperial Academy of Sciences at St. Petersburg.
Her Majesty's Government.
Meteorological Society.
Linnean Society.
The Editor of the Microscopic Journal.
L'Administration des Mines de Russie.
The Observatory at Vilna.
The Royal Society of London.
The Radcliffe Trustees.
The Editor of the Polytechnic Review.
Zoological Society.
Dépôt de la Marine de France.
The Hon. Board of Ordnance.
The Newcastle Philosophical Society.
The Literary and Philosophical Society of Manchester.
Society of Geneva.

M. Argelander.
Professor Bessel.
A. De Morgan, Esq.
E. J. Dent, Esq.
M. De Vico.
M. Dorn.

Professor Encke.
T. Forster, Esq.
Admiral Greig.
J. Herapath, Esq.
Professor Hassler.
G. Innes, Esq.
J. Lockhart, Esq.
M. Leverrier.
M. Nobile.
Professor Quetelet.
W. Rutherford, Esq.
Lieut. Raper, R.N.
The Rev. T. R. Robinson.
Lieut. Stratford, R.N.
R. Snow, Esq.
M. Struve.
M. Simonoff.
Professor Schumacher.
R. Taylor, Esq.

The President (Lord Wrottesley) then addressed the Meeting on the subject of the award of the Medal, as follows:

Gentlemen, — I have now the gratifying duty of stating to this meeting the grounds on which the Council have thought it right to award a Gold Medal to Mr. Francis Baily for his experiments to determine the mean density of the earth, in repetition of what is generally termed the *Cavendish experiment.* In the performance of this duty, I am necessarily required to offer a few remarks on the nature and utility of the end sought, on the previous attempts which had been made to gain it, and on the manner in which the one now before us was conducted by the distinguished friend of the Society to whom we are this day to offer our highest token of acknowledgment.

The labours of the astronomer are directed not only to the accurate determination of the motions of the heavenly bodies, but also to that of their constitution and organisation. If the papers in our *Memoirs* and those of other kindred societies seem to dwell much upon the former division of the subject, and little upon the latter, it is not of preference, but of necessity. Our means of determining satisfactorily any thing which relates to the constitution, even of the bodies of our own system, are few and limited: while those which apply to the prediction of their relative motions constitute the most perfect body of science which exists. But all which is known, certainly or conjecturally, of the interior arrangements of the heavenly bodies, is of the highest interest, and most especially when its action upon the minds of men is considered. Those who have heard the son and successor of the patriarch of this branch of astronomy, on occasions similar to the present one, deliver his views upon the general constitution of planetary systems, cannot but have felt that the subject which could inspire such thoughts must, were it for that reason only, be among the noblest to which human energy can be directed.

The masses of the planetary bodies are important data of the Newtonian theory of gravitation, but only in a relative sense. If, at any given instant, each one of the innumerable particles of which the universe is composed were to acquire a doubly attractive power, and, at the same moment, a double resistance to the alteration of

its state, the effect of the change would be unseen and unfelt, so far as the motions of the system are concerned. Nothing, therefore, is needed, in this last point of view, except a knowledge of the relative quantities of matter contained in the different planets.

There is something vague at the outset in the term *mass* or *quantity of matter*. With this the calculator of the planetary motions has nothing to do: the term *mass* is to him merely a convenient name for the numerator of the fraction which expresses the attractive force of that planet; and what number stands in any one numerator is of no consequence whatever, provided that all the other numerators are in their proper proportion to that one. Hence the mass of the earth, for instance, may be called unity, and the results of observation may be made to give the relative masses of other bodies.

What, then, is the mechanical signification of this numerator? The answer to this question belongs to the physical astronomer, properly so called.

Our first idea of mass is derived from bodies at the surface of the earth; under the same bulk some are lighter, others heavier. Two explanations present themselves: either the heavier body is composed of matter of the same kind as the lighter, but in a state of greater density, or more closely packed; or else the heavier body is composed of particles intrinsically heavier, that is, more powerfully acted on by the earth than those of the lighter. Which of these two explanations is to be received as the true one, more concerns the chemist than the astronomer, but the language of the former explanation is always used in our science. If we have reason to know that water compressed into one-twentieth of its bulk would produce all the mechanical effects of gold, then the latter is, and must be, in mechanics, considered as containing twenty times the quantity of matter of the former, under a given volume.

We are in the habit of referring solid bodies to water, in estimating their quantities of matter, and the specific gravities of the several kinds of matter, or the ratios of the weights of given bulks to the weight of the same bulk of water, have been carefully determined. Now among the questions of primary interest to the physical astronomer comes the following: — What is the quantity of matter in the whole earth, from which that in the several other planets is so easily determined? If the earth were a ball of water, what alteration would need to be made in the numerator of the fraction which expresses its attractive power? Both these questions are answered together; and it is only within the last seventy years that any attempt at an answer has been made on rational grounds.

If we knew the materials of which the earth is composed, it would be a mere matter of calculation to answer the preceding questions. But, ignorant as we are, *à priori*, even as to the fact whether there is an interior to the earth at all, that is, whether it is a solid globe or only a hollow shell, we cannot possibly be in possession of the means of forming so much as a guess at the

character of the answer. There is but one mode of proceeding, and that was, no doubt, suggested by the processes and results of the theory of gravitation. Since a comparison of the actions of two planets upon a third can be made to give the ratio of the masses of those planets, it is obvious that if we can compare the effect of the whole earth with the effect of any known part of it, we may deduce a comparison of the mass of the whole earth with the mass of that part of it. A mountain in one case, a large leaden ball in another, have been weighed against the whole earth by comparison of their effects upon a pendulum: the nearness of the smaller mass making it produce a sensible effect as compared with that of the larger; for by the known laws of attraction the whole earth must be considered as collected at its centre.

Before giving any account of the different modes pursued, it may be permitted to pause a moment, and to consider the bearings of the result. Independently of the satisfaction which the mind receives from the conversion of relative into absolute knowledge, independently of the stimulus given to the advance of inquiry by the acquisition of such a resting point as the determination before us, and of the value which it adds to the theory of gravitation as an instructor of the world at large in the laws of nature, by enabling the teacher to present what would have been a mathematical conception in a more physical and tangible form—there are consequences of no mean importance which spring from the comparison of the whole earth with one of its definite parts.

I do not undervalue considerations which present themselves at every step which is made, at every height which is scaled, when I confine myself on this occasion, and before this Society, to those only which particularly concern just knowledge of the foundations of astronomy, and fitting preparation for its advance among the physical sciences.

When the *Principia* was published, one of the first who gave an unqualified adhesion to the general views of Newton was the justly celebrated Huyghens.* But there was one point which he could not bring himself to admit; it was the universal attraction of every particle upon every other. He could not feel certain that, because the attraction of the whole of one planet upon the whole of every other was established, it followed that the parts of each attracted the parts of all the rest and of each other. Newton did not, and could not, make this important conclusion a fundamental experimental fact, though he was able to advance almost an overpowering presumption in its favour: and it is easy to see how the student of Descartes, even when he had been brought to admit the attraction of planet upon planet, might have suspended his opinion as to the action of part upon part, until the cause of the phenomenon, an inquiry much agitated in those days, was settled. But though the strong balance of probabilities in favour of Newton's opinion gradually gained for it a universal reception, there was no

* Hugenii Opera Reliqua, vol. i. p. 116.

ocular and crucial evidence for the physical fact until the experiments were made which I shall presently describe; and of those experiments, the one of Cavendish, which Mr. Baily has repeated, was by far the most convincing. Seeing is believing; and no one acquainted with the apparatus, and actually noting the visible effect of the approach of a leaden ball upon the oscillations of a torsion pendulum, after the nicest precautions had been taken to remove any possible effect of currents, magnetism, electricity, and heat, could doubt the presence of a totally distinct agent, producing the effect of attraction, and transmitting its influence through all the screens which were employed to exclude disturbing causes, with as much ease as through space occupied only by air.

It had been previously shewn by Laplace, that the mean density of the earth must exceed that of the sea, in order to preserve the stability of the oscillations of the latter: and subsequently, Poisson, in a paper contained in the seventh volume of the *Memoirs of the Institute*, concludes (taking Cavendish's value of the mean density as one of the data, and applying the forces with which the sun and moon act in causing precession, in a manner which is perfectly just, though open to a small amount of objection against the numerical accuracy of the remaining data) not only that there must be an increase of density from the circumference towards the centre, but that, even on the supposition of an homogeneous nucleus, the variable strata which enwrap this nucleus must extend to a depth of at least one-fourth of the radius. The physical astronomer, using that power of judging which the study of past discovery has made habitual to him, has long considered it as all but proved that our terrestrial globe is not only solid to the centre, or at least having its interior strata composed of fluid with the density of a solid, but also that the density of successive strata is gradually increasing from the surface to the centre itself. But those who have not studied physics, and even experimental philosophers of note who have not paid particular attention to the theory of gravitation, have frequently taken the hypothesis of an hollow globe as being that which has the highest probability in its favour. The late Professor Leslie, for example, takes the hollowness of our earth for granted, and proceeds to reason upon the manner in which the part of the interior which is destitute of solid matter is filled. It is his opinion that it is a sure result of induction,* that "the great central concavity is not that dark and dreary abyss which the fancy of poets had pictured. On the contrary, this spacious internal vault must contain the purest ethereal essence, *Light* in its most concentrated state, shining with intense refulgence and overpowering splendour." It is not my intention to discuss the grounds upon which, taking comparative hollowness for granted, the void is to be supposed filled with light or any other ether, but only to observe that the experiment before us strikes most completely at the preliminary assumption. Maskelyne made the earth to be a

* Elements of Natural Philosophy, vol. i. p. 453.

ball five times as dense as the same bulk of water; Cavendish, five times and a half; but Mr. Baily, whose experiments are far more numerous and well supported, five times and two-thirds. The solid rocks with which the geologist professes that his knowledge of the interior strata of our earth terminates, are only between three and four times as heavy as water: barytes itself, which takes its name from the great density of its compounds, has one of the heaviest of those compounds, the sulphate, under four times and a half its weight of water. If we were to ask what substance must be chosen, so that a globe uniformly consisting of that substance might take the place of our earth in the planetary system, retaining its size, the answer would be that nothing under the ores of silver or the lighter ores of lead would serve the purpose. The increase of density, then, from the surface towards the centre, is fully confirmed by this experiment: a highly probable result of the theory of gravitation is made as sure as any result can be. But while we thus admire the manner in which one inquiry is made to confirm the consequences of another, we must not forget that it is no small part of our province to apply the conclusions of sound knowledge to the destruction of those remains of the age of speculation which yet linger about the porch of inductive philosophy; and at which those who have safely gained the inner court will feel little temptation to laugh, when they remember how many may be, and probably are, led finally away by the delusions which lie in wait at the entrance. Among these the assumption that the earth is a mere hollow shell has held a conspicuous place: let us take this assumption, and grant that the depth of the solid matter is even one-eighth of the whole radius, which is more than many speculators would admit. To make such a supposition consistent with the result of the experiment before us, it must be inferred that the actual matter of the shell considerably exceeds mercury in mean density, and is all but equal to hammered gold. Let such a result be established, if it be possible; but in the meantime, and until presumption can be shewn in its favour, let it be the office of the experiment before us to check the wildness of mere hypothesis.

The French academicians, in measuring their South American degree, were the first who found sufficient local attraction in a mountain adjacent to their observations (Chimboraço) to give any hope of making that phenomenon useful in future inquiry. Maskelyne, in 1772, suggested the employment of astronomical observation in the neighbourhood of a mountain, for the determination of the earth's mean density. Schehallien was chosen for the purpose by a committee of the Royal Society, and the result of this celebrated experiment was announced in 1775. The description of this purely statical experiment is easier than in that of Cavendish: the position of a plumb-line, in a state of deviation from the vertical of the place, on account of the attraction of a mountain, is first to be accurately determined by making that plumb-line the regulator of an instrument for measuring zenith distances, and comparing the zenith distances thus obtained with those determined in other

places of known differences of latitude. The weight of the plumb-line is then acted on by three forces of known direction, one of them being of known magnitude. The remaining forces (one of which is the attraction of the mountain) can then be determined: so that if the distance of the mountain be known, its mass can be compared with that of the whole earth. The next step, and the most difficult one, is to compare the mean density of the mountain with that of water, which requires an accurate knowledge of its material and size. Maskelyne's rough computation, made with such knowledge as he had of the composition of the mountain, gave the mean density of the earth nine-fifths of that of the mountain, or from four to five times that of water; Hutton's subsequent and laborious investigation made four and a half for the ratio; but Playfair's examination of the strata of the mountain led to the inference that the result of the experiment could only be considered as placing the same ratio between four and six-tenths and four and nine-tenths. It is worth noting that Newton (*Princ.* book iii. prop. 10) had ventured a conjecture which, as happens so frequently with him, turns out to be true. He thinks that the mean density of the earth is *between five and six times that of water.*

The method first proposed by Michell, and adopted and executed by Cavendish, was wholly independent both of astronomical data and of the uncertainty of the material of comparison. A horizontal pendulum, suspended by a wire, the torsion of which caused it to make slight oscillations about a position of equilibrium, was substituted for the gravitation pendulum, or plumb-line; while a massive leaden ball took the place of the mountain. If the torsion pendulum had been as secure of its position of equilibrium as that of gravitation, the experiment would have been almost as identical in its details with that of Maskelyne as it is in its principle. The time of an oscillation would first have led to the settlement of the amount of torsion force in any given position of the pendulum: it is well known that nothing but the time of oscillation is wanted to give the means of determining the quantity of restitutive force which acts on the pendulum at any degree of departure from equilibrium. The degree of departure caused by submitting the ball at the end of the torsion pendulum to the lateral influence of the large mass would then have given the means of calculating the attraction of that mass, just as the amount of deviation of the plumb-line gave that of the mountain in Maskelyne's experiment. Small as may be the leaden mass compared with the mountain, it was so much better known as to make the risk of error materially less; to which must be added, that it was submitted to an instrument of very much greater delicacy than the ordinary plumb-line.

The torsion pendulum is, in fact, possessed of such an extreme susceptibility, that every detail of the experiment requires adaptation, to an extent which would make a superficial inquirer wonder how it could be in any way compared with that of the plumb-line and mountain. There is no position of equilibrium — the instru-

ment is never at rest. The effect of presenting the large leaden ball is not to draw the torsion-rod from one position into another, but to change its motion from an oscillation of one extent about one point of rest to one of another extent about another point of rest; and a careful and peculiar mode (into which I cannot here enter) must be practised of determining this point of rest, both before and after the pendulum is placed under the influence of the leaden mass. This point of rest, so called, is not at rest, and resembles one of the elements of an elliptic orbit, which would remain constant if there were no foreign disturbance, but is perpetually changing from the action of other planets. And so fast do the indications of the pendulum vary, that is, so rapidly does it change the character of its oscillation, even where no visible cause is at work, that it cannot be permitted to take a long series of observations to determine either the point of rest which precedes the change of position of the attracting mass, or that which immediately follows. For the instrument is always in a state of change, in a manner which would lead to the supposition that the torsion of the suspending wire is either visibly influenced by invisible changes of temperature, &c., or that it is acted upon by some agent of a character wholly unknown.

From seventeen experiments, Cavendish, in 1797, deduced 5·45 for the mean density of the earth; and nothing further was done in this species of experiment until 1836, when it was repeated by M. Reich, of Freiberg, who followed Cavendish's plan in every particular, except in having one leaden mass instead of two. From fifty-seven experiments, M. Reich deduced 5·44 for the mean density of the earth,— a result almost identical with that of Cavendish.

In the year 1835, the Council of this Society appointed a Committee to consider of the best mode of procuring a repetition of the experiment. After some delay, occasioned by the difficulty of procuring funds, and choosing a site, the construction of the necessary apparatus was commenced at the end of 1837. On a representation made by the Astronomer Royal, the Government, in the year just named, granted £500 for the purpose; and Mr. Baily (to whom the conduct of the whole was intrusted) chose, after much deliberation, to make his own house the scene of operation. About £400 has been expended in the actual experiment, and it is right here to acknowledge the liberality of the present Government, which has sanctioned the application of £100 remaining out of the sum granted by the late Government, to the payment of part of the expense of printing the results.

I could hardly undertake to make a description of the apparatus intelligible in little time, or without diagrams; but fortunately this attempt is rendered unnecessary by the fact of an abstract of Mr. Baily's paper having been for some months in the hands of the fellows of the Society. Keeping in view, then, the main point of the present address, I shall make some remarks upon the progress of the experiment, with reference to the justification of the decision

of the Council, on which it will presently be my duty to present a gold medal to Mr. Baily.

And first, with regard to the responsibility which was imposed. It ought to be most distinctly understood, that the functions of the Committee appointed by the Council ceased as soon as the money was obtained for the expenses, and Mr. Baily's offer of superintendence was accepted. Not one direction was given, not one single condition was imposed, not one syllable was put upon record by which failure, had it occurred, could have been thrown from the shoulders of one upon those of several. The suggestions which Mr. Baily acknowledges himself to have received from men of science, whether in or out of the Society, were submitted solely to his judgment, as much as if the experiment had been his own private undertaking. Consequently, it is just that it should be most explicitly acknowledged that all the merit of success lies where all the blame of failure would have fallen. But, while I say this, I must also at the same time observe that the disposition to ask advice, and to try advice, which has characterised Mr. Baily's progress throughout, has been in no small degree conducive to the distinguished character of the result. The Council, when they placed in his hands, and at his sole disposal, the public money with which they had been intrusted, were well aware that no pains would be spared to collect, to compare, and to choose among, the best opinions which could be obtained. And, while on this subject, it is proper to acknowledge the obligations of the Society to Professor Forbes, for the suggestion which overcame the difficulty, and nearly destroyed the anomalies of the torsion pendulum, and to the Astronomer Royal, for his paper on the mathematical theory of Cavendish's Experiment, which will appear as a part of Mr. Baily's *Memoir*.

In the next place, it is necessary, before proceeding to the business of the day, to separate most emphatically the conductor of the experiment from the able and energetic friend of the Society in other respects. If an endowment had been bequeathed for the purpose of enabling the Council to give a yearly medal to the Member who should have been most active in carrying on the ordinary and extraordinary business of the Society, who is there that hears me, I speak particularly to those who are, or have been, on the Council,—who could positively undertake to say that such a medal could, up to this time, have been gained by any one except Mr. Baily? No doubt we have many among us whose active services the Society must most gratefully acknowledge:—our records prove that fact: indeed, I think I may venture to say that ours has been a fortunate one among societies in the amounts of service rendered by individuals. No doubt, again, that the honourable leisure gained by a long attention to business has placed in Mr. Baily's hands a power of serving the Society which most of the Fellows do not possess. But this does not alter the fact, that he has been, ever since its foundation, identified with its progress, and assisting its efforts, in a greater degree than any

other individual member. I state this now, not to acknowledge services which are so perpetually before the minds of all who take an interest in us, but to request you, if you can, to forget them for a short time, and to look on the experiment before us as if it had been the work of a new man, hitherto unknown to the Society, and resting his claim to our gratitude wholly and solely upon its conduct and its result. It is only thus that you can fairly affirm the verdict which the Council has already given, and the approval of which it so confidently expects at your hands; for you must remember, gentlemen, that no services whatever, except those expressed in the resolution awarding this medal, can count in the smallest degree as a justification of that resolution. I am the more desirous of impressing this upon you, when I consider that this is the first occasion on which a medal has been awarded for the manner of employing money intrusted by the public to our charge: so that in fact, this defence, if I may so call it, of the award, is more than the explanation of the Council to their constituents, or at least must become something more. When it has gained your approbation, it must go forth to the public, and to the government, as the account which the Society renders of the funds which were placed in its hands, as the proof that those funds were worthily administered. I am not, of course, alluding to the mere vouchers of pecuniary integrity, to the proof that money asserted to have been spent upon apparatus was actually so expended, but to the justification of a yet higher character.

The actual observations, meaning those which are printed in the *Memoir*, are 2153 in number, varying from ten to thirty minutes each; so that I am under the mark considerably when I say that 600 hours were spent at the apparatus, in the mere act of watching the oscillations of the torsion rod. To this must be added nearly as many more in the series of experiments, of which the results were afterwards abandoned on account of the anomalies of the pendulum. Add to this the time expended in contrivance, in computation, and in deliberation, and it will appear that it is not often that any single inquiry has called forth so determined an exercise of industry and perseverance. The experiments were commenced in October 1838, and were continued until May 1842. With the exception of about a couple of months of interruption, caused by the severe accident which, as all present are aware, happened to Mr. Baily in the summer of 1841, there was a continued succession of trials. This long and patient investigation gives a peculiar value to the result: the effects of the several conditions of the atmosphere cannot but have been eliminated, since the *printed* experiments, or those which were made *after the anomalies of the pendulum were got rid of*, run over nearly a whole year.

I cannot but call your attention to the spirit in which every part of the investigation was conducted. When all possible precautions had been taken to secure the stability of the pendulum; when it had been ascertained that no degree of concussion, whether in the room which contained the apparatus, or in that immediately

above, would produce any sensible effect upon it; when every pains had been taken to remove thermal, electric, or magnetic disturbance; and at the moment when it seemed next to certain that the result, whether that of Cavendish or another, was about to come out clearly, easily, and honestly, it was found that the work was yet to begin; that, in fact, the torsion pendulum was subject to every species of anomalous motion. In vain were such new precautions taken as these appearances suggested, and, for eighteen months, a period so easy to speak of, so difficult to employ in activity under discouragement, no clue appeared to the solution of phenomena which seemed to set all the known laws of mechanics at defiance. Nothing could be more even than the temperature, even at the times when the anomalies were greatest; so that the effects of heat seemed to be out of the question as an explanation. The torsion rod, secure in a wooden case with thick glass ends, seemed to bid defiance to all external cause of currents of air; and so completely was the pendulum isolated, even from the case itself, that the latter might be violently shaken without any perceptible effect upon the former. Cavendish and Reich had both observed corresponding anomalies, but both apparently considered that their effects would disappear in the mean of a large number of observations. Mr. Baily resolved not to quit the subject until the cause of the anomalies was detected, and upon no account whatsoever to present a result vitiated either by assumption of the nature of the difficulty, or by rejection of the observations which appeared most discordant. To this resolute and honest determination we owe it that the paper for which this medal is given to-day is hardly less valuable as a lesson upon the nature and use of the torsion pendulum in measuring small forces than as a determination of the mean density of the earth.

It was at last suggested, and, as I have before stated, by Professor Forbes, that possibly the radiation of heat from the large masses might, when they were brought up close to the torsion box, or case of the pendulum, affect the inside of that case. It was already known that the evaporation of a few drops of spirits of wine sprinkled on the side of the case would produce a large and rapid effect on the pendulum. But, between this frame and the large masses there had already been interposed a wooden screen, which, it was thought, would wholly prevent any effect of radiation. The suggestion above mentioned was accompanied by a recommendation that the outside of the case, and the masses themselves, should be gilt. Mr. Baily carried his precautions still further: the case was first wrapped in flannel, over which a new case (gilt) was placed; the masses, the plank which carried them, and the interior of the frame-work which inclosed both torsion box and masses, were covered with gilt paper; the leaden balls at the two extremities of the torsion pendulum were gilt and burnished; and the masses were made to stand, when at the nearest, a little further from the torsion box. These precautions proved to be sufficient; the anomalies were substantially removed, shewing themselves only now

and then, and in smaller quantities. The lesson thus read to experimentalists on the effects of radiant heat will, it may be hoped, lead to further inquiries. Mere creation of difference of temperature was insufficient to point out the source of the anomaly: red-hot balls, lamps, or lumps of ice, placed near the torsion box, failed to give any clue to the cause of the difficulty.

All the observations, good, bad, or indifferent, made since the removal of the effect of radiation, have been taken into account in the general mean. And they have all been printed at full length, in such a manner that any one can go through the whole of the calculation in every experiment, from the announcement of one of the raw data of observation at the telescope, to the deduction of the mean density of the earth. Thus, as no experiment was ever more honestly or diligently performed, so also none has ever been more completely or satisfactorily described.

Now as to the result. The mean of all the experiments gives 5·675 as the mean density of the earth, with a probable error of ·004. The balls used at the ends of the torsion pendulum have been lead of different sizes, brass, platina, zinc, glass, and ivory; and in some of the experiments, a torsion rod of brass, without any balls, has been used. Various modes of suspension have been employed, single copper wire, double iron, double brass, and double silk thread. The discordances which occur between the results of different balls, modes of suspension, or both, have every appearance of being the consequence of an insufficient knowledge of the torsion pendulum, and lend no countenance whatever to the suspicion that the attraction of matter upon matter varies in different substances. A moderate examination will shew that there is no doubt that the discordances, being such as might have been looked for in any inquiry, must disappear in the mean of so large a number of observations.

We may, then, confidently assert that this important element of the physical part of astronomy is settled, within very narrow limits of error. But suppose, if possible, that a less degree of trust were to be accorded to the mere result: nay, go further, and imagine the theory of gravitation itself, the best demonstrated of all general laws, to be an unfounded delusion. Perhaps we are then, on such a supposition, to give a still higher degree of praise to the manner in which this inquiry has been conducted. All the experiments are published, and all the experiments are *facts*. I have no doubt, and those who hear me have no doubt, that attraction is as real an existence in physics as it is an explanatory hypothesis in mathematics: and experiments of the nature of that before us seem to me, as to you, to put this beyond doubt, both in the hands of Maskelyne, Cavendish, Reich, and Baily. But if we be wrong, how shall we ever be brought to know our error? How, except by experiments conducted with that firm honesty of purpose, and true absence of all bias, which has characterised those described by me to-day. I repeat that these experiments are facts, facts which are and will be true, facts which are the result of an inquiry in which the Newtonian

doctrine was fairly thrown into the scale, and weighed by Nature's own weights. The confirmation of the general truth of Cavendish's mean density of the earth is the *numerical* result; but one more assurance that a century of patient and truth-loving inquiry will not fail of its reward, is the *moral* result.

It generally happens that in the award of our honorary distinctions, our immediate interest is limited to that which we must always take in an extension of our science, whether of principle, of process, or of fact; from what country or quarter soever it may come. We always acknowledge an addition to astronomy as a claim upon our gratitude: how particularly then is it incumbent upon us in the present instance, when the character of the Society depended upon the fulfilment of the pledges under which the means of making the experiment were obtained. Had that experiment failed, had it shewn that Cavendish had deceived himself when he thought of obtaining the earth's mean density by his now established mode, there might indeed have been regret, but there would have been no shame. In any human undertaking no censure need be justly feared, when it can be shewn that extreme diligence, unshrinking honesty, long deliberation, and the most candid research after, and use of, the suggestions and advice of others, have marked its progress from the beginning to the end. But when to all this we have a right to add success—when we can feel that we now present to the philosophical world a result on which they may confidently rely, knowing that the history of the experiment bears evidence which cannot be mistaken of its intrinsic value—we must consider this medal a token of what I may venture to call the *personal* gratitude of the Society towards one of its body who feared neither responsibility nor toil in its cause.

When, at some future time, those who are to profit by the labours of our day shall, with improved instruments and extended knowledge, once more repeat the interesting experiment to which these remarks refer, they will find in the records of this attempt proofs of its honesty which are now hidden from us by those very instrumental deficiencies and theoretical imperfections, the removal of which will be the signal to renew the process. And, in like manner as we now render due honour to Cavendish, not only for the first actual performance of the experiment itself, but because, with comparatively rude apparatus and few trials, he came so near the truth of which he was in search, so will they remember to celebrate the patience, the integrity, and the sagacity of the philosopher who made the next step, and who shewed them the path of amelioration. I cannot better express my strong feeling for the honour and welfare of this Society, than by claiming your response to an earnest wish and desire that, when that time shall come, our Fellows may be among the foremost promoters of the revival of this experiment, and that they may find among themselves one to whom they dare intrust the sole superintendance, and who will justify their confidence as well as Mr. Baily has done that of your Council on the present occasion.

The President then, addressing Mr. Baily, continued as follows:

Mr. Baily,—I present you with this medal in the name of the Society for which you have done so much, as the highest testimony which they have power to bear to the splendid service which you have rendered to the science of Astronomy. I thank you in their name for the care which you have taken of the honour of the Society, and for the augmentation which it has received from your labours. And to our sincere congratulations upon the providential escape which you experienced, during the prosecution of your inquiry, from a sudden and violent death, I add the expression of our earnest hope that you may long be spared to continue your services in the advancement of knowledge, and to enjoy that well-earned fame which will wait upon your life and your memory.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.:

President: Francis Baily, Esq., F.R.S.—*Vice-Presidents:* George Biddell Airy, Esq., M.A. F.R.S., *Astronomer Royal;* Augustus De Morgan, Esq.; Rev. George Fisher, M.A. F.R.S.; Lord Wrottesley, M.A. F.R.S.—*Treasurer:* George Bishop, Esq. —*Secretaries:* Thomas Galloway, Esq., M.A. F.R.S.; Rev. Robert Main, M.A.—*Foreign Secretary:* Captain W. H. Smyth, R.N. K.S.F. D.C.L. F.R.S.—*Council:* Samuel H. Christie, Esq., M.A. F.R.S.; Rev. W. Rutter Dawes; Thomas Jones, Esq., F.R.S.; John Lee, Esq., LL.D. F.R.S.; Captain W. Ramsay, R.N.; Edward Riddle, Esq.; Richard W. Rothman, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Lieut. William S. Stratford, R.N. F.R.S.; Charles B. Vignoles, Esq.

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The following communications were read:—

I. Occultations observed at Port Royal Dockyard, Jamaica. By Capt. Alexander Milne, H. M. S. Crocodile. Communicated by the Rev. Geo. Fisher.

Date.	Phenomenon.	Star.	Mean S. Time.	Longitude Computed from the Elements as given in the N. Almanac.
1839.			h m s	° ′ ″
Nov. 17	Im.	δ Piscium	6 50 37·0	76 48 52 West.
19	...	μ Arietis	6 28 54·7	76 47 7
20	...	*b* Pleiadum	5 51 53·3	76 50 30
20	...	*e*	6 6 20·3	76 49 12
20	...	*c*	6 13 3·3	76 48 35
20	Em.	?	12 6 30·0	

The two first observations are excellent ones. The results from the others may be vitiated in a very slight degree by the moon being a little past the full at the time of observation; but the times I believe to be faultless. The latitude of the place of observation (which was the Commodore's House) was determined by a mean of several observations, made with great care, to be 17° 56′ 35″ North.

II. Observations of the Beginning and End of the Solar Eclipse on the 8th July, 1842. In the Fort on the left bank of the Shanghaie River, near to the Town of Woosung, on the Coast of China. By Capt. Sir Everard Home, Bart. F.R.S., H. M. Ship North Star. Communicated by the Rev. Geo. Fisher.

Latitude of the Place of Observation, 31° 26′ 36″ North.

	h m s	
Beginning of Eclipse, M. S. Time	3 16 54·7	nearly.
End ditto	5 24 1·0	Very good observation.

The error of the chronometer, determined by several sets of observations of the upper and lower limbs of the sun with an 8-inch sextant and false horizon by Troughton; telescope by Tulley, 4 feet; aperture, 3½ inches; power, 90.

III. Translation of a Letter from M. Hansen to R. W. Rothman, Esq., accompanying a copy of a printed paper on the Perturbations of the Heavenly Bodies moving in very Eccentric and very Inclined Orbits.

"Sir,—I have the honour to send you herewith the abstract of a memoir in which I have developed a method for calculating the perturbations of those celestial bodies which move in very eccentric and very inclined orbits. I beg you to forward this abstract to the Royal Astronomical Society. In this memoir I have treated only the case in which r is less than r', and I have simply adverted to the case in which r is greater than r'. That I may be better understood, I add here that, in this case, it is the true anomaly of the perturbed body which must be employed. The integration is performed in this case in an analogous manner; but we cannot make the factors of integration depend on continued fractions: those factors depend in this case on a linear differential equation of the first order.

"Accept the expression of high consideration with which I am, Sir, your very obedient servant,

"P. A. Hansen.

"*Gotha, March* 1, 1843."

IV. On the Application of the Method of Least Squares to the Determination of the most probable Errors of Observation in a portion of the Ordnance Survey of England. By Thomas Galloway, Esq. A.M. F.R.S., one of the Secretaries of the Society.

The object of this communication is to give the results of an application to a part of the Ordnance Survey, of a general method of correcting the observed horizontal angles, whereby the positions of the stations are determined in such a manner as to give the nearest, or most probably accurate, representation of the whole of the observations. The method in question, which is due to Gauss and Bessel, has only recently been introduced into geodesy. In all the geodetical measurements which were executed prior to the latter part of the last century, the errors of observation were of such magnitude that it was unnecessary to take account of the curvature of the earth, and the triangles were accordingly computed as if they had been on a plane surface. On this hypothesis the sum of the three angles of each triangle is 180°; and as the strict fulfilment of this condition is necessary for the computation of the triangles, the universal practice was to apply arbitrary corrections to the observed angles, the observer usually assigning the largest correction to the angle which he *supposed* most likely to be erroneous. The large and excellent theodolite used by General Roy in his triangulation (begun in 1784) for connecting the observatory of Greenwich with the meridian of Paris, and the repeating circle of Borda, introduced about the same time on the Continent, gave the means of measuring terrestrial angles with far greater precision than had been obtained with the old quadrants,

and the curvature of the earth became a necessary element in ascertaining the amount of the errors of observation. No alteration was, however, required on this account in the mode of correcting the angles, for as the *spherical excess* can be computed from approximate values of the sides to any required degree of exactness, the condition necessary for the computation of an individual triangle was still that the sum of the three horizontal angles should be equal to a known quantity; and in all the principal trigonometrical operations of which accounts have been published (excepting Colonel Everest's prolongation of the Indian arc of meridian, and some recent surveys in Germany), this condition (speaking generally) is the only one which has been attempted to be satisfied in computing the observations. But the observance of this single condition is not by any means sufficient to give the best representation of the *whole* of the observations; nor does it even suffice to give a determinate solution of the problem under consideration, for when the distance between two stations is computed through different series of triangles, each mode of computing leads to a different result. When the instrument has been set up at every station, and the angles between the other stations visible from that point have been all observed, other geometrical relations are established which the corrected angles ought likewise to satisfy, and angles are obtained which cannot be made use of otherwise than in satisfying such relations. Now, it is manifest that any mode of computing the triangles in which any observed angle is not taken account of, or any geometrical relation among the parts of the figure not satisfied, or which does not allow to every single observation its due influence, cannot be regarded as satisfactory. In order to obtain the nearest representation of the whole of the observations, or the result which is affected by the smallest probable error, it is necessary to solve the following problem, *viz.* to determine the corrections which must be applied to the observed angles in order that they may satisfy *all* the geometrical relations or equations of condition, and in order that the sum of the squares of the corrections may be an absolute minimum. A general solution of this problem was given by Gauss in his *Supplementum Theoriæ Combinationis*, &c. (Gottingen, 1828), and the method has been applied by Bessel to the triangulation for the measure of the meridional degree in Prussia, and also to the computation of the extension of the French meridian through Spain, from Montjouy to Formentera.

The triangulation which has been selected in the present case for an example of the method, includes ten stations (commencing with the base on Hounslow Heath), at which thirty-five independent angles were observed. For determining the corrections of those angles, nineteen equations of condition are furnished by the observations, among which are instances of all the kinds which can occur in a trigonometrical survey. The final results differ extremely little from those given in the *Survey*, the greatest difference in the length of any side amounting only to about half a

foot, and this in a distance of nearly eighteen miles. This close agreement must be attributed, however, to the smallness of the triangles, and the very great accuracy of the observations in this portion of the Ordnance Survey. If the distances between the stations had been two or three times greater, the observations would probably have been less exact, and the differences between the results of the two methods of computation more considerable; but however this may be, it is only by following the method here explained that the whole of the precision which is attained by the observations is preserved in the results; and for this reason the method should be adopted in all important surveys, particularly in those for determining the curvature of the earth. Besides giving a determinate result, and that result the one which is most probably nearest the truth, it has the great advantage of superseding all arbitrary corrections, and admitting only such as are rigorously deduced from the observations.

The methods of deducing the most probable values of the angles from the observations, of assigning the weights, of forming the equations of condition, and all the steps of the process to the final determination of the corrections by which the equations are satisfied, are given at length.

V. The President announced a communication that he had received from the Rev. Baden Powell, relative to an easy and convenient method of imitating the appearance of the *corona*, or glory, that surrounds the body of the moon, during the time of total darkness, in total eclipses of the sun; and also the appearance of the *beads* that occur not only in total eclipses, just prior to the time of total darkness, but likewise in annular solar eclipses. A sketch of the method was exhibited, which is merely this: a candle is placed in the focus of a lens, fixed in a screen, with an aperture of about $\frac{1}{4}$ of an inch in diameter, on the opposite side of which screen is placed an opake circular disc, of equal (or even greater) diameter than the aperture, which may be placed at different distances, so as to produce an eclipse of any magnitude, as the spectator shifts his position. When it is central and total, there is a brilliant ring, or glory, even when it is so much nearer to the eye as to subtend a much greater angle than the aperture. Also, when there are any cusps, minute irregularities, on the edge of the disc, produce distinct *beads*. Professor Powell has tried a similar experiment with the circular opake disc and the rays of the *sun* reflected from a small piece of glass, which produced a most brilliant ring, the disc being nearly double the apparent diameter of the sun: and he proposes to pursue the inquiry still farther.

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ROYAL ASTRONOMICAL SOCIETY.

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Francis Baily, Esq., President, in the Chair.

William Gravatt, Esq. F.R.S. was balloted for, and duly elected a Fellow of the Society.

The following communications were read:

I. Extract (translated) of a letter from M. Bessel to Professor Henderson, dated November 14, 1842. Communicated by Professor Henderson.

"We have just now commenced here the observations of another star which excites our hopes of a sensible parallax. It is No. 1830 of Groombridge's Catalogue; in regard to which star Argelander has found that it possesses a greater proper motion even than 61 *Cygni*. The star is not so advantageously placed as the latter. Besides, it is not a double star, and therefore not so well adapted for heliometric observations; but the two neighbouring stars with which we compare it have a highly favourable position, since they are on opposite sides and equally distant from it. We must only wait to see how far the observations will succeed, and whether the hopes of the parallax, sensibly surpassing the errors of observation, will be justified. If you should add any thing new to your results of α *Centauri* you would give me much pleasure by communicating it.

"I have been much occupied with my Circle of Repsold, that is to say, with the examination of its divisions. I have here met with a difficulty before unknown to me, namely, the great influence of the inequality of the temperature in the interior of the observatory, on the figure of the circle, and also on the magnitude of the interval between its divisions. When I observe a diameter with two opposite microscopes, and then, turning the circle through 180°, repeat the observation, I obtain thereby the distance of this diameter from the axis of rotation of the instrument. This distance ought to be unalterable; but, after some time, I frequently find a difference amounting to a second of space, which can only arise from an irregular influence of the temperature. Hence it follows that a great number of repetitions of the estimation of the errors of the divisions of any part of the circle are necessary, before we can place that confidence in the mean result, which must be attained, f the estimation is to be *indeed* useful. I imagine that this effect of temperature has an influence also on your circle, and perhaps to a still greater extent, and I should read with considerable interest a communication from you on this subject."

II. Letter from Professor Henderson to the Secretary, dated March 23, 1843, on the Great Comet of 1843.

The object of this letter was chiefly to fulfil a promise made in a letter from Professor Henderson to the Secretary, dated Nov. 1, 1842, to send to the Society the elements of the comet of 1668, which appeared to him worthy of more notice than it had received. Mr. Henderson cites the following works relatively to this comet: Newton's *Principia*, book iii. prop. 41; *Phil. Trans.* Nos. 35 and 150; and Pingré's *Cométographie*, vol. ii. p. 22.

The elements obtained by him from the best data which he could find are as follow:

Time of Perihelion Passage, 1668, Feb. 24·8, new style, Greenwich mean time.

Log. Perihelion Distance	9·3999
Perihelion Longitude	40° 9′
Long. of Node	193 26
Inclination	27 7

III. Elements of the Great Comet of 1843, with an Ephemeris, from the Observations of March 20, 22, and 25, reported in Professor Schumacher's Circular. By Professor Henderson.

Time of Perihelion Passage, Feb. 27·50, Greenwich mean time.

Log. Perihelion Distance.........	7·61348 (less than sun's semidiameter)
Longitude of Perihelion	279° 29′
——— Ascending Node	359· 43
Inclination	35 58

Motion retrograde.

Ephemeris for 9ʰ *Greenwich mean time.*

Day.	R.A.	Declination.	Distance from Sun.	Distance from Earth.
1843.	h m s	° ′		
April 5	4 22 44	−5 29	1·22	1·52
6	4 26 8	−5 19	1·24	1·55
7	4 29 28	−5 9	1·26	1·58
8	4 32 44	−4 59	1·28	1·61
9	4 35 52	−4 50	1·30	1·64
10	4 38 52	−4 40	1·33	1·67
11	4 41 48	−4 31	1·35	1·70
12	4 44 44	−4 23	1·37	1·73
13	4 47 32	−4 15	1·39	1·76
14	4 50 20	−4 7	1·41	1·79

IV. Letter from Lieutenant G. B. G. Downes, Royal Engineers, to Professor Henderson. Communicated by Professor Henderson.

"*Barbadoes, West Indies, March 7th*, 1843.

"My dear Sir,—I wish to take the opportunity of sending you by this mail a short account of the remarkable phenomenon, now known to be a comet, which has been visible in this latitude for

about a week past. It was first seen by me on the second instant soon after sunset, as a light rising only a few degrees above the horizon, and bearing little south of the true west. Some parties in the island, however, state that they saw a similar appearance for the first time on the 28th ultimo. For the first few evenings various were the conjectures as to what it might be. Lunar rainbows, volcanic eruptions at some distant island, being called into play to account for it. On the 3d instant, the view of it was similar, excepting that the arc occupied by the light was much increased, and it issued from a cloud low in the horizon. On the 4th instant, about half an hour before sunset the light again appeared quite clear of the horizon, and was evidently a comet of large magnitude, the nucleus being distinctly visible to the naked eye. The nucleus subsequently became buried in a cloud; and a similar cause prevented any accurate observations of it being made on the following evening, viz. the 5th instant. Last night, the 6th instant, it appeared to great advantage, not only the nucleus being visible to the naked eye, but even the contraction and elongation of the tail could be detected. I noted the time of sunset as exactly six o'clock, the sun bearing about 9° south of west. At half past six I took the bearing of the nucleus as 18° south of west; and at $7^h\ 25^m$ the nucleus set at a bearing of 20° south of west. The arc subtended by the tail varied in proportion to its contraction and elongation, which I have before referred to;—it certainly never occupied a less space than 30°, and increased to about 35°. From the brightness of the moon, and its proximity to the comet, it was difficult to trace the exact position of the nucleus. I have no doubt, however, that yesterday it was situated in *Pisces*, the tail extending to the constellation of *Aries*. It is evidently receding from the sun, with which its recent path must almost have been in conjunction. As this comet, from its situation, will, of course, be equally visible in England, this memorandum can only be useful as noting its first appearance in this latitude. As it has not been predicted I am most anxious to hear of the observations made at our observatories at home. You will already have heard of the calamitous earthquake that has visited some of the islands in the West Indies. Of course, there will not be persons wanting to connect the sudden appearance of the comet with it. There is no apparent change of temperature, the thermometer in the shade at three o'clock in the day being about 80°, at which hour the comet is not many degrees from our zenith. It cannot be detected by the naked eye, or by any telescopes we have here, until about an hour before sunset. I close this at the last moment for the English packet, and in great haste believe me," &c. &c.

V. Letter from Professor Henderson to the Secretary, on the Great Comet of 1843.

Edinburgh, April 10, 1843.

My dear Sir,—There is considerable probability for the supposition, which I see has been taken up in Professor Schumacher's second

circular, that the late comet is the same as the one which appeared in 1668. I am in possession of a map representing the apparent track of the latter in the heavens as seen at Goa, in the East Indies, from the 9th to the 21st of March. It is entitled "Observationes Goæ habitæ circa Phœnomenum cœleste quod apparuit mense Martis, Anno 1668, Romam missæ ad P. Ægidium Franciscum de Gottignies in Collegio Romano Matheseos Professorem;" and it seems to have formed part of the work entitled "Ægidius Franc. de Gottignies de figuris Cometarum qui an. 1664, 1665, 1668 apparuerunt, cum animadversionibus et tabulis in æs incisis Romæ 1668," mentioned by Weidler (*Historia Astronomiæ*, p. 508), and La Lande (*Bibliographie Astronomique*, p. 271). This work may contain other information about the comet, and may probably be found in libraries. The map is sufficiently rough, and I find that the relative positions of the stars are sometimes a degree out; nevertheless, by comparing the places of the termination of the tail nearest the sun, which I suppose denote the nucleus, with those of the neighbouring stars, I have obtained places of the comet as in the subjoined table, which also contains places calculated for the orbit of the late comet from the following assumed elements:

Perihelion Passage 1668, Feb. 28·8, new style, Greenwich Time.

Longitude of Perihelion	277° 2′
Longitude of Ascending Node	357 17
Inclination of Orbit	35 58
Logarithm of Perihelion Distance	7·68000
Motion retrograde.	

The Time of Observation on Each Day assumed to be 2^h *Mean Time at Greenwich.*

Date, New Style.	Place by Map.		Place Computed.		Difference.	
	R.A.	Declination.	R.A.	Declination.	R.A.	Declin.
1668. March 9	18° 49′	− 10° 50′	16° 46′	− 10° 16′	+ 123′	− 34′
10	20 51	10 7	19 59	10 10	+ 52	+ 3
11	23 8	9 23	23 1	10 2	+ 7	+ 39
12	25 10	8 59	25 53	9 52	− 43	+ 57
13	27 53	8 37	28 36	9 42	− 43	+ 65
14	30 22	8 23	31 11	9 28	− 49	+ 65
15	32 52	8 39	33 38	9 15	− 46	+ 36
16	34 45	8 22	35 58	9 1	− 73	+ 39
17	37 57	8 15	38 10	8 46	− 13	+ 31
18	41 26	8 39	40 15	8 30	+ 71	− 9
19	45 8	8 49	42 12	8 14	+ 176	− 35
20	48 34	8 50	44 3	7 57	+ 271	− 53
21	52 8	− 8 41	45 47	− 7 40	+ 381	− 61

"On inspecting the daily differences of the places by map, it is evident that they are affected with considerable errors; and although it appears that they be within such distances of the places in the assumed orbit as to warrant a presumption that the comets are the same, and although these distances may be diminished by a better determination of places from the map, and a better assumption of elements, yet the identity cannot be held to be conclusively ascertained; for the observed places are in that situation when the problem of determining a comet's orbit acquires an indeterminate character, and when the results obtained from any mode of solution are greatly affected by errors of observation. In fact, as I formerly mentioned to you, I obtained from these observations an orbit so widely different from the one I have just assumed, as not to bear the least resemblance to it, and yet the places in the former orbit agree better with the places in the map than those computed in the latter orbit.

"The later places of the comet, from the map, may have been much influenced by the decreasing light of the nucleus, and the increasing moonlight. The new moon was on March 12. The places from the map may belong to the apparent termination of the tail, the nucleus and adjoining part of the tail having become invisible, and on this supposition the agreement may be much better."

VI. Letter from T. Forster, Esq., dated Bruges, March 22, 1843, on the Great Comet.

Mr. Forster first saw the train of the comet on March 18, at about eight o'clock, while making a sweep of the western part of the heavens. It appeared as a broad band of bright white light, of about 6° in breadth, commencing from the horizon just where it is cut by the equator, and including the star γ *Ceti* just within its northern boundary-line. It passed through the constellation *Lepus*, including in its light the four stars of the *Hare's Ears*, where it was 3° in breadth, and finally became evanescent between *Sirius* and β *Canis Majoris*. From this direction it appears that it did not lie in a straight line; but this appearance, the author thinks, may be due to the effect of difference of refraction.

On March 20, when the train was again seen, it was accompanied, from $8^h\ 7^m$ to $8^h\ 25^m$, by a very sudden and bright aurora borealis, darting upwards from the base of the train, and illuminating the sky to beyond the *Bull's Horns*, and finishing by a wide diffusion of yellow light.

VII. Second Letter from T. Forster, Esq., dated Bruges, March 28, 1843, on the Great Comet.

In this letter the author desires to correct an error in his letter of the 22d of March: "The band of light described as issuing from the horizon at γ *Ceti*, and being 16° * broad at its base, and 3° at

* In the letter of March 22, it is mentioned as being 6° in breadth, probably through an error of transcribing.

its vertex, was not the light suspected to be a comet's tail, but a luminous envelope, whose axis exactly coincided with it, and which, as it were, sheathed it on the 20th inst. This phenomenon was not repeated on the 21st, since which the train of light issuing from the horizon a little north-west of γ *Eridani* has not exceeded 1° 15′ in breadth, and extends to α *Leporis*. But on Sunday night it was again traceable, though very faintly, over β *Canis Majoris*, nearly to above *Sirius*.

"On Sunday it was again accompanied by a sort of aurora borealis, consisting of a broad cone of whitish light, whose axis, if we may so speak, would have cut the equator at an acute angle, between *Menkar* and δ *Ceti*. Its superstructure extended upwards as before, and involved the *Pleiades*, where it became suffused and mixed with the surrounding twilight."

VIII. Letter from James Nasmyth, Esq., to Sir J. F. W. Herschel, on an early Observation of the Train of the Great Comet. Communicated by Sir J. F. W. Herschel, Bart.

Patricroft, near Manchester,
March 28, 1843.

"Sir,—I sincerely trust you will pardon this intrusion on your attention, as I wish to communicate to you what occurs to me may be in some slight degree interesting to you, in regard to an observation I made about eight o'clock in the evening of Tuesday, the 7th of March, at which time, while looking at the moon with a 7½-inch Newtonian reflector, I observed near it and the *Pleiades, a well defined ray of light*, which I could trace along distinctly with the telescope for several degrees. The position of the stream of light I have endeavoured to convey to you by the inclosed rude sketch: it was well defined on one edge, and had a slow motion upwards, as seen by the inverted position due to the telescope. I was enabled to observe its motion by the edge of the stream of light being near to one of the stars of the *Pleiades* when I observed it first, which distance was considerably altered and increased when I observed it about an hour afterwards.

"It occurs to me, that the fact of my having seen this stream of faint light at that time, and in that position, may derive some additional interest in reference to the comet which you have been lately engaged in observing, and as the date (7th March) *and the position* is pretty clearly given by my observation, it has occurred to me that the fact may claim to be interesting to you.

"Two days afterwards, I communicated the observation I had made of this stream of light to my friend Mr. Lassels of Liverpool, who, not being able to account for it, was induced to suppose it to be due to some temporary derangement of my telescope; but this was by no means the case, in so much as the stream of light in question held its due position in reference to the moon and stars near to it, whatever alteration I made in the position of the telescope. I could with the greatest distinctness trace along its outline for

several degrees upwards from the horizon, but the light of the moon rendered it invisible on the side next that of the then first quarter moon: it was a *position* object in the telescope.—I am, &c.

"JAMES NASMYTH.

"The angle formed between the edge of the stream of light and the line of the horns of the moon was, as near as I could judge, about 120°. When the stream of light was viewed at some distance from the moon, say ten diameters or so, it was so distinct as to divide the field very plainly, occupying any portion due to the position of the telescope, but always with the same inclination with respect to the horizon.

"The motion, with respect to the increasing distance between the edge of the stream of light and the star nearest, was sufficiently rapid to render the space between very evidently increased in the interval of one hour.

"Telescope, 7 feet 6 inches focal length, Newtonian: power employed, about 70."

IX. Letter from M. C. L. Littrow, Director of the Observatory of Vienna, on the Great Comet. Communicated by T. Galloway, Esq.

"My Dear Sir,—From the following observations, made at the observatory of Vienna:—

	Mean Time of Vienna.	R. A.	Dec.
	h m s	° ′ ″	° ′ ″
March 18,	7 49 11	42 15 0	−9 59 30
— 21,	7 28 27	47 18 45	−8 56 19
— 23,	7 42 30	50 33 45	−8 23 54

"I have deduced the elements of the new comet's orbit as follows:—

Passage through its Perihelion Feb. 27·855. mean time Vienna.

Longitude of Perihelion	186° 38′
Longitude of the Ascending Node	352 5
Inclination	40 53
Log. of the Shortest Distance from the Sun	9.76094
Motion retrograde.	

As it appears from these elements, the comet has no striking resemblance with any former one. In regard to its geocentric path, it is one of the most remarkable bodies we have observed till now. It was in the constellation of Antinous in the last days of January, at a distance of only 31,000,000 of German miles, and was therefore visible in the oriental heavens; perhaps the astronomers of the austral hemisphere, the attention of whom was excited by the comet of Laugier, had observed it at that time. Even at its passage through the perihelion (Feb. 28), it was visible in Aquarius. During the first days of March, it was lost in the rays of the sun, and began at last to be visible for southern observers on the 5th of March. As it stands on this day between us and the sun, at a distance of

only 8,000,000 of German miles from the earth, and as its train is at least 12,000,000 of German miles long, and 4,000,000 broad, it is not impossible, that we have passed this train without perceiving it, and that for a short time it had been possible to see the comet before the sun. As my elements are not yet exact, I cannot more nearly examine these conjectures. It passed the plane of the ecliptic in a point, situated between the orbits of Mercury and Venus, but rather far from these two planets. From the 5th of March it removes at the same time from the sun and the earth, and therefore decreases so much in its light. I believe the whole visibility will not be longer than till the first days of May, till when it does not step over the limits of the constellation Eridanus. For the naked eye it probably ceases to be visible on the 1st of April, as the moon will prevent from this day its appearance.

I have the honour to be, Sir,
&c. &c. &c.
CH. L. LITTROW,

Vienna, 1*st April*, 1843.

X. Circular letter from Professor Schumacher, dated March 26, 1843, on the Great Comet.

In this letter notices are given of the observations of Mr. Cooper and Sir John Herschel, which have been already sufficiently published in England. A letter was received from M. Humboldt, dated March 22, announcing that M. Galle had found the head of the comet in the twilight on the 20th. At eight o'clock it was $1\frac{1}{3}^{\circ}$ west and 1′ north of ζ Eridani. M. Galle estimated the length of the tail at about 40°.

At Altona, two single observations were made just before the comet disappeared behind the houses, but which, from the circumstances under which they were made, are of necessity not very accurate. They are for Altona mean time:

Mar. 24	$8^h\ 20^m$	R. A. $52^{\circ}\ 7'$	Decl. $-8^{\circ}\ 5'$
25	8 12	53 30·4	−7 52

In a postscript, dated March 28, it is announced that M. Littrow had made at Vienna a not altogether satisfactory observation of the comet on the 18th of March, at $7^h\ 49^m\ 11^s$, Vienna mean time.

R. A. $2^h\ 49^m\ 0^s$. Decl. $-9^{\circ}\ 59'\ 30''$.

M. Galle, at the Observatory of Berlin, announced on the 25th of March, that he had computed the following elements from his observations of March 20, 21 and 22,

T	1843, Feb. 27·4567	
Log. q	8·053966	
π	$274^{\circ}\ 30'\ 4''9$	From the mean
☊	357 43 25·2	Equinox of Mar. 0.
i	36 22 19.8	

Motion retrograde.

The observations of the comet, still affected with aberration and parallax, are, for 8^h Berlin mean time,

Mar. 20	R. A. = 45° 42′ 30″	Decl. = − 9° 13′ 40″
21	47 25 30	8 56 40
22	49 3 27·5	8 40 0
24	52 4 58·7	8 7 27·6

The above elements give for the error of the mean of these observations in geocentric latitude and longitude,

$$+ 11''\cdot 4 \text{ and } + 5''\cdot 1,$$

and the rectangular co-ordinates reckoned on the equator from the mean equinox of March 0, are

$$\begin{aligned} x &= r\,[9\cdot999880]\,\sin\,(175^\circ\ 3'\ 19''\cdot 8 + v) \\ y &= r\,[9\cdot988921]\,\sin\,(265\ 21\ 51\cdot 7 + v) \\ z &= r\,[9\cdot350754]\,\sin\,(\ 79\ 10\ 44\cdot 5 + v) \end{aligned}$$

M. Rümcker had observed the comet at the Observatory of Hamburg, on March 25, at $8^h\ 32^m$ mean time.

R. A. = 53° 34′ 34″ Decl. = − 7° 52′ 29″.

XI. Second circular letter from Professor Schumacher, dated March 31, 1843, on the Great Comet.

On the 23d of March, the following observations and elements had been furnished by M. Plantamour, Director of the Observatory of Geneva.

For Geneva mean time:

Mar. 18	$7^h\ 34^m\ 38^s$	R. A. = $2^h\ 47^m\ 57^s\cdot 18$	Decl. = − 9° 47′ 52″
19	7 33 33	2 55 35·46	9 30 47
21	7 27 30	3 9 41·30	8 56 50

From these observations he computed the following elements:

T	1843, Feb. 27·4882
q	0·0045
π	279° 12′ 11″
☊	359 53 21
i	36 0 27

Motion retrograde.

From these elements the comet must have been nearer to the sun than any one which is known to us, nearer even than the comet of 1680.

Mr. Cooper believes the present comet to be identical with the comets of 1668 and 1702. But if this were the case, it must, on the 27th of February of the present year, have completed its fourth revolution since the year 1702, and this would give for its period 35¼ years, instead of 34 years, which is the interval between 1668 and 1702. It also does not seem easy to account for its not having been seen at its intermediate reappearances. It appears, however, worth while to try whether it is probable that it is identical with the

comet of 1702, an account of the observations of which by Maraldi, on the 2nd of March, appears in the Memoirs of the Academy of Sciences for 1702. To determine this more accurately, M. Peterson, using Galle's elements, computed the place of the comet for March 2, 1702. He reduced the perihelion and node to 1702, and found, with different assumptions for the time of the passage through the perihelion (for which he assumed successively Feb. 19·5, Feb. 21, Feb. 21·5, Feb. 23, and Feb. 26), the place of the comet.

	Feb. 19·5.	Feb. 21.	Feb. 21·5.	Feb. 23.	Feb. 26.
Long.	6° 18′	3° 12′	2° 3′	358° 50′	351° 48′
Lat.	− 20 3	− 17 55	− 17 6	− 14 48	− 9 12

None of these hypotheses satisfy the position of the train given for the 2nd of March, when we consider that it must pass through the sun.

The discussion of an observation by Martin Brouwer, which is found in the Introduction to the Universal Geography of Struyck, seems also to shew that it is not identical with the comet of 1702.

With respect to the comet of 1668. Cassini has traced on a map of the stars the position of the train for three days, March 10, 14, and 19. For these days Galle's elements give (assuming the perihelion passage to be on Feb. 27·5) the following places of the comet.

Mar. 10·5	Long. = 19° 36′	Lat. − 20° 4′	Sun's long. = 351° 11′
14.5	29 51	22 46	355 10
19·5	40 16	24 38	0 8

If we trace on the map these places of the comet and the sun, the situation of the train agrees, as nearly as we can determine from this map, with the place of the comet.

Pingré, in his *Cométographie*, Part II., p. 22, mentions an observation of this comet made by Father Valentin Estancel, in St. Salvador, on the 7th of March. The head of the comet is a little beneath, and on one side of θ *Ceti*, the end of the train was in contact with ζ *Ceti*. It is not mentioned on which side of θ *Ceti* the comet was, but the data for the position of the train shew that it was to the left of the observer, or towards the south. Calculation gives

Long. of Comet 10° 39′
Lat. of Comet − 16 59

which is within the limits of the accuracy of the observation.

According to these calculations it is very possible that our comet is identical with that of 1668, which would give a period of revolution of 175 years. We find in the *Cométographie* of Pingré, vague notices of comets for 1493, 1317, 1143, 968, 442, and 268.

In a postscript, the following observations are announced as having been received from Rome:

Mar. 18	7h 30m 50s·5	R.A. = 2h 47m 47s	Decl. − 9° 50′ 55″·4
19	7 58 35·8	2 55 37	− 9 30 43·7
20	7 38 44·5	3 2 44	− 9 13 6·1

Also in a letter of the 26th of March, from Professor Encke, the following observations, made at his observatory, and reduced for convenience to 8^h Berlin mean time were contained.

	R. A.	Decl.
Mar. 20	45° 42′ 30″·0	− 9° 13′ 40″·0
21	47 25 30·0	8 56 40·0
22	49 3 27·5	8 39 59·9
24	52 4 58·7	8 7 27·6
25	53 29 17·1	7 51 46·6
26	54 49 33·0	7 36 27·6
27	56 6 20·6	7 21 25·3
28	57 19 47·3	7 7 4·4

Professor Encke deduced the orbit from the observations of the 20th, 24th, and 28th of March, taking account of the small corrections. These gave the following elements:

T	1843, Feb. 27·40162	
Log. q	7·482318	
ϖ	281° 21′ 19″·9	From the mean
☊	5 51 7·7	Equinox of Mar. 0.
i	35 0 34·0	

Motion retrograde.

$$x = r\,[9{\cdot}999256]\,\sin\,(v + 169^\circ\ 41'\ 52''{\cdot}4)$$
$$y = r\,[9{\cdot}991363]\,\sin\,(v + 259\ \ 1\ 17{\cdot}6)$$
$$z = r\,[9{\cdot}313776]\,\sin\,(v + \ 95\ 51\ 47{\cdot}8)$$

XII. The President announced that he had received notices of the comet from the Islands of St. Vincent's and St. Christopher's.

In a letter from St. Vincent's, dated March 5, the writer states that it had been seen for the previous three days, being very brilliant, of a pale colour, and with the nucleus very distinct.

At St. Christopher's the comet was seen first on the 2nd of March, but then only as a streak of light, and not visible much above the horizon. It soon became quite distinct, but on the 8th it appeared to be fast receding. It appears to have caused considerable consternation in the island.

XIII. A Memoir on Astronomical Drawing. By Piazzi Smyth, Esq. Communicated by Capt. W. H. Smyth, R. N.

This paper was partly read, and will be concluded at the next meeting.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *May* 12, 1843. No. 31.

Francis Baily, Esq., President, in the Chair.

Joseph Bateman, Esq. LL.D. was balloted for, and duly elected a Fellow of the Society.

The following communications were read:

I. A Memoir on Astronomical Drawing. By Piazzi Smyth, Esq. Communicated by Captain W. H. Smyth, R.N.

The reading of this paper, which was begun at the preceding evening meeting, was now concluded.

The author's attention had been drawn to this subject on account of the small degree of notice, compared with its importance, which it has received from modern astronomers; 2000 years having elapsed since the commencement of the first catalogue of stars by Hipparchus, yet scarcely 100 years since any attempts have been made to hand down to future ages those signs and appearances in the heavens which admit of no direct application of measure or of number.

The recent art of photography having put it into the power of persons provided with clock-moved equatoreals to obtain infallibly correct delineations of the solar spots and of the surface of the moon, the author omits the consideration of these, and confines himself especially to the consideration of nebulæ and similar telescopic objects.

After a minute description of the different classes of nebulous objects which are known to us, the author enlarges upon the importance of diligently observing them with a view to noting whatever changes occur in each individually, and also of those extensive surveys of all that are visible to us, which have been so laboriously executed by the two Herschels. To procure evidence of their changes from one state to another was mainly the object of Sir J. Herschel's paper, read before the Society in 1825 (the recommendations of which have never been followed up), in which he attributes the want of good and trustworthy drawings of them to the extreme difficulty of drawing and engraving them, and to the fact that astronomers are too seldom draughtsmen.

The author then proceeds with the description of astronomical drawing, which may generally be divided into two large classes:— the positive, or that in which the shade is represented by blackness,

and the negative, or that in which the lights are the darkest parts of the picture. As specimens of the first class, he instances Messier's representation of the nebula of *Orion* in the *Memoirs* of the French Academy for 1774. This is a line engraving, and the principal fault is, that the brightest part of the nebula has been made of the utmost whiteness that the paper will allow, so that, to represent the still brighter stars, he has been obliged to make use of black figures Another and far better specimen of this class is Sir W. Herschel's fine collection of nebulæ in the *Phil. Trans.* for 1811; and the latest and best is that in Sir J. Herschel's Astronomy in the *Cabinet Cyclopædia.*

Of the negative class of drawings he instances Sir W. Herschel's clusters and nebulous stars in the *Phil. Trans.* for 1814; Sir J. Herschel's nebula in *Orion* from the *Memoirs* of the Royal Astronomical Society for 1825; and Mr. Cooper's view of Halley's Comet in 1835. In the above specifications the peculiarities and the imperfections of each are pointed out.

One of the great objects to be attained in astronomical drawing is the absolute fidelity of the details, and in this it differs materially from ordinary drawing from nature, where the accuracy of the *general* resemblance is the great point to be aimed at.

The most common defect in drawings of nebulæ is an unnatural brightness, arising from a neglect of what is technically called *chiaroscuro*, the whiteness of the paper having been taken for the whitest parts of all classes of objects; and deviations from this alone having been marked, to the exclusion of all that local shade which gives the tone to the picture. This, in the old engravers, was owing to the defects of the art as it then existed; but at present it is too frequently aimed at under an erroneous idea of producing a splendid figure; and the author in concluding this part of his subject again observes, that nothing can atone for want of fidelity in the representation which is thus made to suffer, and that the further desiderata appear to be: 1st, a positive representation in which lights shall be represented naturally by lights; 2dly, the adoption of such measures for producing shade, as shall not be visible to the eye; and, lastly, an ability in the artist to engrave his own views.

For the original drawings, Indian ink and the brush are abundantly sufficient, a little practice giving the requisite facility, and the method admitting of the advantage of allowing alteration and correction to a certain extent.

The author then describes the processes of engraving by the aqua-tinta, line, and mezzo-tinto methods, in the first two of which he points out insuperable objections, when applied with reference to the present subject, and shews that mezzo-tinto is admirably adapted for it. He then gives an elaborate description of the latter process, and of the modifications of the method and the apparatus necessary in its application to astronomical drawing. He, in conclusion, observes on the rapidity with which astronomical drawings would increase in value in proportion as they

became more accurate. "It is easy to fancy how exceedingly valuable a series of perfect drawings of Halley's comet, at its various apparitions, would be; how exceedingly interesting to examine into, and have confidence in so doing, the indications of any of that which we may call a secular change in the appearance of this wonderful body, and to determine whether, in the course of its periodical changes, it followed the same laws in each of its perihelion passages. Again, the nebulæ offer a field of inexhaustible speculation and conjecture, in which, says Sir J. Herschel, 'it is easier to propound questions than to offer any probable reply to them.' But meanwhile appeal to fact, by the method of constant and diligent observation, is open to us; and, as the double stars have yielded to this style of questioning, and disclosed a series of relations of the most intelligible and interesting description, we may reasonably hope that the assiduous study of the nebulæ will ere long lead to some clearer understanding of their intimate nature."

II. On a Revision of the Boundaries of the Constellations, as usually drawn on Celestial Maps and Globes. By Francis Baily, Esq. President of the Society.

The advantage and importance of having the boundaries of the constellations of the stars distinctly and properly defined on our maps and globes, must be evident to every one that has occasion to refer to so useful and convenient an auxiliary to the practical astronomer: for unless due attention is paid to some clear and well-organised plan of arrangement, and to some regular method of drawing the lines that constitute the limits of the constellations, much confusion and intricacy soon enters into the system, and not only does the whole become an unintelligible mass of intersecting and undefinable boundaries, but the nomenclature of the catalogues also becomes sadly deranged. This is no ideal annoyance; for the present state of all our modern maps and globes bears evident proofs of the existence of the evil to which I have here alluded: and the catalogues likewise partake of this confusion. But the time has arrived when this inconvenience, now become so troublesome and perplexing, can be no longer tolerated. The proposed extension of the catalogue of this Society, now about to be sent to the press, and in which there are a number of additional stars selected from various works, differing very essentially in the nomenclature of the stars which they contain, requires that every star thus introduced should be compared with maps in which the boundaries of the constellations are constructed and drawn upon some definite and systematic plan, so that the name of the constellation to which the star may be thus found to belong, should be correctly affixed thereto, and thus shew at once its true and accurate locality in the heavens. This can only be done by a general revision of the whole system.

Ptolemy drew his *figures* on the globe in such a manner that the stars should occupy the positions that he has designated in the

descriptions of them in his catalogue: and the boundary of each figure thus drawn was, in fact, the limit of the *constellation* intended to be represented. For, when he observed any stars that were beyond the outline of his figures, he denominated them *αμορφωτοι*, *unformed;* and this method was long followed by his successors. But in the time of Tycho Brahé this plan was in some measure departed from, and a more comprehensive extension of the original limits adopted, by including the unformed stars within the boundaries of one or other of the contiguous constellations: so that all the constellations abutted against one another, and the whole of the heavens was thus occupied by one portion or another of some known constellation, the *figures* remaining the same. When Hevelius formed his catalogue, he observed many stars, in the large spaces between Ptolemy's figures, that had not been previously noticed; and in these spaces he introduced new figures, or constellations, many of which are retained even to the present day. But, the greatest innovator on this system was Bode, who although no great observer himself has, in his Catalogue and in his Maps, filled the heavens with a host of new figures and constellations that were by no means requisite, and that tend only to annoy and confuse, without presenting one single advantage.

In these remarks I have reference only to the constellations in the northern hemisphere; or, at least, to those constellations only that are visible in the northern latitudes, which, of course, include many of the southern stars. When the Southern Ocean however was visited by European navigators in the sixteenth century, a map of the portion of the heavens, there visible and not hitherto described, became requisite and was soon formed: but it was not till the time of Halley that any catalogue or map of the southern constellations could be depended upon. The constellations that were adopted or introduced on this occasion were in some measure altered and increased by Lacaille in the last century. But as this subject has been already discussed, and is still under consideration, by Sir John Herschel, I shall not further allude to it in this place, except as to those points that may be connected with the general view which I am about to take of the whole question.

When Hevelius formed his catalogue of stars, he at the same time constructed the map of the constellations, in which they were to be respectively placed. By this method he in some measure preserved an uniformity in his classifications and arrangements, and obviated any considerable distortion of the boundaries of the constellations, having himself defined the limits. But Flamsteed did not possess this advantage, since his maps were not constructed till long after his catalogue had been formed, and indeed not till many years after his decease: and as Hevelius's maps were not published till after Flamsteed had commenced his observations with the mural quadrant, the *Uranometria* of Bayer was the only authority to which he could refer even for an approximate classification of any new stars that he might observe. This however appears to have been often done without due consideration or

attention, and the name of a constellation was frequently written down, in the margin of the observation-book, as that which, *at the time of observation*, Flamsteed supposed to be the true constellation under review; but which afterwards, when the observations came to be reduced and arranged, have been found to be incorrect. An inspection of Flamsteed's manuscript books, at the Royal Observatory at Greenwich, and indeed the second volume of his *Historia Cœlestis*, will fully confirm this remark. The consequence has been that several of the stars in his catalogue have been inadvertently arranged and classed under erroneous constellations: and our modern map-makers, instead of correcting these obvious errors in due time, and in a proper manner, or of laying down any general principle on which the boundaries might be constructed and drawn in all cases of new discoveries, have suffered the evil not only to continue, but to increase to such a degree by subsequent innovations, that the celestial maps have at length become a system of derangement and confusion. For, a practice seems to have been adopted that whenever a modern astronomer has, in his catalogue, inadvertently introduced a star which he has designated by an erroneous constellation, the map-maker, or globe-maker, immediately extends the circuit of the constellation so as to embrace it within its limits; although in so doing he causes the most inconvenient and absurd distortion of the boundary lines, and, in some cases, actually includes thereby stars that ought not to have been disturbed; which consequently renders the map, or the globe, a mass of confusion and intricacy, and totally unfit for accurate reference. The inspection of any modern celestial map or globe will fully confirm this remark.

Before a catalogue of any considerable extent, containing new stars, is finally arranged as to its nomenclature, a map of the constellations, or at least their general outlines or boundaries, ought to be laid down upon some uniform and acknowledged system. The plan which was pursued by Ptolemy, and which with some slight alterations has been continued down to the present time, may serve as a basis for any modern improvements. Its antiquity, and the numerous references which have always been, and still are, constantly, made to it, render it now difficult (even if it were desirable) to make any considerable deviation from a system which is associated with so many scientific, historical, and mythological recollections. But whatever plan be adopted, it ought to be preserved with some degree of uniformity and regularity: so that if an author has inadvertently designated a star by a wrong constellation, the name in the catalogue should be amended, rather than the boundary of the constellation distorted. This however will occasionally admit of some laxity; for if such star should happen to be near the confines of a constellation, a *slight* variation in the curvature of the boundary may be justly allowed in the case of a well-recognised star, more especially as the precise limits are in some measure arbitrary. But where a star in any catalogue is designated by the name or title of a constellation, to which it

manifestly does not belong, and has been recorded and arranged as one of the stars in such constellation, the only proper mode of correcting the error is to alter its name and character in the catalogue, and thus restore it to its proper designation and position.

As an example of the confusion which is created by such misnomers, I need only adduce the case of two stars in Flamsteed's catalogue; one of these is called 44 *Lyncis*, but whose position is in the middle of *Ursa Major*, and was so located by Ptolemy; and the other is 19 *Ursæ Majoris*, whose position is in the middle of *Lynx*. Now the map-maker, in order to comprise these stars within the limits of the constellations in which Flamsteed has thus inadvertently and erroneously placed them, has extended the boundaries of each of these constellations in such a confused and intersecting manner that the limits are scarcely intelligible. The proper mode would have been to alter the nomenclature, at once, in the catalogue; and thus prevent the perpetuity of the error. Another example occurs in the star 13 *Argus* in Flamsteed's catalogue; which star is in fact situate in the constellation *Canis Minor*, that lies to the north of the intermediate constellation *Monoceros*: and the map-maker, in order to include this distant star within the limits of *Argo*, has in a similar manner traced a double line directly through the body of *Monoceros*, which thus appears like two distinct constellations. Many other examples of similar distortions might be adduced, but it is needless to multiply proofs of such evident absurdities, which need only be seen to be duly estimated and repudiated.

Cases of another kind occur where the constellation is improperly and unnecessarily extended, although there may not be any intersection of the boundary lines: such as that which may be seen in Flamsteed's catalogue of stars in the constellation *Cratera*, where many of the stars there introduced do not fall within the limits of the figure drawn by Bayer; nor is Flamsteed's extension of the boundaries warranted by Ptolemy's description of the position of the stars in that constellation.

Much confusion has also arisen from inattention to a regular classification and arrangement of certain clusters of stars that lie near the adjoining confines of two contiguous constellations; such as the cluster of stars about the head of *Serpens*, which are strangely intermixed with the stars that are considered to be in the arm of *Hercules*: and many similar cases may be seen in *Monoceros* and *Hydra*, *Draco* and *Cepheus*, *Auriga* and *Camelopardus*, *Libra* and *Hydra*, *Hercules* and *Ophiuchus*, *Vulpecula* and *Cygnus*, &c.

But the most striking proof of the inattention of map and globe-makers, to accuracy of arrangement, occurs in the cases where the author of the catalogue has placed the same star in two distinct constellations, and where unfortunately (in constructing the map) the erroneous one has been selected for its location. A singular case of this kind occurs with Flamsteed's 25 and 27 *Aquarii*, which are the *same* stars as 6 and 11 *Pegasi*. The map-maker has cor-

rectly placed the stars in the head of *Aquarius*, and then, as if doubtful of such a step, or desirous of preserving the double interpretation, extends the boundary line of *Pegasus* so as to embrace it within the limits of that constellation.

Cases of such double insertions in a catalogue are not to be wondered at in the early state of the science, where minute accuracy was not always attainable, nor the error always discoverable on account of the mode of classification; and we accordingly meet with a few of such cases in the catalogues of Ptolemy and others. But in more modern times the error has arisen principally, if not solely, from the method of arranging the stars, in a catalogue, under distinct and separate constellations, whereby the similarity of position is not readily discovered; and this will account for the synonyms that occur in the catalogues of Flamsteed and Hevelius: but when discovered they ought to be at once corrected, and not suffered to remain a perpetual blot in the catalogue. The modern mode, however, of arranging the *whole* of the stars in a catalogue, according to the order of their right ascension, without any regard to the constellation in which they may be placed, prevents the occurrence of a similar inconvenience in future.

But a similar source of error arises, and frequently causes doubt and difficulty to the map-maker, when the authors of two different catalogues vary in their decision as to the constellation in which a star should be located. Numerous instances of this kind may be seen in comparing the catalogues of Hevelius and Flamsteed, or either of these with the catalogues of Piazzi or Taylor: which confusion has arisen from a want of a system of well-defined and acknowledged boundaries to the respective constellations, whereby the astronomer may know when he is correct in locating the observed stars. Let any one examine the stars in Hevelius's constellation *Andromeda*, and he will find that Flamsteed has placed some of them in *Pegasus*, one in *Perseus*, and one in *Lacerta*: whilst Piazza places one of them in *Cassiopeia*. Those only who have to make frequent references to the smaller class of stars, and are desirous of identifying them, and of comparing the results of different observers, can justly appreciate the labour and inconvenience that occurs from such a confused state of location. And with respect to the map-maker, it is a forlorn hope to expect from him any thing like regularity, uniformity, clearness or precision so long as he continues the present system of circumscribing every star with the boundary line of the constellations to which the author of the catalogue, in which it is found, considers it to belong.

On the maps published by the executors of Flamsteed, there are not any boundaries surrounding the figures that are there drawn: for all the stars in Flamsteed's catalogue are placed in their true positions, as given in the British Catalogue, without any boundary lines, and those who consult the maps are at liberty to draw the boundaries in such manner as they may think most proper. It is the catalogue which is in error, and not the maps: and it is very

probable that the editors were aware of this circumstance, having found out the mistake, when it was too late to mend it.

Bode appears to have been the first that drew boundary lines to the constellations; and in so doing, instead of correcting the catalogue and preserving an uniform system of drawing his lines in a simple and regular manner between contiguous constellations, whereby the contour was distorted as little as possible, he introduced the practice (above mentioned, and which has been implicitly followed by the English map and globe-makers) of *hooking* within such limits all the stars that Flamsteed or any subsequent astronomer had inadvertently designated by a wrong constellation; thus disfiguring and distorting the boundaries and rendering them very intricate, perplexing, and annoying. In his large set of celestial maps, however, which he published about twenty years afterwards, he became sensible of his error, and very prudently discontinued this absurd practice, and confined his boundaries to their proper restriction. But the English map and globe-makers, instead of following this laudable example, have not only continued the evil, but have carried the practice to such an enormous and ludicrous extent that the modern celestial charts and globes at the present day exhibit a complete mass of intersecting and conflicting lines, utterly subversive of the object and design of such a divisional arrangement of the heavens. Harding, in his celestial atlas, has avoided this confusion.

In order that our catalogues and our maps (or globes) should speak the same language, and that they should at the same time be clear and intelligible to those who consult them for the purpose of identifying the stars in the heavens, it is requisite that the nomenclature of the stars, or, in other words, the boundaries of the constellations, should be placed on a more uniform, regular, and well-defined plan: but, in making this necessary reform, regard must be had (especially in the northern hemisphere) to long-established names and authorities, which by their antiquity and constant use have acquired full possession of the public opinion and favour. Now, it fortunately happens that very material improvements may be made in the present mode of delineating the boundaries of these constellations, without encroaching at all on any of the ancient arrangements, and without much alteration in those of more modern date. All that is required will be the correction of some of those manifest errors which have been caused principally by following too closely and implicitly the arrangement and classification of the stars in the constellations in Flamsteed's catalogue; and which has opened the door to further encroachments by his successors.

I have alluded here to the correction of Flamsteed's catalogue only, not as being the only one, (or even the most discordant) that requires reform, since similar anomalies, and equal in amount, are to be found in the catalogues of Hevelius, Piazzi, Taylor, and perhaps some others; but because it is the only one in these latter days (if we except Hevelius's which is not frequently referred to) in which the stars are quoted and known by the numerical order and

position in which they stand in the respective constellations; those of other astronomers being always designated by the order of their right ascension. And as all our map and globe-makers fill up the boundaries of the constellations with Flamsteed's *numbers*, as they find them in his catalogue, whether properly located or not, it is requisite in the first instance, to place those stars in their proper positions. The method which I should propose for carrying this object into execution, would be as follows:

1. Ptolemy's constellations to be preserved, and to form the basis of the construction and arrangement of the constellations in the northern hemisphere.

2. Nine of the constellations adopted by Hevelius, to be retained; but no others to be introduced in the northern hemisphere. These nine constellations are *Camelopardus*, *Canes Venatici*, *Coma Berenices*, *Lacerta*, *Leo Minor*, *Lynx*, *Monoceros*, *Sextans*, and *Vulpecula*.

3. Ptolemy's figures to be attended to, so that the drawings (if any) should embrace all the stars mentioned by him, and in their true places. *Libra* perhaps may be an exception to this rule, as this constellation has been introduced instead of the Claws of *Scorpio* adopted by Ptolemy. There are also four stars in Ptolemy's catalogue that are common to two adjoining constellations: namely Flamsteed's 52 *Bootis*, which is common to *Hercules*; 112 *Tauri*, which is common to *Auriga*; 79 *Aquarii*, which is common to *Piscis Australis*; and 21 *Andromedæ*, which is common to *Pegasus*.

4. If Bayer or Flamsteed has introduced any star from another constellation that would distort the correct drawing, it must be named, in the catalogue, after the constellation into which it is correctly inserted, and its pseudonym must be discontinued. In other words, the catalogue must be corrected, but not the boundaries of the constellations distorted.

5. Bayer's and Flamsteed's errors being thus rectified, and the figures of the constellations introduced by Hevelius being properly drawn (if requisite) within the intermediate spaces, the boundaries of the constellations, thus decided on, should be carefully drawn and laid down agreeably to some systematic plan, which may thus serve as the perpetual limits of the constellations: and no distortion of the outlines or boundaries of any of these constellations, in the northern hemisphere, should be permitted in consequence of the mistakes of any subsequent astronomers in arranging their stars under improper divisions of the heavens.

6. As all Flamsteed's stars are designated by the numerical order in which they stand in the constellation, and as these numbers are in most cases well known and recognised, it is desirable to preserve his stars within the boundaries of their respective constellations, wherever it can be conveniently done. But in the case of synonymous stars (amounting to 22) this is evidently impossible; and there are also several other cases, which have been already alluded to, (amounting to about 70) where it is impracticable, consistently with the rules here proposed. These anomalous stars must

be corrected in the catalogue, and there located in their proper constellations.

7. As all the stars in the catalogue of Piazzi are designated and always quoted by their *number* in the *hour* of right ascension, and those of Taylor and others, by their *ordinal number*, it is not so requisite to pay *special* attention to inscribing such stars within the boundaries of the constellations to which they are assumed to belong; and which will frequently be found to be discordant. Still, if any of these stars lie near to the boundaries so assumed, a slight detour may be allowed in the drawing.

Such is the plan which I propose to adopt in the nomenclature of the stars in the northern constellations, which will form a large portion of the extensive catalogue now about to be sent to the press: and I have thought it proper thus to bring the subject before the meeting, in order that it may undergo due deliberation and investigation prior to its final adoption; for it is absolutely requisite that some effectual steps should now be taken to divest the subject of its present confusion and ambiguity.

III. Account of an Observation of the Solar Eclipse of the 7th July, 1842, made at Starfield Observatory, together with an Account of some Chronometrical Experiments made to determine the Longitude of that Observatory. By W. Lassell, Esq.

The morning of the 8th July (civil reckoning) before sunrise was gray and partially clear. During the time of the eclipse scarcely a cloud remained to interfere with the observation of the eclipse. The telescope used was the nine-feet equatoreal, described in the twelfth volume of the *Memoirs* of this Society, to which was attached a parallel wire micrometer. The observation of the time of the commencement of the eclipse was made very satisfactorily, though the limb of the sun was serrated by the undulations of the atmosphere, the eye having been directed exactly to the point when the contact took place. The observed time of the beginning was $23^h\ 49^m\ 19^s{\cdot}1$ sidereal, or $16^h\ 46^m\ 21^s{\cdot}8$ mean time at the observatory. The emersion was well observed, and took place at $1^h\ 36^m\ 7^s{\cdot}0$ sidereal, or $18^h\ 32^m\ 52^s{\cdot}2$ mean time.

The chronometrical determinations were carried into effect through the kindness of the Rev. W. R. Dawes and of Mr. Dent, the former of whom conveyed to Liverpool, by railway, the six chronometers lent for the purpose by the latter. Their errors were obtained at the Royal Observatory, Greenwich, and at Mr. Bishop's Observatory in the Regent's Park, on the 25th of July, previously to setting out. Mr. Dawes arrived at Starfield Observatory about seven o'clock on July 26, when the chronometers were immediately compared with the sidereal clock. The chronometers remained at Starfield till the 10th of August, at 19^h, having been carefully rated from day to day, whenever celestial observations could be obtained.

They were compared both at Mr. Bishop's Observatory and at Greenwich on their return, though the author, suspecting an error

in the comparisons of two of them at the latter place, did not include them in the results of the journey up.

The mean result for the difference of longitude between Mr. Bishop's Observatory and that of Starfield, from the six chronometers, in both journeys, is $11^m\ 10^s{\cdot}06$, and, assuming Mr. Bishop's Observatory to be $37^s{\cdot}0$, the longitude of Starfield, west from Greenwich, results $11^m\ 47^s{\cdot}06$. The result derived from the direct comparisons made at Greenwich is $11^m\ 47^s{\cdot}2$. The journey down, when calculated by itself, gave $11^m\ 10^s{\cdot}07$ for the difference of longitude between Mr. Bishop's and the Starfield Observatory, and the comparisons made at Greenwich and the former Observatory, on the 25th of July, gives for their difference of longitude $36^s{\cdot}96$. The above results have not been cleared of the effects of personal equation of the observers, this not having been yet ascertained. The author, in conclusion, assumes that his longitude is $11^m\ 47^s{\cdot}1$, which he thinks cannot be far from the truth.

IV. Professor Schumacher's third, fourth, and fifth Circulars on the great Comet of 1843.

The following is an abstract of the most important of the observations recorded in the above:—

Observations by Professor Bessel at Königsberg:—

March 26	8h 14m 38s M.T.	R.A. = 54° 48′ 25″·3	Decl. = −7° 36′ 23″·9
27	8 7 28	56 5 25·0	7 21 55·5
29	8 19 4	58 29 56·2	6 52 24·9

By M. Kreil at Prague:—

March 25	8h 19m 35s Sid.	R.A. = 53° 30′ 55″·5	
	8 20 40	....	Decl. = −7° 54′ 1″·9

By Professor Encke at Berlin, for 8^h Berlin mean time:—

March 29	R.A. = 58° 30′ 4″·1	Decl. −6° 53′ 2″·9
30	59 37 10·1	6 39 45·0
31	60 42 6·0	6 26 19·6

The unsatisfactory representation of the observations by a parabolic orbit, and especially the impossible perihelion distance, induced M. Encke to try whether another conic section would represent them better; and he has deduced the following hyperbolic elements:—

Time of Perihelion Passage, Feb. 27·49778, Berlin mean time.

Long. of the Perihelion	179° 2′ 29″·9	From the mean Equinox of March 0.
—— Node	4 15 24·9	
Inclination	35 12 38·2	
Eccentricity...............	1·00021825	

ψ 1° 11′ 49″·0 (e = Sec. ψ)

Log. Perihelion Dist. 7·717642

Motion retrograde.

The above observations represent the observations from March 20 to March 31 pretty well.

Observations by M. de Vico at Rome :—

March 18	7h 30m 50s·5 M.T.	R.A. = 2h 47m 47s	Decl. − 9° 50′ 55″·4
19	7 58 35·8	2 55 37	9 30 43·7
20	7 38 44·5	3 2 44	9 13 6·1

By M. Argelander at Bonn :—

March 21	7h 56m 25s·8 M.T.	R.A. = 47° 27′ 21″·4	Decl. = −8° 56′ 30″·9
25	8 10 23·8	53 31 28·8	7 51 15·0
28	8 2 46·6	57 21 6·5	7 7 10·6
29	7 57 45·4	58 31 11·9	6 53 0·7

from which, viz. those of March 21, 25, and 29, the following elements are deduced :—

Time of Perihelion Passage, 1843, Feb. 27·497, Berlin mean time.

Log. q 7·85576.

Dist. of the Perihelion from the Node ...	83° 28′ 58″ (Perihelion 277° 29′ 21″)
Node........................	9 58 19
Inclination	35 44 22

Motion retrograde.

By Professor von Steinheil at Munich : —

March 19	7h 48m M.T.	R.A. = 43° 52′ 24″	Decl. − 9° 40′ 6″
20	7 48	45 42 7	9 13 54
21	7 48	47 24 54	8 57 22·5
22	7 33·3	49 2 46	8 40 19
23	7 52·4	50 36 30	8 23 4
30	7 58·3	59 37 42	6 40 4

The above observations M. Steinheil failed to represent by a parabolic orbit.

By M. Nacolai at Manheim :

March 21	7h 44m 13s M.T.	R.A. = 47° 25′ 40″	Decl. = − 8° 56′ 43″
25	8 13 36	53 30 45	7 52 2
29	8 16 50	58 31 46	6 52 52
30	7 59 2	59 38 20	6 39 ±

from which observations, viz. those of March 21, 25, and 29, are deduced the following parabolic elements :

Time of Perihelion Passage, 1843, Feb. 27·4256, Berlin mean time.

Log. q	7·567843
π	280° 32′ 25″
☊	4 36 0
i	35 10 39

Motion retrograde.

By M. Capocci at Naples:

March 17	$7^h\ 46^m\ 5^s\!\cdot\!9$ Sid.	R.A. =	$39^\circ\ 58'\ 38''\!\cdot\!4$	Decl. $-\ 10^\circ\ 5'\ 36''\!\cdot\!0$
22	7 48 2·3		49 2 13·2	8 40 7·2
27	7 46 11·8		56 4 19·2	7 21 56·4
31	8 26 7·3		60 41 7·8	6 26 40·0

And from the first three of the above observations are deduced the following elements:

Time of Perihelion Passage 1843, Feb. 27, $9^h\ 55^m$, Greenwich mean time.

q	7·63148
ϖ	$279^\circ\ 59'\ 7''$
☊	3 55 17
i	35 15 42

Motion retrograde.

By M. Plantamour of Geneva:

March 30 $7^h\ 50^m\ 23^s\!\cdot\!6$ M.T. R. A. = $59^\circ\ 37'\ 50''$ Decl. = $-\ 6^\circ\ 39'\ 37''$

by means of which and his former observations he finds the following corrected elements:

T	1843, Feb. 27·4461, Geneva mean time.	
q	0·005807	
ϖ	$278^\circ\ 18'\ 3''\!\cdot\!0$	From the mean Equinox of 1843, January 1.
☊	0 51 4·1	
i	35 45 39	

By Professor Knorre at Nicolajew:

March 19	$7^h\ 34^m\ 0^s$ M.T.	R.A. =	$43^\circ\ 46'\ 12''\!\cdot\!6$	Decl. $-\ 9^\circ\ 32'\ 14''\!\cdot\!2$
22	7 51 54		48 57 53·1	8 40 54·2
25	7 49 17		53 24 16·7	7 52 43·0
31	8 6 31		60 38 59·3	6 27 5·5

By Professor Kreil at Prague:

March 29	$8^h\ 21^m\ 59^s$ M.T.	R.A. =	$58^\circ\ 31'\ 0''\!\cdot\!6$	Decl. $-\ 6^\circ\ 54'\ 12''\!\cdot\!3$
30	8 16 41		59 38 15·5	6 39 50·5
31	8 14 22		60 43 3·2	6 26 10·1
April 1	8 22 54		61 45 6·7	6 14 32·1

V. Circular Letter from Professor Schumacher on a New Telescopic Comet discovered by M. Mauvais on May 3.

The comet presents the appearance of a small oval nebulosity of about 3′ in diameter, and it has in its centre a bright spot.

On May 3, at $15^h\ 10^m\ 54^s$, Paris mean time.

R.A. = $326^\circ\ 33'\ 44''$
Decl. = + 29 35 10

The Secretary announced that several communications had been received respecting the Great Comet, which would be read at the next meeting.

Vol. XIII. of the Memoirs is now published, and may be had of the Assistant Secretary, at the Apartments of the Society. Price to Fellows Ten Shillings.

Vol. XIV. has been previously announced.

London :—Printed by Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. V. *June* 9, 1843. No. 32.

Francis Baily, Esq., President, in the Chair.

Edward Turst Carver, Esq. was balloted for, and duly elected a Fellow of the Society.

The following communications were read:

I. On a Self-acting Circular Dividing Engine. By W. Simms, Esq.

The original graduation of a circle, notwithstanding the great improvements in the method invented by Mr. Troughton, is still attended with very great difficulties, requiring not only the greatest care on the part of the operator, but tending to injure his health by the labours required in it, and thus not admitting of frequent repetition. The necessary cost of an instrument produced by such an amount of severe labour is also another very serious objection. The author had long been of opinion, that, to copy the divisions of a circle which had been graduated with extraordinary care, upon work of smaller dimensions, would, in general, be more satisfactory than original graduation. The latter process consists of several successive steps, in either or all of which a certain amount of error may escape detection, which in general may go far to balance one another, although there will be parts in almost every work where errors appear arising from an accumulation of those minute quantities.

The author had long since determined, as soon as he could obtain sufficient leisure, to construct an engine sufficiently large for the graduation of all circles, excepting those of the largest class, and the object of this paper is to lay before the Society a brief notice of the successful termination of the work.

The engine, in general arrangement and construction, is similar to that made by Mr. Edward Troughton, in the author's possession, though there are several additions and peculiarities which are pointed out by him. The circle or engine-plate is of gun-metal, 46 inches in diameter, and was cast in one entire piece by Messrs. Maudslay and Field, teeth being ratched upon its edge. The centre of the engine-plate is so arranged that it can be entered by the axis of the instrument to be divided, and the work by this means brought down to bear upon the surface of the engine-plate, which arrangement prevents the necessity of separating the part

intended to receive the divisions from its axis, &c.—a process both troublesome and dangerous.

Upon the surface, and not far from the edge of the engine-plate, are two sets of divisions to spaces of five minutes, one set being in silver and the other strongly cut upon the gun-metal face. There are also as many teeth upon the edge as there are divisions upon the face of the engine-plate, namely, 4320, and consequently one revolution of the endless screw moves through a space of five minutes. The silver ring was divided according to Troughton's method with some slight variations. In this operation it seemed to the author the safer course to divide the circle completely, and then to use a single cutter for ratching the edge; and he believes that the teeth upon the edge have been cut as truly as the original divisions themselves.

Another very important arrangement is, that the engine is self-acting and requires no personal exertion or superintendance, nothing being necessary but the winding up of the machine, or rather the raising of a weight which, by its descent, communicates motion to the dividing engine. The machinery is so arranged that it can be used or dispensed with at pleasure, there being some cases in which a superintending hand is desirable.

The author then proceeds with a description of the machinery, as represented in the drawings accompanying his paper, and draws particular attention to the contrivance by which the engine can discharge itself from action when it has completed its work.

He concludes by observing that, as the machinery is simple, by no means expensive, can be made by an ordinary workman, is adapted to all the engines now in existence, which are moved by an endless screw, as it lessens the labour of the artist and increases the accuracy of the graduated instrument, he trusts his communication will prove acceptable to all who are interested about such matters.

II. Recomputation of Roy's Triangulation for connecting the Observatories of Greenwich and Paris. By W. Galbraith, Esq.

The author considers from internal evidence that Roy's measurement of the base on Hounslow Heath was in his own scale, and that of Mudge in Ramsden's scale, and he has used in his calculations the mean of these in imperial measure, reduced to the mean level of the sea. He has also availed himself of the New Survey of France to obtain such data as it afforded to connect the two countries.

Some of the most important of the results are as follows:—
Assuming the latitude of Greenwich to be 51° 28′ 38″·50 N., and the compression of the earth to be $\frac{1}{300}$, there results for

Calais	Lat. 50° 57′ 27″·67 N.;	Long. 1° 51′ 17″·30 E.
Dunkirk	Lat. 51 2 6·68 N.;	Long. 2 22 39·72 E.

Again, assuming from the new *Description Géometrique de la*

France, the long. of Calais to be 29′ 0″·40 West of Paris, and that of Dunkirk, 2′ 22″·66 East of Paris,

The Long. of Paris by comparison with Dunkirk is.........	° 2	′ 20	″ 17·70	E.
———— Calais	2	20	17·06	
———— Calais, from Kater's New Survey	2	20	19·13	

	h	m	s	
The Mean of which in Time is............	0	9	21·20	E.
Mr. Dent's Result by Chronometers was	0	9	21·21	
And Sir J. Herschel's by fire signals	0	9	21·46	

III. Occultations of Fixed Stars and the Planet *Jupiter* by the Moon. Observed at Hamburgh by C. Rumker, Esq.

				h	m	s	
1842.	July 26	15 Piscium	Em.	11	35	47·0	Hamb. M. T.
	— 30	μ Arietis	Em.	13	12	34·6	
	Sept. 26	B Tauri	Em.	11	20	19·7	
	Nov. 7	Jupiter { First contact		4	32	0·1	
		Jupiter { Complete	Im.	4	33	27·1	
	— 8	σ Capricorni	Em.	5	23	44·0	
	Dec. 14	47 Arietis	Im.	5	41	0·7	
1843.	Mar. 6	47 Arietis	Im.	8	11	52·1	
	— 8	95 Tauri	Im.	7	48	56·2	
	— 20	i Scorpii...............	Im.	15	16	38·3	
			Em.	15	50	52·4	
	Apr. 10	16 Sextantis	Im.	13	6	54·28	
	— 12	✱	Im.	8	54	2·18	
	May 3	3 Geminorum	Im.	9	41	29·9	
		4 Geminorum	Im.	9	57	32·7	

IV. The following communications concerning the Great Comet of 1843: —

1. Notes on its Appearance made during a Voyage from the Cape of Good Hope to England. By M. Close, Esq. Commander of the Ship Ellenborough.

It was first seen on the evening of the 4th of March, and before the discovery of the nucleus on the same evening was taken for a lunar iris. The nucleus was on this evening estimated to be of equal brightness with a star of the second or third magnitude, and the length of its tail 32° 30′. The tail had a darkish line from its nucleus through the centre to the end. Stars of the third magnitude were visible through the broadest part, but not near the nucleus. It was seen on several evenings, the last time being on the evening of the 31st of March; it was occasionally brilliant enough to throw a strong light on the sea. The greatest length of tail estimated was on the 19th of March, it being then 43° 30′, and it was observed to have considerable curvature. The account was accompanied by a small sketch of its appearance on the 5th of March.

2. A Letter from John Belam, Esq. Master of H. M. Sloop Albatross, on the Great Comet of 1843. Communicated by G. B. Airy, Esq. Astronomer Royal.

" On the 2d of March, in latitude 18° 33′ 18″ N. and longitude 72° 17′ 0″ West of Greenwich, a meteor made its appearance in

the western quarter of the heavens of a whitish colour. It became brighter each succeeding evening, and on the 7th we obtained the following observations:— In form it is like an elongated birch rod, slightly curved; the head or commencement of it being nearest the horizon at an altitude of about 19°, the tail pointing in the direction of *Sirius*, and measuring from the head 28°. That part of it from which the tail is produced is of a reddish appearance, but no star is visible through a common telescope.

Greenwich Mean Time.		Observed Distance between	
	h m s		° ′ ″
March 7	11 54 56	The Comet and Sirius......	83 12 10
	11 56 33	——— α Canopus	75 25 40

Observed Azimuth South, 79° 10′.

Greenwich Mean Time.		Observed Distance between	
	h m s		° ′ ″
March 13	11 52 46	The Comet and α Canopus	66 55 10
	11 54 46	——— Sirius......	67 1 40
	11 55 46	——— Aldebaran	44 26 30

Its altitude appears to be about 23°; its azimuth (observed), South, 79° 11′.

" It shewed brightest on the 7th instant, and since then to the 13th it has gradually become fainter.

" Since its appearance the generative point has been surrounded by a misty haze, from which cause the observations could not be taken with any great precision.

"*H. M. Sloop Albatross, Port au Prince,*
St. Domingo, 14th March, 1843."

3. Observations of the Comet by Mr. S. C. Walker and Professor Kendall, at the Observatory of the High School at Philadelphia. Communicated by Lieut.-Col. Sabine.

The comet was observed with the 9-feet Fraunhofer equatoreal, and the observations are cleared of the effects of differences of refraction, but not of the effects of parallax and aberration. Latitude of the Observatory, 39° 57′ 8″; longitude, $5^h\ 0^m\ 41^s{\cdot}9$.

Date of Observation Sidereal Time.	Apparent Right Ascension.	Sidereal Time.	Apparent Declination.
d h m s	h m s	h m s	° ′ ″
March 19 7 32 6·7	2 57 24·06	7 32 6·7	−9 27 36·5
22 7 48 45·3	3 17 43·34	7 44 49·2	−8 36 3·4
23 7 40 33·7	3 23 43·97	7 40 50·1	−8 19 20·8
24 7 35 52·5	3 29 36·28	7 46 32·9	−8 3 33·0
26 7 57 24·3	3 40 30·54	8 6 54·5	−7 32 32·2
29 8 11 35·1	3 54 14·26	8 14 10·0	−6 50 3·1

The observations of the 19th, 22d, and 24th, by computations,

made by the same gentlemen, give the following approximate elements.

Perihelion Passage, Feb. 26·0489, mean time Philadelphia.

Ascending Node	166° 1′ 25″
Inclination	39 0 22
Longitude of Perihelion	292 50 31
Perihelion Distance	0·00834

Motion direct.

4. Notes on the Comet, accompanied by a Pencil Sketch, by Captain Hopkins, commanding the East India Company's Ship, Seringapatam, on a voyage from the Cape of Good Hope. Communicated by Sir John Herschel.

The comet was seen first on the 2d of March, but indistinctly. A good view of it was obtained on March 4, when it was very brilliant. Its tail appeared separated through half its length by a dark line, and was, by rough measurement, found to be about 30° in length. After this it decreased in brightness, and was not seen longer than the 7th of April.

5. Extract of a Letter, dated St. Kitt's, 6th of March, 1843, from Lieut. D. W. Tyler, R. E.

6. Letter from J. T. Austin, Esq., dated Funchal, Madeira, April 8, 1843, accompanying a sketch of the Comet. Communicated by Sir John Herschel.

7. Notes on the Comet as seen by M. Montojo, at San Fernando. Communicated by Sir John Herschel.

The tail of the comet was first seen on the 6th of March; the nucleus was compared roughly with two small stars seen in the same field with it on the 13th; on the 14th and 15th some observations were obtained with an altitude and azimuth instrument, and it was compared with some known stars; the nucleus was not seen after the 1st of April.

8. An Account of the Comet as seen on board the ship Childe Harold on her voyage from Bombay to London. By Lieut. W. S. Jacob, R. E.

The tail was first seen on the 3d of March, but a good view of it was not obtained till the 9th. On this evening, the nucleus seen in a night telescope appeared like a star of the sixth magnitude, and the following distances from *α Eridani* and *α Orionis* were measured with a sextant:

Time by watch h m	° ′	
8 15	46 11	Distance from *α Eridani*.
16	12	
25	8	
26	10	
18	56 35	Distance from *α Orionis*.
19	34	
21	34	
23	34	

Watch fast on Greenwich mean time (by estimated longitude) 39m·0. The length of the tail 36°.

From the above observations Mr. Jacob infers the following place of the comet:

At $7^h\ 42^m$ Gr. M.T. R.A. $= 1^h\ 17^m\ 9^s$ Decl. $= -11°\ 59'$.

The nucleus was last seen on the 5th of April.

9. Letter from T. Forster, Esq., dated Bruges, April 22, 1843.

Dr. Forster, with a view of drawing attention to the phenomena observed by him on the 20th of March in connexion with the comet, had accurately represented in a coloured drawing the appearance of the comet and of the surrounding sky, and had caused it to be copied by an artist, with the intention of presenting the same to the Society. The drawing has since been received, and is now in the possession of the Society.

10. An Account of the Comet as seen on board the ship Malabar on her passage from the Cape of Good Hope. By R. Pollock, Esq. Commander.

The comet was first seen on the 2d of March. On the 5th the nucleus was well seen, and appeared as a star of the fourth magnitude; the length of tail was 23°.

The following measures of the distance of the nucleus and bright stars were made:

	h m		° ′ ″	° ′
March 10	7 0	Dist. from Regulus	53 20 0	
	7 35	Sirius...	74 33 30	Long. 7 22 W.
	7 40	Canopus	70 6 0	
11	7 35	Sirius...	71 41 0	
	7 40	Canopus	68 47 0	
13	7 35	Sirius...	67 0 0	Long. 9 50 W.
	7 40	Canopus	69 30 0	
14	No observations...........	....		Long. 15 55 W.

11. Letter from H. A. Cowper, Esq., H. M. Consul at Pernambuco in Brasil, dated 9th March, 1843.

The comet was seen first on March 1; and on the 4th Mr. Cowper saw the nucleus very distinctly, and makes the following remarks on its appearance:

"It is particularly small, without any nebulosity, but of extreme brightness, of a golden hue, and a line of the same bright colour may be distinctly traced running directly from it into the tail for 4° or 5°: the tail is perhaps 30° in length, and is of a brilliant silver colour, perfectly opaque, but becoming less and less dense until it is lost in space."

Mr. Cowper adds the following observations, made with a sextant on March 9, at his request, by a master of a merchant vessel:

		° ′	
Bearing of Nucleus	W.	7 45	S.
Altitude of ditto...		9 0	
Length of tail......		28 0	

Breadth of tail at two-thirds of its length from the nucleus 1°.

12. Observations made at the Royal Observatory, Cape of

Good Hope, by Piazzi Smyth, Esq. Communicated by Sir J. F. W. Herschel, Bart.

The nucleus of the comet was first seen on the 3d March, but it set about ten minutes after its discovery. It was looked at with the 46-inch achromatic telescope, and an approximate observation was attempted, by leaving the telescope fixed, and measuring, the next morning, the azimuth and altitude of the point where it set.

The nucleus seemed to consist of a planetary disk from which rays emerged in the direction of the tail. To the naked eye there appeared a double tail about 25° in length, the two streamers making with each other an angle of about 15′, and proceeding from the head in perfectly straight lines. From the end of the forked tail, and on the north side of it, a streamer diverged at an angle of 6° or 7° towards the north, and reached a distance of upwards of 65° from the comet's head; a star (probably τ *Ceti*) was near the end of this appendage: a similar, though much fainter, streamer was thought to turn off south of the line of direction of the tail.

On the 4th of March Mr. Smyth, accompanied by some friends, went to the Lion's Rump signal station, where the comet would set in a sea horizon, and several distances were taken with sextants and a reflecting circle. These not being reduced sufficiently are not inserted here.

On the 5th, the comet was seen, and several sextant observations were made. The appearance of the comet on this evening appeared considerably changed; the angle of the north streamer with the direction of the tail had been diminishing and was now south; it had also diminished in brightness. The total length was about 35°. All the rays proceeding from the head were now of uniform brightness, excepting one bright streak, which could be traced along the tail.

Though the observatory is very deficient in extra-meridional apparatus, Mr. Smyth succeeded, on March 6, by various expedients in obtaining several comparative measures of the nucleus and neighbouring stars, of which the unreduced observations only are given. On this evening he makes the following remarks respecting the appearance of the comet: "The nucleus is now the broadest part of that end of the comet; all the rays come from the posterior side, and are pretty equal in brightness, with the exception of a narrow bright streak in the middle, which runs for 3° or 4° along the middle of the tail, and then verges to the north side." The tail this evening was about 27° long. Several sextant observations of distance were made this day.

On the 8th, several differential observations were made.

On the 9th, some good differential observations and some sextant observations were made. The angle of the two sides of the tail at the head appeared to have undergone a gradual diminution, and the middle part was becoming more and more equal in brightness to the sides.

The paper contains also some observations of Laugier's Comet, and some observations of occultations of stars by the moon.

13. **Abstract, by the Secretary, of Newspaper Accounts of the Comet which have been forwarded to the Society.**

Some of these accounts are of considerable interest, and Mr. Main thought it desirable to collect and to bring them before the Society, which will thus be in possession of almost every thing that has been published relatively to the comet.

The first is extracted from a newspaper of Tobago, and is signed M. Dill, Lieutenant Royal Engineers, Superintendent of Signals, and dated Fort King George, 8th March, 1843.

"During the last few days, our island has been visited by a very large and brilliant comet. It is said to have been seen first on Friday evening, but was more generally observed on Saturday, when it presented a most luminous appearance; the nucleus of the comet being, at about ¼ to 7 o'clock, in the direction of south-west, and the tail stretching to an immense length across the heavens in a south-east direction, and in opposition to the sun's light. On Sunday night, it was observed by compass, when the nucleus bore west 25° 0′ south, at ¼ to 7 o'clock. On Monday, an observation was taken with a theodolite at $16^{m}\frac{1}{4}$ past 7 o'clock, when the nucleus bore west 16° 7′ south, with an elevation of 6° 4′, and the end of the tail, as nearly as it could be caught, bore west 28° 14′ south, with an elevation of 28° 21′."

Similar observations were made of it on the following Tuesday and Wednesday.

The following letter, from Mr. Benjamin Pierce, dated Cambridge, March 31, 1843, appeared in the *Boston Courier* of April 1, 1843:—

"The elements of the comet's orbit, which I send you, are roughly computed, and will need future correction. They agree very closely with Mr. Clarke's noonday observations of February 28, and were computed from Mr. Bond's observations of March 11, 18, 24, and 26. More correct calculations, in which all the observations will be thoroughly discussed, will in due time be presented to the American Academy.

Long. of the Ascending Node	348° 33′
Inclination	39 16
Long. of the Perihelion..............	280 31
Perihelion Distance	0·00872

Time of Perihelion Passage, Feb. 27ᵈ·01, mean time at Cambridge.
Motion retrograde.

In another American paper was the following account:—

"A comet of unusual size and brilliancy was distinctly visible to the naked eye in this vicinity on Tuesday last, the 28th of February, 1843, at noonday, at a distance, as we should judge, of 5° or 6° east from the sun. It extended over a space in the heavens of nearly 3° in length, with little more than 1° in width, and appeared like a very small white cloud, with its nucleus, or densest part, towards the sun, and its luminous train in opposition to it. On

viewing it through a common telescope of moderate power, it presented a distinct and beautiful appearance, exhibiting a very white and bright nucleus, and a tail dividing near the nucleus into two separate branches, with the outer sides of each branch convex, and of nearly equal length, apparently 8° or 10°, and a space between their extremities of 5° or 6°. Though viewed several minutes under these favourable circumstances, no coruscations were perceived."

The above American accounts were communicated to the Astronomer Royal by Mr. J. Cranch, at the request of Mr. Bond, of the Cambridge Observatory, near Boston.

In an article by M. Plana, extracted from the *Gazetta Piemontese* of the 4th of April, are the following parabolic elements of the comet:—

Perihelion Passage, 1843, Feb. 27·652, Munich mean time.

Perihelion Distance	0·0056343
Long. of the Perihelion	189° 51′ 25″
Inclination	40 29 37
Long. of the Ascending Node	353 0 59

Motion retrograde.

The following observations and elements of the comet, given by M. Carlini, Director of the Royal Observatory of Milan, are extracted from an Italian Gazette:—

1843.	Mean Time.	R. A.	Declination.	Longitude.	Latitude.
March 19	h m 7 37	43° 33′	−9° 31′	1ˢ 8° 18′	−25° 5′
29	8 14	58 30	−6 52	1 24 34	−26 32
30	8 14	59 38	−6 38	1 25 51	−26 33

The above observations, together with one made at Munich on the 23d of March, and another made at Padua on the 24th, furnished the following elements:—

Perihelion Passage, Feb. 27, 5ʰ mean time of Milan.

Perihelion Distance	0·1542
Long. of the Perihelion	243° 33′
Long. of the Node	353 45
Inclination	38 0

Motion retrograde.

In the *Guiana Herald* (city of Georgetown) of March 30, 1843, appeared a notice of the comet, dated Demerara, March 25, and signed J. Bamber, with observations of its distance from neighbouring bright stars; the most important of which are as follow:—

Saturday, March 18, at 7^h 14^m, the nucleus was brilliant; the coma, body, and tail, very transparent:

At	h m 7 23	Sirius and the Nucleus, Apparent Distance.........	° ′ ″ 55 56 0
	7 28	γ Orionis — —	40 16 7
	7 45	Aldebaran — —	35 11 0

"Saturday, March 19. Appearance as before. The evening was beautiful.

At	h m 7 7	α Orionis and Nucleus, Apparent Distance	° ′ ″ 47 0 0
	7 11	γ Orionis — —	38 14 0
	7 14	Rigel — —	32 32 9
	7 17	Aldebaran — —	32 2 0
	7 22	Sirius — —	55 9 0

"Sunday, March 29:

At	h m 7 5	Rigel and Nucleus, Apparent Distance	° ′ ″ 26 56 30
	7 11	α Orionis — —	38 4 0
	7 22	Sirius — —	45 13 0
	7 33	Aldebaran — —	26 32 0
	7 57	Sirius — —	44 25 0

"Monday 27th. The nucleus very faint, as also the body and tail.

At	h m 7 33	Rigel and Nucleus, Apparent Distance...............	° ′ ″ 21 0 0
	8 3	Procyon — —	52 50 0

It appears that the comet was first seen in this colony on March 3.

The newspaper containing the above account was received by the Astronomer Royal, and communicated to the Society by him.

An article by M. Capocci, on the comet, of which the following is an abstract, is extracted from the *Giornale della duc Sicilie,* of 1st May, 1843, and communicated by Colonel Jackson.

The article gives an account of a paper read by M. Capocci before the Royal Academy of Sciences of Naples. M. Capocci first corrects a mistake into which some observers appear to have fallen, in over-estimating the length of the tail, to which some persons attributed an extent of 80° to 90°, but which certainly was not visible beyond 40° to 45° from the nucleus. With respect to the difficulty attending the orbit of the comet, he attributes it to the very small perihelion distance, and the consequently very rapid angular motion at the passage through the perihelion; the comet, during the eighteen days following its perihelion passage (that is, prior to the time of its first observation on March 17), having gone through at least 170° of its angular motion round the sun; while, during the whole of the time of its visibility afterwards, it described only 3°, from which the orbit was to be determined; whence it has happened that astronomers of very high reputation have published results altogether false. With respect to the particular difficulty attending the circumstance of some of

the sets of observations having given a perihelion distance smaller than the sun's semi-diameter, and the apparent consequence that the comet must thus either have passed within the luminous matter of the sun, or have been projected obliquely from his surface, M. Capocci considers that it is more seeming than real, as an error sufficient to account for such a paradox, would have excited no surprise in an orbit with a greater perihelion distance.

In the meanwhile, the parabolic orbit, which seems to represent best all the observations, is the following:—

Perihelion Passage, Feb. 27·5643.

Perihelion Distance..................	0·00538
Long. of the Perihelion	277° 52′ 35″
Long. of the Node	354 48 50
Inclination	35 56 55

Motion retrograde.

M. Capocci thinks it probable, however, that the comet really moves in an elliptic orbit, and that it has appeared several times previously. He thinks it exceedingly probable that the comets of 1618, 1668, and 1702 were identical with the one in question, and that that of 1689 was still more clearly so, a probability which has not suggested itself to any one on account of the orbit of that comet inserted in the catalogue, calculated by Pingré, not being correct. But M. Capocci has found that, supposing the day of the perihelion passage in the year 1689 to have been December 3, the old observations of that comet are sufficiently well represented by the elements of the present one. The physical characters of the comet coincide also perfectly with those of the present one. Now this new and undeniable recognition, observes M. Capocci, curiously modifies the supposed period; and to make it satisfy all the returns, of which we have an account, it is perhaps necessary to reduce it to one of seven years nearly. He does not deny the difficulty of explaining how it has happened that the comet has not been seen at its nineteen former returns; but he contends that it is less difficult to do this than to account for the strange coincidence in the positions and in the physical appearances of the four comets above mentioned. The following is the whole series of the apparitions which may possibly belong to this one and the same body:—

1618, 1652, 1668, 1689, 1702, 1723, 1758, 1843.

Without laying very great stress on this coincidence, he thinks it proper to draw the attention of other astronomers to it, to the end that each, deducing a corresponding ellipse from his own observations, may either confirm or destroy the hypothesis; a circumstance so much the more important, as each may cherish the reasonable hope of seeing with his own eyes, within the space of seven years, the prediction verified.

The following is an abstract of a notice of the comet from a Madras paper received by the Astronomer Royal :—

"The comet was first seen on the 2d of March, but the only part seen above the horizon was part of the tail, and that faintly.

"On the 3d and 4th the nucleus was distinctly visible to the naked eye: the tail was divided into two distinct branches, the one long, but faint, the other much shorter, but broader and much brighter.

"On the 5th the tails had apparently united; but, on a careful examination, a less luminous band was detected between them.

"On the 6th several stars were visible through the tail, which near the star τ *Ceti* was about 40′ in breadth. At this part it appeared through the telescope to consist of three luminous bands; the one next to the sun being broad and bright, the other two fainter and more narrow towards the nucleus. These bands were less distinct, and not more than a single separation could be detected. The nucleus appeared like a star of the fourth or fifth magnitude; its light was pale, and it was surrounded by a luminous halo of no great extent."

14. Observations of the Comet made at the Observatory of Trevandrum, accompanied by a Drawing. By J. Caldecott, Esq. Director of the Observatory.

The observations were made with an achromatic telescope of 7½ feet focal length and 5 inches aperture, made by Dollond for the Observatory. It is mounted equatoreally on exactly the same plan as Mr. Bishop's instrument, the ends of the polar axis (which is of brass) being supported on pillars of granite. The micrometer made use of is a reticulated diaphragm of gold wire. The instrument keeps its adjustments very permanently, and the place of a known star (after correction for collimation and index error) seldom differs more than a second of time in right ascension, and 15″ to 20″ in declination.

The right ascensions and declinations of the comet are those read from the circles, after being corrected for instrumental errors, and for the effects of refraction, the instrumental corrections having been obtained almost every evening by observations of θ *Ceti*, when at nearly the same hour-angle as the comet was observed afterwards. In addition, differential observations of small stars passing through the field within a few minutes before or after the comet have been obtained, and the results will be communicated after the places of the stars have been determined by meridional observations.

The following is Mr. Caldecott's account of the observations :—

Places of the Comet.

Trevandrum Observatory, Lat. 8° 30′ 32″ N.; Long. $5^h\ 7^m\ 59^s$ East.

Date.	Trevandrum Mean Time.	Observed Right Ascension.	Observed North P. D.	Remarks.
1843.	h m s	h m s	° ′ ″	
March 6	7 4 35·30	0 33 56·4	101 58 0	The N. P. D. is probably erroneous this evening on account of interruption from visitors.
7	Observations	prevented by	clouds.	
8	6 54 30·81	1 0 45·0	102 7 22	The corrections obtained from β *Ceti.*
9	6 48 27·13	1 13 48·7	102 0 44	
10	6 50 47·96	1 26 22·1	101 51 37	Ditto from δ *Ceti.*
11	6 43 53·35	1 38 19·4	101 41 47	Ditto ditto.
12	Observations	prevented by	clouds and rain	
13	7 5 57·15	2 0 31·2	101 15 20	Ditto ditto.
14	6 53 36·38	2 10 37·8	101 0 6	Ditto ditto.
15	7 13 2·85	2 20 21·3	100 43 22	Ditto ditto.
16	6 45 57·44	2 29 20·6	100 27 4	Ditto ditto.
17	6 45 19·51	2 37 57·0	100 9 48	Ditto ditto.
18	6 59 30·01	2 46 11·7	99 52 2	Ditto ditto.
19	7 11 56·56	2 53 59·7	99 34 43	Ditto ditto.
20	Not observed	on account of	clouds.	

Notes.

The comet was first seen (partially only) on the 4th March, about half-past six P.M.; but clouds over the head of it, which was besides very near the horizon, prevented any observations.

On the 5th, a larger portion of the tail was visible, and it was evidently higher than the evening before; clouds, however, again hung over the head until it set.

On the 6th, the sky was free from clouds, and the comet presented a most magnificent appearance. Observations of it in R. A. and N. P. D. were obtained this evening with the equatoreal; but from the excitement at first view of so splendid an object, together with the confusion caused by a number of visitors at the observatory, I do not consider them entitled to much confidence, especially those in N. P. D. The length of the tail I measured roughly with a sextant, by bringing down the image of a star which happened to be situated near the faint end of it, into contact with the head, and made it to be about 36°; but from a much better measurement made in the same way on the 13th, this was probably too small. The nucleus of the head (seen through the 7½-feet telescope) presented rather a well defined planet-like disc, the diameter of which I *estimated* to be about 12″, and that of the nebulosity surrounding it at about 45″. The tail had a dark appearance along its axis, as if hollow; and at about half way from the head, it even appeared to separate slightly into two parts, the upper one being rather longer than the other.

On the 13th, after the observations for position, I introduced a parallel wire micrometer, with a view to measure the diameter of the bright part, or disc, of the head, and, by a pretty fair measure, made it to be 11″. The nebulosity about it I *estimated* to be about four times the diameter of the bright part. The length of the tail, measured carefully with a sextant, I found to be 45°; its

breadth, at one-third its length from the head, 33′, and at two-thirds its length, 60′.

Since the 19th, the weather has been unfavourable, and no observations have been obtainable. The comet appears to be getting somewhat fainter than it was on the evenings of the 6th and 8th, but only slightly and very slowly so.

JOHN CALDECOTT.

Trevandrum Observatory,
March 22, 1843.

A second letter has been received from Mr. Caldecott, dated April 21, giving the following additional observation :—

March 26 $7^{h}\ 3^{m}\ 36^{s}{\cdot}35$ Trev. M.T. R.A. = $3^{h}\ 38^{m}\ 7^{s}{\cdot}3$ N.P.D. = $97^{\circ}\ 39'\ 12''$

From the observations of the 8th, 13th, and 18th of March, Mr. Caldecott computed the parabolic elements, which are as follow :—

Long. of the Ascending Node	$3^{\circ}\ 7'$
Inclination	35 3
Long. of the Perihelion	279 6
Perihelion Distance	0·0048

Time of Perihelion Passage, Feb. 27·654, Trevandrum mean time.
Motion retrograde.

15. Letter from Professor Kendall, containing Observations of the Comet made at Philadelphia. Communicated by Lieutenant-Colonel Sabine.

Philadelphia, April 27th, 1843.

Sir,—I send you the result of the observations of the great comet of February 1843, made by Mr. Walker and myself, with the Fraunhofer equatoreal, at the Observatory of the Central High School, latitude 39° 57′ 8″, longitude $5^{h}\ 0^{m}\ 41^{s}{\cdot}9$ west of Greenwich. The measures were all made with the Fraunhofer filarmicrometer, power 75, except on the 9th and 10th of April, when the extreme faintness of the comet compelled us to use the ring-micrometer. We first saw the nucleus on the 11th of March, and brought the comet to the centre of the field, and read the graduations. The place given on that evening is liable to an error of two minutes of space. That of the 10th of April is liable to an error of about one minute of space. Those of the other evenings were the result of satisfactory measures. The nucleus, on the 11th of March, was near the star ζ *Ceti*, of the third magnitude, and was of about the same brightness. The tail extended between *Rigel* and *Sirius*, about 1° south of its position on the 18th, when we saw it and also the nucleus, but made no measures. In the comet-searcher, the nucleus appeared on the 11th, with a well-defined disc, larger than that of *Jupiter* in the same instrument. In the 9-feet equatoreal, it had no appearance of a disc, but only of a nebulosity gradually condensed toward the centre; so that it was impossible to distinguish any nucleus. I have no doubt that this comet was seen in the day-time, on the 28th of February and

the 1st of March. The particulars are stated at length in Professor Silliman's Journal. An observer at Woodstock, Vermont, saw the nucleus and tail in a good telescope, probably a 3½-feet Dollond. Mr. Clark of Portland-Maine, a teacher of navigation, measured its distance from the sun's limb at the time of culmination, and found it to be 6° 15¼′. Professor Loomis, of Western Reserve College, Hudson, Ohio, has computed the intensity of the comet's light on the 28th of February, and finds it to have been 24 times brighter than on the 11th of March; that is, 24 times brighter than a star of the third magnitude.

Mean Time, Philadelphia.	Star of Comparison and Magnitude.	No. of Measures.	Comet's observed R. A. corrected for Parallax, but not for Aberration.	No. of Measures.	Comet's observed Dec. corrected for Parallax, but not for Aberration.
1843. d h m s			h m s		° ′ ″
March 11 7 21 20·79			1 43 35·00		−11 35 23·00
19 7 25 55·68	∗*b*, 7·8	2	2 57 14·46	2	− 9 26 50·44
	∗*c*, 9·10			1	− 9 26 53·02
22 7 46 48·46	∗*h*, 8·9	7	3 17 44·47	2	− 8 35 58·55
23 7 39 59·79	∗*i*, 8	14	3 23 50·21	3	− 8 19 13·16
24 7 26 51·79	∗*k*, 8·9	4	3 29 36·44	2	− 8 3 35·48
	∗*l*, 8·9	4	36·61	1	40·35
	∗*m*, 8	4	36·74	1	54·90
26 7 36 10·32	∗*n*, 8·9	5	3 40 28·93	1	− 7 32 27·12
	∗*o*, 8	5	29·59	1	− 7 32 17·14
April 1 7 0 20·88	∗*s*, 9	8	4 7 54·53		
	∗*t*, 8·9	6	54·68		
2 7 48 6·35	∗*u*, 9	8	4 11 50·91	2	− 5 58 46·84
7 7 52 10·20	∗*v*, 8	4	4 29 33·93		
	∗*y*, 8	4	33·93		
	∗*z*, 9	4	33·44		
9 7 57 59·64	∗A, 9	7	4 35 52·21	7	− 4 45 38·29
10 8 21 46·25	∗A, 9	1	4 39 1·00	1	− 4 36 38·00

Apparent places of the Stars compared above with the Comet.

Name.	Right Ascension.	Declination.
	h m s	° ′ ″
b	2 57 37·68	−9 33 31·84
c	2 57 47·57	−9 27 27·04
h	3 19 17·08	−8 32 6·08
i	3 24 25·03	8 22 38·02
k	3 30 12·86	8 10 56·86
l	3 30 17·24	8 10 4·51
m	3 30 31·57	8 0 13·08
n	3 40 9·48	7 29 53·63
o	3 41 8·18	7 30 2·58
s	4 8 15·74	
t	4 8 57·61	
u	4 13 21·22	5 56 13·37
v	4 27 36·41	5 3 46·09
y	4 31 21·13	5 7 25·53
z	4 31 24·05	5 0 48·52
A	4 36 4·31	4 35 56·58

From the observations of the 19th and 26th of March, and 2d of April, we have computed the following elements:

Perihelion Passage, February 27^{d}·436953, Greenwich mean time.

Longitude of the Ascending Node 1° 55′ 18″·6 from mean equinox of March 26.
Longitude of the Perihelion ... 277 43 53·7 —— ——
Inclination.......................... 35 34 0·8
Perihelion Distance .... 0·00701906 log. q = 7·8462789
Motion retrograde.

The ephemeris computed from these elements, after applying aberration, requires the following corrections in order to agree with our observations:

	Corr. in A.R. $\frac{1}{15}\Delta\alpha$.	Corr. in Decl. $\Delta\delta$.
March 19	− 0^{s}·33	+ 38″·3
22	− 1·19	+ 5·0
23	− 1·38	+ 27·5
24	− 0·94	+ 1·8
26	− 2·07	+ 23·9
April 1	+ 1·99	
2	+ 4·50	− 21·6
7	+ 7·82	
9	+ 9·25	− 54·1

This corresponds well enough with the observations to be used in computing the parallax and aberration, and in reducing to a common date the places observed during the same half hour. These elements have some resemblance to those of the comet of 1689 as computed by Pingré. The inclination, however, of the latter, 69° 17′ differs too much to be consistent with their identity. Professor Benjamin Pierce, of Harvard University, Cambridge, Mass., has recomputed the observations used by Pingré, and finds for the elements of the comet of 1689,

Perihelion Passage 1689, December 2ᵈ·1403 Greenwich mean time.

Longitude Ascending Node	344° 18′
Longitude Perihelion.....................	271 16
Inclination	30 25
Perihelion Distance	0·0103

Motion retrograde.

The elements of the comet of 1843, with a period from 1689, December $2^{d}\cdot1403$, to 1843, February $27^{d}\cdot4370$, represent the places given by Pingré within 5°. Whether the errors of Pingré's places of the comet of 1689, together with the effect of perturbations, amount to 5°, is a subject worthy of investigation. It has never happened, I believe, that two comets have appeared with elements agreeing so well, without being found in the end to be the same.

Respectfully,

Lieut.-Colonel Sabine, R.A., Woolwich. E. O. Kendall.

16. Observations of Distance of the Comet from known stars, made at Demerara by Captain Geale of the ship Isabella, Lieut. A. S. Glascott, R.N., and James Donald, Esq. Communicated by Sir John Herschel.

March 11 at $7^{h}\ 20^{m}$ M.T.	Distance of comet from	Rigel......	= 50° 23′
		Sirius ...	= 71 21
		Capella ...	= 77 30
		Canopus	= 68 23
	Length of tail 46°.		
March 12 at 7 22	Distance from	Rigel......	= 47 24
		Sirius ...	= 69 0
		Aldebaran	= 46 40
		Canopus	= 67 11
		Capella ...	= 71 55
March 13 at 7 5	Distance from	Canopus	= 65 1
		Capella ...	= 70 13
		Sirius ...	= 66 36
		Aldebaran	= 43 30
March 19 at 7 15	Distance from	Capella ...	= 62 24
		Sirius ...	= 54 14
		Canopus	= 59 17
		Rigel......	= 32 6
	Length of tail 42°.		
March 26 at 7 10	Distance from	Sirius ...	= 44 10
		Capella ...	= 56 55
		Canopus	= 55 46
		Rigel......	= 21 35
	Length of tail 32°.		

March 27 at 7h 12m	Distance from	Sirius ...	= 43° 8′
		Rigel......	= 20 30
		Canopus	= 55 37
		Capella ...	= 56 28
	Length of tail 30°.		
March 31 at 7 35	Distance from	Canopus	= 53 28
		Sirius ...	= 58 31
		Capella ...	= 54 41
		Aldebaran	= 23 30
	Length of tail 24°.		

17. Some Account of the Comet in a Letter from J. Gimblett, Esq. Communicated by Sir John Herschel.

18. Extract of a Letter from Lieut.-Colonel Harvey, 14th Light Dragoons, dated Poona, March 13. Communicated by Professor Narrien.

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TO

VOL. V. OF THE MONTHLY NOTICES.

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ERRATA.

Page 68, line 12 from foot, *for* 59° 20′ 24″ *read* 59° 20′ 54″.

— 93, line 27, *for* have, *read* has.

— 107, line 17, *for* 1840, *read* 1839.

— 109, line 16, *for* parallel, *read* at right angles.

— 130, line 15, insert *the Earth* after *Venus*.

— 132, line 18, *for* (1,0) $\frac{e_0}{e}\cos(\varpi_0-\varpi_1)$, *read* [1,0] $\frac{e_0}{e_1}\cos(\varpi_0-\varpi_1)$.

— 132, line 19, *for* (1,2) $\frac{e_2}{e_1}\cos(\varpi_2-\varpi_1)$, *read* [1,2] $\frac{e_2}{e_1}\cos(\varpi_2-\varpi_1)$.

— 180, line 11 from the bottom, *for* node, *read* note.

— 208, line 27, *for* 65° *read* 65.

— 300, line 6, *for* Saturday, *read* Sunday.

line 13, *for* March 29, *read* March 26.

line 28, *for* della duc Sicilie, *read* delle due Sicilie.

243.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1843, *TO JUNE* 1845.

VOL. VI.

LONDON:
PRINTED BY
GEORGE BARCLAY, CASTLE STREET, LEICESTER SQUARE.
1845.

ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *November* 10, 1843. No. 1.

Francis Baily, Esq., President, in the Chair.

The Rev. John Moore Heath, M.A., Fellow and Tutor of Trinity College, Cambridge, was balloted for, and duly elected a Fellow of the Society.

The following communications were read:—

I. Description of a small Observatory constructed at Poona, in the year 1842, accompanied by observations of Eclipses, &c. of *Jupiter's* Satellites. By Lieut. W. S. Jacob, R.N.

The observatory was built for the purpose of containing a 5-feet equatoreal of Dollond. It was commenced in May 1842, and was completed in three months, at an expense of about 25*l*. The building is of brick, 20 feet long, from east to west, and 10 feet broad; the angles at the east end being cut off, so as to form part of an octagon. Instead of a rotatory roof, a folding one was constructed, which could be opened on any side that might be required. This was effected by means of a truncated octagonal pyramid, attached by hinges to an octagonal frame, laid on the top of the wall of the building, each side of the pyramid opening independently of the rest, and the top being closed by a flat octagonal shutter, attached to one of the sides of the pyramid by hinges. To support the instrument, a pier was built of brick, in the form of a T, at the ends of which were three stones, forming cubes of thirteen inches, for the foot of the stand.

The following are the observed phenomena of *Jupiter's* satellites, the longitude of Poona being $4^h\ 55^m\ 46^s$ east, and the latitude $18°\ 30'\ 42''$ north.

Eclipses and Occultations of Satellites, and Transits of Satellites and Shadows.

Day, 1842.	Sat.	Phenomenon.	Sidereal Time.
			h m s
Aug. 10	I	Eclipse reappearance	21 7 37·7
Sept. 26	I	Ingr. of shad. { first contact / total	19 34 13 / 19 36 24
	I	Egr. of sat. ...{ first contact / last contact	20 35 32 / 20 37 14
27	III	Egr. of sat. (last contact) ...	19 12 19
	I	Eclipse reappearance	19 15 30·5
	III	Ingr. of shad. { first contact / total	20 58 15 / 21 3 36
Oct. 3	I	Egr. of sat. ...{ first contact / last contact	22 54 17 / 22 57 7
7	II	Eclipse reappearance	23 6 43
14	II	Occultation, immersion......	20 42 23
18	I	Occultation, immersion......	22 50 32
20	I	Eclipse reappearance	21 0 57
Nov. 9	III	Egr. of sat. ...{ first contact / last contact	22 39 14 / 22 44 32
	III	Ingr. of shad. { first contact / total	23 49 51 / 23 55 42
11	I	Ingr. of sat....{ first contact / total	21 55 24 / 22 0 36
	I	Ingr. of shad. { first contact / total	23 5 18 / 23 8 26
12	I	Eclipse reappearance	22 46 20·3
Dec. 3	II	Eclipse reappearance	23 51 13·7

II. The following communications concerning the Great Comet of 1843:—

1. A Letter from S. C. Walker, Esq., to Sir J. F. W. Herschel. Communicated by Sir John Herschel.

"*Philadelphia, May* 23, 1843.

"Sir,—From the observations made at the High School Observatory, from March 11th to April 10th, the earliest and latest dates at which the place of the nucleus was measured, we have computed the elements of the orbit on the model of Gauss' *Theoria Motus*, without making any hypothesis respecting the particular conic section in which the comet moves. The result has been as follows:—

Perihelion passage, February $27^{d}\cdot5893933$ Greenwich mean time.

Longitude of the Perihelion	280° 44′ 3″·7	mean eq. March 30.
Longitude of the ascending Node......	15 57 3·2	mean eq. March 30.
Inclination	34 19 52·0	
Perihelion Distance	0·00410369	
Gaussian Angle χ	2° 26′ 12″·05	
Eccentricity, Sec. χ	1·00090495	
Mean Sidereal Daily Motion............	159″·58936 retrograde.	

"The normal places used were —

		March 20·5. Mean Time, Greenwich.	March 30·5. Mean Time, Greenwich.	April 9·5. Mean Time, Greenwich.
From the App^t Equinox	Geo. R.A.	46° 4′ 38″·4	59° 51′ 1″·2	68° 56′ 41″·6
——	Geo. Decl.	− 9 9 45·5	− 6 36 32·5	− 4 45 35·7
Errors of the Hyperb. Ephem.	R.A.	− 0·6	+ 0·0	− 0·6
——	Decl.	+ 0·7	− 1·0	+ 0·3

"Our observations were made with a 9-feet Fraunhofer, power 75, and with a Fraunhofer filar micrometer. The places of the stars used for comparison were taken from the *Histoire Céleste* and Bessel's *Zone Observations*. A small error in our measures, or in the star catalogues, in the declination of the comet, causes a great change in the elements. Still, allowing to that source of error all the weight to which it is entitled, the orbit comes out an hyperbola, and the perihelion point is either in a tangent to the sun, or as nearly so, as physical circumstances will permit.

"I have the honour to be, &c.

"Sears C. Walker."

2. Observations of the Comet. By Captain Tucker, R.N. Commander of H. M. Ship Dublin. Communicated by the Lords Commissioners of the Admiralty.

The comet was first seen on the 3d of March, and observations, of which the following is a tabular statement, were made from the 4th to the 26th of March:—

Day and Ship Mean Time of Observations.	Ship's Longitude.	Ship's Latitude.	Observed Altitude of Comet.	Distance of Comet from known Stars.	Names of Stars of Comparison.	Length of Tail.
d h m s	° ′	° ′	° ′	° ′ ″		° ′
Mar. 4, 6 58 26	106 0 W.	8 10 S.	4 45	72 44 ...	Rigel	
7 4 28	......	......	4 30	48 44 30	Fomalhaut	
5, 7 0 57	107 17	11 2	7 30	47 28 30	α Eridani	
				66 16 0	Aldebaran	
6, 7 4 49	107 59	14 1	9 6	46 26 8	α Eridani	
				65 32 45	Rigel	
				85 59 0	Sirius	
				62 4 0	Aldebaran	
7, 6 30 0	108 22	16 44	6 5	61 56 15	Rigel	43 0
				59 25 0	Aldebaran	
				82 31 30	Sirius	
8, 6 41 54	107 31	18 21	16 32	79 34 0	Sirius	
				59 3 0	Rigel	
7 6 4	......	......	14 0	79 33 0	Sirius	
				56 44 30	Aldebaran	
9, 7 10 23	106 29	19 33	14 48	76 39 15	Sirius	
				55 49 50	Rigel	

Day and Ship Mean Time of Observations.	Ship's Longitude.	Ship's Latitude.	Observed Altitude of Comet.	Distance of Comet from known Stars.	Names of Stars of Comparison.	Length of Tail.
d h m s	° '	° '	° '	° ' "		° '
Mar. 10, 6 52 15	106 17 W.	20 41 S.	18 30	73 45 0	Sirius	
				52 28 50	Rigel	
				46 17 0	α Eridani	
11, 7 1 35	105 25	22 4	18 12	46 31 0	α Eridani	
				71 2 0	Sirius	
				50 11 50	Rigel	
12, 7 15 10	105 16	24 1	18 0	68 36 15	Sirius	
				47 14 45	Rigel	
				46 31 15	Aldebaran	
				46 57 0	α Eridani	
17, 7 8 15	96 44	30 45	20 ...	57 40 0	Sirius	
				35 36 30	Rigel	
				36 49 0	Aldebaran	
				49 3 0	α Orionis	
18, 7 14 40	95 52	32 0	20 17	55 58 0	Sirius	42 16
				33 49 13	Rigel	
				60 18 0	Canopus	
				35 19 0	Aldebaran	
				47 0 30	α Orionis	
19, 7 19 40	93 52	32 35	......	54 11 0	Sirius	
				31 57 15	Rigel	
				33 56 0	Aldebaran	
				59 21 0	Canopus	
				45 20 45	α Orionis	
20, 7 11 30	90 5	33 13	......	52 10 30	Sirius	45 0
				30 9 8	Rigel	
				32 19 30	Aldebaran	
				43 35 0	α Orionis	
				58 46 0	Canopus	
22, 7 34 38	86 42	34 24	20 0	49 31 30	Sirius	
				26 49 53	Rigel	
				57 27 30	Canopus	
				30 3 0	Aldebaran	
26, 7 15 57	82 40	32 2	23 30	44 27 0	Sirius	36 26
				55 46 0	Canopus	
				21 12 23	Rigel	
				26 24 30	Aldebaran	
				34 19 53	α Orionis	

3. Observations of the Comet made at Auckland, New Zealand, accompanied by a map of its progress amongst the stars. By John Collyer Haile, Esq. Communicated by G. B. Airy, Esq.

The comet was first seen on the 2d of March, and continued visible till the 2d of April. The following places of the comet are annexed, which are deduced from observations made at Auckland, though the method of observation is not mentioned, nor are any details given:—

Day, 1843.	Time.	Right Ascen.	Declination S.	Length of Tail.
	h m	h m s	° ′ ″	° ′
Mar. 7	8 2	0 43 27	9 39 8	32 30
9	7 37	1 10 52	10 55 48	35 10
10	7 45	1 24 7	11 21 44	35 50
13	7 58	1 58 32	11 10 42	
19	7 35	2 49 3	9 27 50	41 50
20	7 37	3 0 2	9 22 43	41 30
24	8 3	3 25 50	8 29 13	35 10

Long. of Auckland 174° 45′ 40″ East.
Lat. of ditto 36 51 0 South.

4. Notes on the Comet, extracted from the Journal of Captain G. Rodney Mundy, R. N. Commander of H. M. S. Iris. Communicated by G. B. Airy, Esq.

5. Extracts from a Daily Journal of Remarks and Observations on the Comet, as seen at Van Dieman's Land. By Lieut. Kay, R. N. Communicated by Lieut.-Colonel Sabine, R. A.

The tail of the comet was first seen on the 1st of March. The nucleus was first seen on the 6th of March; and on this evening the observed length of the tail was 23° 20′, being at its broadest part about 54′ in breadth; the extreme breadth and the greatest condensation of light occurring at a distance of 12° from the nucleus.

On the 7th, the tail was 26° in length, and 50′ in extreme breadth. A dark line commencing near the middle and extending to the end, divided the tail into two portions.

On the 9th, the length of the tail was 39°, and its extreme breadth 76′. The dark line was again observed, commencing at about the middle of its length.

On the 11th, the nucleus was well seen, and examined with great care. No stellar point was visible, but its appearance was that of a large star covered with a thin film of cloud, or viewed through a telescope which had not been adjusted to focus. The length of the tail was 45°, and the extreme breadth 76′.

On the 12th, a different telescope was employed for the examination of the nucleus, but no stellar point was visible. The length of the tail was 42°, and its breadth 80′.

On the 14th, 22d, 24th, and 27th, the observed length of the tail was 42°, 40°, 39°, and 35° respectively.

The following table exhibits the distances from conspicuous stars which were observed with the sextant:—

Day, 1843.	Mean Solar Time.	Observed Distance.	Star of Comparison.
	h m s	° ′	
March 7	7 30 0	62 17	Aldebaran.
	7 36 0	104 49	α^2 Centauri.
	7 38 0	64 36	Rigel.
	7 40 0	77 31	α Orionis.
	7 42 0	102 34	Procyon.
	7 44 0	85 9	Sirius.
	7 47 0	76 43	Canopus.
10	7 38 13	53 24	Aldebaran.
		70 51	Canopus.
	7 45 50	53 26	Aldebaran.
		70 49	Canopus.
11	7 36 1	69 9	Canopus.
		50 28	Aldebaran.
	7 44 15	46 19	α Eridani.
		64 4	α Orionis.
	7 49 50	107 17	α^2 Centauri.
		89 25	Procyon.
	8 1 19	72 46	Sirius.
		93 44	Castor.
	8 9 22	64 11	α Orionis.
		69 20	Canopus.
12	7 44 15	67 38	Canopus.
		48 14	Aldebaran.
	7 54 27	46 36	α Eridani.
		62 18	α Orionis.
	8 2 2	108 11	α^2 Centauri.
		87 5	Procyon.
	8 10 1	99 54	ι Argûs.
		70 15	Sirius.
	8 19 10	67 32	Canopus.
		92 45	Pollux.
13	8 4 6	43 41	Aldebaran.
		64 49	Canopus.
	8 12 28	47 34	α Eridani.
		56 58	α Orionis.

Day, 1843.	Mean Solar Time.	Observed Distance.	Star of Comparison.
	h m s	° ′	
March 13	8 20 39	43 49	Rigel.
	8 21 36	65 15	Sirius.
	8 22 43	81 51	Procyon.
	8 26 14	97 58	ι Argûs.
2[illegible]	7 25 41	57 38	Canopus.
		30 52	Aldebaran.
	7 40 23	52 44	α Eridani.
		41 30	α Orionis.
	7 56 26	28 1	Rigel.
		103 11	Regulus.
	8 5 3	65 49	Procyon.
		30 54	Aldebaran.
	8 13 55	41 28	α Orionis.
		28 4	Rigel.
	8 20 55	57 40	Canopus.
		50 27	Sirius.
24	7 51 20	54 20	α Eridani.
		28 49	Aldebaran.
	7 58 7	103 12	Regulus.
		25 11	Rigel.
	8 3 34	25 12	Rigel.
		28 51	Aldebaran.
	8 8 5	47 46	Sirius.
		38 34	α Orionis.
	8 13 23	54 22	α Eridani.
		62 51	Procyon.
27	7 45 58	55 40	Canopus.
		25 49	Aldebaran.
	7 56 10	56 3	α Eridani.
		20 59	Rigel.
	8 1 8	97 32	Regulus.
		44 4	Sirius.

6. Extract of a letter from A. Abbott, Esq. containing remarks on the Comet as seen at Madeira, accompanied by two drawings. Communicated by Sir John Herschel.

7. Observations of the Comet by Captain P. P. King, R.N., made at Port Stephens, New South Wales. Communicated by Captain Beaufort, R.N.

The tail of the comet was first seen at Port Stephens on the 2d of March, producing great alarm among the natives.

On the 3d it was again seen, and a second ray was observed extending obliquely from it, and making with it an angle of 10°.

On the 4th, the nucleus was observed with an achromatic telescope when about 8° above the horizon. It appeared like a reddish stellar spot, the limbs well defined, and about 1′ in diameter by estimation. On this evening and the following, distances of the comet from neighbouring stars were measured with the sextant.

On March 18th and following days, comparisons of the comet with neighbouring stars were made with an annular micrometer attached to an achromatic telescope, of which the following are the most important:—

March 19, $7^h\ 37^m\ 14^s$ sidereal time (mean of three observations), the nucleus was east of η *Eridani* $3^m\ 53^s{\cdot}22$, and 7′ 0″ south of it.

March 20, $7^h\ 36^m\ 57^s{\cdot}5$ sidereal time (mean of four observations), the nucleus was east of η *Eridani* $11^m\ 16^s{\cdot}23$, and 11′ 39″ north of it.

March 23, $7^h\ 27^m\ 6^s{\cdot}3$ sidereal time (mean of three observations), the nucleus was $4^m\ 17^s{\cdot}2$ east, and 9′ 19″ south of ζ *Eridani*.

March 29, $8^h\ 26^m\ 59^s{\cdot}1$ sidereal time (mean of three observations), the nucleus was $12^m\ 0^s{\cdot}28$ west, and 15′ 41″ north of A *Eridani*.

This paper was accompanied by a map shewing the path of the comet amongst the stars, and by drawings of its appearance on successive nights.

8. Abstract of an article in *Le Cernéen*, a newspaper published at Port Louis, Mauritius, March 14, 1843. Communicated by G. B. Airy, Esq.

9. An article extracted from the *Colonial Observer*, of March 8, 1843. Published at Sidney, New South Wales. Communicated by F. Baily, Esq.

10. Copy of a Letter addressed to the Editor of the *New Zealand Colonist* by Captain W. M. Smith, R.A.; and extracts from three other Letters from New Zealand. Communicated by F. Baily, Esq.

11. Observations of the Comet, as seen off the Island of Timor, at the south-eastern entrance to the Indian Archipelago, at sea, by J. A. Murray, Esq. Communicated by G. B. Airy, Esq.

12. Pen-drawings of the appearance of the Comet, by Mrs. Grant. Communicated by Sir John Herschel.

13. A Letter from R. H. Williams, Esq., containing an extract from Notes on the Comet made at the Observatory of Batavia. Communicated by G. B. Airy, Esq.

14. Remarks on the Comet, as seen at Galveston, Texas, by W. Bollaert, Esq. Communicated by F. Baily, Esq.

15. Letter from the Rev. W. S. Mackay to Sir John Herschel, dated Calcutta, June 10, 1843. Communicated by Sir John Herschel.

The comet was first seen at Calcutta on the 5th of March, and continued visible until April 3. Distances from bright stars were observed, from which approximate right ascensions and declinations have been deduced.

Mr. Mackay observes with respect to the star η *Argûs*, that "in March last, it had become a star of the first magnitude, fully as bright as *Canopus*, and in colour and size very much like *Arcturus*. This has been observed by several other persons to whom I pointed it out. Is the star known as a variable star, or is the change now first observed? α *Crucis* looked quite dim beside it."

With regard to the variability of η *Argûs* Sir John Herschel remarks as follows:—

The sudden increase of η *Argûs* from a star intermediate between the first and second magnitude, to a first-rate first magnitude, which took place between 1837 and 1838, was mentioned by me in a letter to Messrs. Beer and Mädler, of which an extract is in No. 354 of Schumacher's *Astronomische Nachrichten*. It was then *far inferior* to *Canopus*, but equal to *Arcturus*, and very nearly, or quite so, to α *Centauri*. It had diminished materially when I left the Cape in April 1838, but was still a great star of the first magnitude. It would appear to be now making another start forward. If this continue, we may have a rival to *Sirius*, or, perhaps, to the planets. In 1838, its brightness was such as to obliterate many curious and interesting details of the great nebula in its immediate proximity, which I had fortunately recorded in its former state.

I do not quite understand Mr. Mackay's distinction between *size* and *brightness* of a fixed star. *Canopus* is at least double of *Arcturus* in its quantity of light. *Arcturus* and α *Centauri* are nearly equal, the latter, however, being somewhat the brighter of the two.

I take this opportunity to mention, that I remain fully convinced of the reality of the periodical variation of α *Orionis*. Not so of α *Cassiopeiæ*, in the case of which star the amount of supposed change, however, was very much less considerable, and in which, on account of its difference of colour from γ, the compared star, *the moon affects the comparison*, when above the horizon.

16. Observations of the Comet made in March 1843, at the Mauritius. By W. Lloyd, Esq.

The comet was first seen on the 1st of March, and its nucleus was seen by Mr. Lloyd on the 3d. On the 4th, instruments were set up at Doguet, on Les Plaines Willhems, and observations were commenced. These consist chiefly of observations of altitudes and azimuths, the details of which are given. The comet was observed till the 22d of March.

III. Occultations observed at Ashurst in the year 1843, by R. Snow, Esq.

Day.	Name of Star.	Phenom.	Limb.	True Sidereal Time.	Remarks.
				h m s	
Feb. 12	ζ Cancri	Em.	Bright	11 38 29·4	
April 10	16 Sextantis...	Imm.	Dark	13 36 52·2	Excellent.
		Em.	Bright	14 33 43·7	Not good.
11	g^1 Leonis	Imm.	Dark	13 45 19·5	Excellent.
		Em.	Bright	14 47 43 ±	Not good.
	g^2 Leonis	Imm.	Dark	13 57 59	Good.
May 9	e Leonis	Imm.	Dark	11 45 11·3	Beautiful.
Aug. 17	τ^1 Arietis	Imm.	Bright	20 32 1·9	Excellent.
		Em.	Dark	21 23 54·4	Ditto.
20	3 Geminorum	Em.	Dark	0 30 33	Emerged gradually.
Sept. 13	ζ Arietis	Imm.	Bright	3 57 41·5	

IV. Right Ascensions and North Polar Distances of the Comet of Mauvais observed at Hamburg. By C. Rumker, Esq. Communicated in a letter to F. Baily, Esq., dated July 25, 1843.

Day, 1843.	Mean Time at Hamburg.	Right Ascension.	Declination.
	h m s	° ′ ″	° ′ ″
May 24	12 49 10·1	337 57 16·2	+28 10 17·9
29	12 44 21·3	340 18 39·1	27 33 2·7
30	12 9 11·8	340 45 15·3	27 25 7·4
June 4	12 35 2·2	342 55 46·4	26 39 0·4
6	12 40 17·1	342 45 19·2	26 18 4·5
18	11 47 27·3	348 5 2·1	23 44 8·3
19	13 23 44·1	348 26 51·1	23 28 18·8
20	12 11 51·1	348 46 19·3	23 13 41·4
23	12 2 49·7	349 39 53·2	22 23 51·9
July 3	12 57 34·8	352 9 49·9	19 4 16·5
4	12 6 56·6	352 21 45·8	18 43 27·7
5	12 11 24·4	352 33 45·1	18 19 24·7
6	11 56 10·2	352 44 57·6	17 55 41·7
7	13 18 53·0	352 56 25·3	17 30 7·0
9	12 21 19·5	353 16 18·3	....
9	12 27 41·0	....	16 40 43·1
19	12 4 48·1	354 27 0·0	11 54 26·6
20	11 53 56·5	354 31 11·6	+11 22 31·9

In a letter to Dr. Lee, dated August 18, 1843, the following additional places of the comet were communicated :—

Day, 1843.	Mean Time at Hamburg.	Right Ascension.	Declination.
	h m s	° ′ ″	° ′ ″
July 30	11 32 52·7	354 45 11·1	+5 33 28·1
Aug. 2	11 55 10·6	354 39 47·1	3 37 50·0
3	11 45 19·3	354 36 33·9	2 58 57·3
4	12 30 1·8	354 32 53·2	2 18 14·8
6	12 29 55·8	354 24 42·5	0 57 58·0
7	13 6 1·3	354 20 7·5	+0 16 29·4
8	14 23 45·6	354 13 57·9	−0 26 29·3

From the former of these sets of observations, M. Götze calculated the following elements of the orbit of the comet :—

Perihelion Passage, 1843, May 6·90816 Greenwich mean time.

Longitude of Ascending Node ...	157° 13′ 23″·72	From mean Equinox of June 20, 1843.
Longitude of Perihelion	281 50 58·99	
Inclination	52 53 2·94	
Log. Perihelion Distance	0·2097314	Motion direct.

V. On the Divisions of the Exterior Ring of the Planet *Saturn*. By the Rev. W. R. Dawes.

The existing evidence relating to a division of the outer ring of the planet *Saturn*, into two or more concentric rings, is of a very conflicting character. A few observers have been well satisfied that they have occasionally perceived such a division, among whom stand conspicuous, Short, the celebrated maker of reflecting telescopes, Professor Quetelet, of Brussels, and Captain Kater, whose paper on the subject, published in Vol. IV. Part 2, of the *Memoirs of the Astronomical Society*, discusses the subject at some length, in addition to a detail of his own observations. The evidence on the other side of the question, however, though of a negative character, has always appeared to me so strong, that I must confess myself to have been somewhat incredulous of the supposed fact of any subdivisions existing. This circumstance increases my inclination to put on record a recent observation of a peculiarly satisfactory kind.

September 7, 1843.—At Mr. Lassell's observatory, Starfield, near Liverpool. The day had been cloudless and remarkably warm, the maximum of the thermometer being 76° where all precautions had been taken to keep it as cool as possible. In the evening the sky was hazy and the stars dull. At about 9 P.M. Mr. Lassell turned his equatoreally mounted 9-foot Newtonian reflector, of 9 inches aperture, upon *Saturn*, with a power of 200, and was electrified at the beautifully sharp and distinct view of the planet presented to him. Having applied as an eye-piece an achromatic lens (being the object-glass of a microscope), which produced a power of 450 times, Mr. Lassell examined the planet for a few minutes. I then took my place at the telescope, and Mr. Lassell requested

me to examine carefully the extremities of the ring, and say if I observed any thing remarkable. Having obtained a fine adjustment of the focus, I presently perceived the outer ring to be divided into two. This perfectly coincided with the impression Mr. Lassell had previously received. For some minutes I scrutinised this interesting object, and occasionally, for several seconds together, had by far the finest view of *Saturn* that I was ever favoured with. The outline of the planet was very hard and sharply defined with power 450; and the primary division of the ring very black and steadily seen all round the southern side. *When this was most satisfactorily observed*, a dark line was pretty obvious on the outer ring. I was not only perfectly satisfied of its existence, but had time during the best views carefully to estimate its *breadth*, in comparison with that of the division ordinarily seen. The proportion appeared to me to be as one to three; but Mr. Lassell estimated it at *scarcely* one-third. It is certainly rather *outside the middle* of the outer ring, and is broadest at the major axis, being in this respect precisely similar to the primary division. *It was equally visible at both ends of the ring.*

For further satisfaction, other eye-pieces were tried. A positive double eye-tube, magnifying 400 times, came nearest to the achromatic lens in efficiency; yet the latter gave the impression of equal sharpness and light, with an increase of 50 in the power. With 400 the secondary division was perceptible during occasional best views of the planet; but no lower power displayed it at all, though with them the usual features of *Saturn* were splendidly distinct. A positive double eye-piece producing a power of 520 was also applied; but by this time the state of the air had deteriorated; and though some confirmatory glimpses were obtained, the view was not so good as with the achromatic lens.

Neither Mr. Lassell nor myself obtained a single glimpse of any further subdivisions of the ring. The shading of the interior edge of the inner ring was very obvious, but no dark line was even suspected in that situation.

From my description of this splendid telescopic view of *Saturn*, it will be seen that it was very similar to that depicted by Captain Kater, in fig. 3 of his drawings of the planet, in Vol. IV. Part 2, of the *Memoirs;* except that, in his plate, the outer ring is much too broad in proportion; and also that his subdivision *bisects* the outer ring. Moreover, the ring is now more obliquely seen than in 1825, and the *northern* side of it is in view. It is difficult to suppress the unavailing regret that the planet was not, as in that year, at an altitude of about 60°, instead of only 14°; and that the atmosphere of this country is so rarely in a state to do justice to the capabilities of our most powerful and perfect instruments.

It may be proper to remark, that a record of our observations was entered in Mr. Lassell's journal both by him and myself, from which the above account has been compiled.

November 8th, 1843. W. R. DAWES.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *December* 8, 1843. No. 2.

Francis Baily, Esq., President, in the Chair.

The following communications were read:—

I. On the Apparent Magnitudes of the Fixed Stars. By C. Piazzi Smyth, Esq. Communicated by Captain W. H. Smyth, R.N.

The author complains of the want of information on the methods of observing the apparent magnitudes of the stars, and of the little attention which has been paid to the proposal of a prize for a successful photometer (*Memoirs*, Vol. I. p. 507), by the Astronomical Society.

He proposes to employ telescopic vision, and to measure the degrees of brightness of every star by means of the obscuration which is necessary to make it vanish. By this means, the necessity of direct comparison between stars taken two and two is avoided, and an absolute zero is established.

For producing the obscuration, he proposes, in the first place, a long wedge of blue coloured glass (with its prismatic qualities counteracted by a similar transparent wedge), made to slide between the object and eye-glasses, a little way out of focus. This wedge might be fixed on the eye end of the telescope, mounted either in a micrometer frame, or made to move in the manner of a barometer scale.

Another plan is, to have a coloured *disc* of glass in the tube, capable of sliding up and down in it, by which means the object will be differently obscured, on account of the variation of the diameter of the pencil of rays at different distances.

The author then dwells on the method of observation, the means of getting rid of the atmospheric effect, the establishment of a common unit of comparison, and the obviation of the practical difficulty of obtaining a uniform rate of obscuration.

II. On an Astronomical Time Watchcase. By the Rev. Professor Chevallier.

The author has invented a contrivance, by means of which a common watch can, at pleasure, be made to denote sidereal time,

nearly enough for the purpose of warning an observer when his presence will be wanted in the observatory.

The principle of the contrivance is to ***set a moveable face to the hands of the watch instead of setting the moveable hands to a fixed face.*** This is effected by means of a circular box containing the watch. The lid has a circular aperture, through which the hands of the watch may be seen. Upon the lid is a circular plate, upon which is engraved a double circle of hours, from 0 to 12 and from 12 to 24; and a concentric inner circular plate, moveable separately, upon which the minutes are engraved. A small pointer projects from the part of the inner circle, which indicates 60^m, directing the eye at once to that point as the temporary upper part of the face.

In order to set this watch-case for use, it is quite unimportant what time the watch itself indicates. The lid is simply placed so that the hour-hand of the watch may point to the part of the hour-circle corresponding to the sidereal hour: the minute-circle being subsequently turned, till the minute corresponding to the minute of sidereal time is opposite to the minute-hand of the watch.

The hands of the watch then, ***as referred to the temporary position of the moveable circles***, indicate sidereal time; and, if they are set a little too fast, they will continue to do so to the nearest minute for almost six hours; thus giving the observer upon his table a duplicate of his observatory clock, sufficiently exact for the purpose which he wants.

It is plain that this contrivance can, with the greatest ease, be applied to any common watch-case; or, if a watch-glass were made capable of being turned round, the hours might be marked upon the glass, the minutes being engraved upon a moveable rim upon the watch-case.

III. Mean Places, for Jan. 1, 1842, of 50 Telescopic Stars, within two degrees North Polar Distance, observed in the years 1842 and 1843, at Markree, in the County of Sligo. By E. J. Cooper, Esq. and A. Graham, Esq.

In the reduction of the observations the index correction was, with a very few exceptions, taken from observations of *Polaris;* using the apparent place given in the *Nautical Almanac*, corrected for the terms of nutation involving 2 ☽. From the corrected observed right ascensions and declinations, the ecliptic polar distance and angular distance from the solstitial colure were determined, using the apparent obliquity of the *fixed* ecliptic, after having allowed for the motion of the equinoxial points along the equator. Applying the correction for aberration, precession, and nutation, the mean of the resulting co-latitudes and co-longitudes, with the mean obliquity of the *fixed* ecliptic for the 1st January, 1842, were used in finding the mean right ascensions and declinations.

In order that a judgment may be formed of the accuracy of the results, columns are annexed, to shew the extreme difference of the mean co-latitudes and co-longitudes which resulted from each observation.

IV. On the Orbits of several ancient Comets. By J. R. Hind, Esq. of the Royal Observatory, Greenwich. Communicated by the Rev. R. Main.

The elements of the following comets are in this paper deduced from observations given by M. Edouard Biot, in the Appendix to the *Connaissance des Temps* for 1846, viz.: of the second comet of 568; of the comet of 574; and of the comet of 1385. In the cases of the first and second of these an ephemeris is computed from the elements which represent the observations with tolerable accuracy. The observations are found in a supplement to the catalogue of Ma-tuon-lin (translated by M. Biot). This supplement and Ma-tuon-lin's *Catalogue of Extraordinary Stars* contain many observations, not previously published, and the orbits of the above-mentioned comets are not found in any catalogue. From the nature of the Chinese observations the longitude of the nodes and inclinations of the orbits are subject to great uncertainty; indeed, none of the elements can be considered as better than rough approximations.

V. Approximate Elements of the Orbit of the Comet recently discovered by M. Faye. By Professor Henderson.

The comet discovered at Paris by M. Faye was observed at Edinburgh on December 2d.

At $9^h\ 58^m\ 6^s$ mean time, its right ascension was $5^h\ 20^m\ 20^s{\cdot}4$, and declination $+5°\ 5'\ 13''$.

From the observations made at Paris on November 24, at Kensington on November 29, and at Edinburgh on December 2, the following approximate elements of the orbit have been deduced:—

Time of Perihelion Passage, Sept. 28·898 Greenwich mean time.

Peribelion Distance	2·5976	
	°	′
Longitude of Perihelion	57	2
Longitude of Ascending Node	226	4
Inclination	22	14

Motion direct.

From the great distance of the comet, and slowness of its motion, small errors in the observations and quantities neglected in the computations have a considerable effect upon the elements. Those here given may indicate the place where the comet is to be found.

Of all the comets whose orbits have been determined, this has the greatest perihelion distance, except the one of 1729.

$$x=[9{\cdot}9833]\ r.\sin(v+144°\ 50')$$
$$y=[9{\cdot}9968]\ r.\sin(v+\ 52\ 44)$$
$$z=[9{\cdot}4784]\ r.\sin(v+118\ 40)$$

VI. Two circular letters from Professor Schumacher on the Comet discovered by M. Faye. Communicated by F. Baily, Esq.

These circulars contain letters from M. Faye, announcing the

discovery and observations of the comet. The observed places are as follows, for Paris mean time:—

1843, Nov. 22	$14^h\ 44^m\ 11^s$	R.A. $= 81°\ 5'.0$	Decl. $= +6°\ 56'\ ...''$
	15 28 54	81 4.5	6 58 ...
24	17 4 43	80 50.7	6 30 35

VII. Results of Observations made with a Sextant and Pocket Chronometer, for determining the Latitude and Longitude of the Apartments of the Society. By J. Hartnup, Esq. Communicated by Captain W. H. Smyth, R.N.

The observations from which the latitude and longitude were determined, were made with a 10-inch sextant mounted on a stand, and the altitudes were taken in an artificial horizon of mercury.

The resulting longitude is the mean of nine partial results, deduced from observations included between June 24, 1842, and May 4, 1843. The mean of these results gives $27^s.38$ west of Greenwich, the extreme difference being $0^s.82$. The first result was derived by observations made at Somerset House, and at Lord Wrottesley's Observatory, at Blackheath, with an assumed rate of the chronometers during the interval, the longitude of Lord Wrottesley's Observatory being assumed to be $2^s.7$ east of Greenwich. For the second and third results, the difference of longitude between Somerset House and Mr. Bishop's Observatory, in the Regent's Park, was obtained, the longitude of Mr. Bishop's Observatory being assumed to be $37^s.1$ west of Greenwich. The last six results were obtained by direct comparison with Greenwich, the Greenwich time being furnished by the Rev. R. Sheepshanks, through chronometers which had been compared by him with the transit clock at the Royal Observatory.

The altitudes of stars from which the latitude is derived were taken on the eastern end of the terrace, about 340 feet south of the apartments of the Society. Stars were observed south of the zenith to balance with *Polaris*, which was observed both on and at a distance from the meridian.

From six partial results, obtained between November 12, 1842, and January 4, 1843, the latitude of the east end of the terrace was found to be $51°\ 30'\ 34''.9$ north; the extreme difference being $3''.4$. Whence the latitude of the apartments of the Society results $51°\ 30'\ 38''.3$ north.

Erratum in last Monthly Notice (November 10, 1843).

Page 10, Right Ascension of Mauvais's Comet, June 6, *for* 342° *read* 343°.

London:—Printed by Moyes and Barclay, Castle Street, Leicester Square.

The following additional communication respecting the comet discovered by M. Faye has been received from Professor Henderson :—

The comet was observed again at Edinburgh on December 15. At $11^h\ 38^m\ 7^s$ mean time (time of meridian transit), its right ascension was $5^h\ 14^m\ 1^s{\cdot}6$, and declination $+3°\ 28'\ 12''$; with some uncertainty, for the comet was very faint, and only one observation could be made.

From the Paris observation of November 24, and those at Edinburgh on December 2 and 15, the following elements have been deduced :—

Time of Perihelion Passage, Sept. $8^d{\cdot}836$ Greenwich mean time.

Log. Perihelion Distance............	0·26102		
Longitude of Perihelion	32°	56′	24″
Longitude of Ascending Node......	219	2	19
Inclination	16	17	10

Motion direct.

$$x = [9{\cdot}9931]\ r.\sin(v + 121°\ 47')$$
$$y = [9{\cdot}9927]\ r.\sin(v + 29\ \ 53)$$
$$z = [9{\cdot}4045]\ r.\sin(v + 74\ \ 56)$$

Though these elements must be much more correct than those formerly communicated, they may still undergo considerable alteration; for parabolas considerably different represent the observations with almost equal accuracy.

ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *January* 12, 1844. No. 3.

Francis Baily, Esq., President, in the Chair.

The following communications were read :—

I. On the Advantages of employing Large Specula and Elevated Situations for Astronomical Observations. By C. Piazzi Smyth, Esq. Communicated by Captain W. H. Smyth, R.N.

The author adverts to methods proposed by Mr. H. F. Talbot for the multiplication of copies of specula by means of the electrotype, and for observing astronomical objects with a telescope absolutely fixed, by means of a revolving plane mirror, which methods he considers might, if carried out, produce great improvements in astronomy. Amongst the advantages of the latter method he enumerates the following, arising chiefly from the unlimited focal length which it would be possible to give to the mirror: First, The obviation of the necessity of an accurate parabolic shape for the reflector; Secondly, The magnifying of the image without distortion or colour; Thirdly, The small effect which inaccuracies of the screw of the micrometer would produce, eye-pieces of low power being employed; Fourthly, The elimination of errors dependent on the contraction or expansion of the tubes of telescopes; and lastly, The advantage of having the eye in a fixed position.

The author then enlarges on the advantages which would attend the use of such a fixed telescope if placed on the slope of a high mountain, with the object-mirror and the eye-piece fixed on piers, and separated by a considerable interval, the mirror being beneath. The Nilgherry hills in India he instances as being favourable for the purpose, the climate being particularly well suited for astronomical observations. He then answers the obvious objection of the impossibility of reflecting objects from every part of the heavens to the speculum, by assuming that it would be most advantageous for astronomical science that every observatory should confine itself to those classes of objects which its geographical position enables it most readily to command. He finally dwells upon the cheapness of the labour of computation in India, arising from the circumstance of the great number of Brahmin priests who are willing and competent to undertake the labour for a very trifling remuneration.

II. Observations of the Planet Uranus, made in the year 1843. By C. Rumker, Esq. Communicated by Dr. Lee.

The observations extend from September 10 to October 31. They are corrected for refraction, but not for parallax.

III. A letter was read from Professor Schumacher to Mr. Baily, dated January 5, 1844, enclosing the Elements of the New Comet, computed by Dr. Goldschmidt, at the request of Professor Gauss, and which are as follows:—

Epoch of Mean Longitude, 1843, Dec^r. 2^d·11876 Berlin mean time, 58° 31′ 39″ (from the apparent equinox).

Mean Daily Motion	535″·7079
	° ′ ″
Perihelion	52 32 55
Angle of Eccentricity	31 29 39
Log. Semi-axis Major	0·5473857
	° ′ ″
Node	208 21 20
Inclination	10 58 58

And he remarks that, if the observations of the comet that have recently been made can be depended on, the orbit approaches the nearest to a circle of any that are yet known.

IV. On the Orbit of the Comet of Faye. By Professor Henderson.

Professor Challis had the kindness to communicate to me the following places of the comet observed at Cambridge. They were determined by comparison with 23 and 32 *Orionis*, and he believes that they are pretty accurate.

Mean Time from Greenwich Mean Noon.		Apparent R. A. of Comet.	Apparent N. P. D. of Comet.
	h m s	h m s	° ′ ″
1843 Nov. 29	11 12 23	5 21 37·5	84 24 55
Dec. 8	9 59 18	5 17 28·7	85 47 53
16	11 55 45	5 13 33·0	86 35 55

Suspecting that the great differences between the elements of the two parabolic orbits which I formerly communicated, might arise from errors in the observations employed, I proceeded to investigate the elements anew from the Cambridge observations. I followed the method of Olbers, and, after repeated approximations, the best parabolic orbit which I obtained, differing considerably from both the former, did not represent the middle observation to within six minutes of space.

This quantity being much too great to be imputed to error of observation, I concluded that the orbit was not parabolic, this supposition seeming to explain the discordances of the elements.

I next investigated the conic section in which the comet moves, according to the method of Gauss in the *Theoria Motus Corporum Celestium*, employing the three observations at Cambridge; and I

obtained an elliptic orbit, whose period of revolution is about six years and a half. The elements are

Time of Perihelion Passage, Oct. $23^d \cdot 6970$ Greenwich mean time.

Longitude of Perihelion	52° 57′ 52″	mean eq. of 1844·0.
Longitude of ascending Node	208 7 28	
Inclination	10 55 23	
Eccentricity .... sin	31 20 58	
Logarithm of Semi-axis Major	0·545352	
Mean Daily Motion	539″·484	
Time of Revolution	6·57702 sidereal years.	

Motion Direct.

The following expressions for the comet's co-ordinates enable its geocentric positions to be more readily computed :—

$$x = [0 \cdot 50302] \sin (e + 138° \ 5' \ 32'') - 1 \cdot 10654$$
$$y = [0 \cdot 50843] \sin (e + \ 55 \ 35 \ 24) - 1 \cdot 38397$$
$$z = [9 \cdot 94451] \sin (e + \ 74 \ 56 \ 11) - 0 \cdot 44213$$

e denoting the eccentric anomaly.

The errors of the computed places for all the observations which have reached me are as follows :—

		R.A.	Declination.
Nov. 22	Paris	− 303″	− 19″
24	Paris	− 1	+ 7
29	Cambridge ...	+ 18	+ 10
	Kensington ...	+ 3	+ 1
	Greenwich ...	+ 42	+ 2
Dec. 2	Edinburgh ...	− 6	− 17
8	Cambridge ...	+ 15	+ 12
15	Edinburgh ...	+ 7	+ 46
16	Cambridge ...	+ 23	+ 12
25	Edinburgh ...	− 40	+ 23

The first observation of right ascension at Paris must be affected with a considerable error.

It is very desirable that the comet should be observed for as long a period as possible. The following ephemeris for $10^h \ 30^m$ Greenwich mean time, indicates its position :—

Day.	Right Ascension.	Declination.	Distance from Sun.	Distance from Earth.
1844.	h m s	° ′		
Jan. 11	5 8 20	+ 3 36	1·843	0·973
15	5 9 12	3 55	1·858	1·008
19	5 10 36	4 17	1·873	1·046
23	5 12 28	4 42	1·889	1·087
27	5 14 44	5 8	1·906	1·130
31	5 17 32	5 35	1·923	1·176
Feb. 4	5 20 44	6 4	1·941	1·222
8	5 24 16	6 31	1·959	1·270
12	5 28 16	+6 58	1·977	1·320

In several respects this comet is very remarkable; and it may afford room for speculation regarding its identity with the lost comet of 1770. The orbit resembles more nearly the elliptical orbits of the planets than those of the periodic comets yet known. In its aphelion and perihelion it approaches nearly the orbits of Jupiter and Mars; and it must occasionally experience great perturbations from the former. It also passes within comparatively small distances of the orbits of the minor planets.

I have to-day received Professor Schumacher's circular, dated the 5th instant, communicating Dr. Goldschmidt's elements, which are nearly the same as mine. They may be considered as confirming each other.

I obtained an observation of the comet on December 25. Another on December 20 is not yet reduced, the star of comparison being undetermined.

	Mean time at Edinburgh.	R.A.	Declination.
	h m s	h m s	° ′ ″
December 25......	11 31 58	5 10 4·6	+3 1 42

Edinburgh, January 10, 1844.

V. A Letter from Professor Henderson announcing an additional Observation of the Comet of Faye.

"*Edinburgh, January* 10, 1844.

"This evening the comet was barely visible in my telescope, a 5-feet achromatic, with an object-glass of 3·6 inches. It was seen only at times, according to the varying clearness of the sky. At $9^h\ 44^m\ 24^s$ the right ascension was $5^h\ 8^m\ 28^s{\cdot}1$; the declination estimated $+ 3° 29'$. The right ascension observed exceeds that deduced from the ephemeris by 16 seconds of time.

"T. Henderson."

VI. Elements of the Comet of Faye, computed by J. C. Adams, Esq. of St. John's College, Cambridge. Communicated by Professor Challis.

The observations used were made with the Northumberland Telescope of the Cambridge Observatory; and the deduced places are as follows:

Greenwich Mean Solar Time.	Apparent R. A. of Comet.	Apparent N. P. D. of Comet.
h m s	h m s	° ′ ″
Nov. 29, 11 12 23	5 21 37·5	84 24 55
Dec. 8, 9 59 18	5 17 28·7	85 47 53
16, 11 55 45	5 13 33·0	86 35 55

Mr. Adams had previously computed the orbit by the method of Olbers, on the supposition of its being a parabola, but he found that the middle observation was so badly represented, that this

hypothesis could not be correct. He then proceeded to determine the elements without making any hypothesis as to the conic section, and the resulting elements are as follows :

Perihelion passage, 1843, October $26^d \cdot 33$ Greenwich mean time.

Longitude of Perihelion on the Orbit	54° 27′·8	From the equinox of Dec. 5.
Longitude of ascending Node	207 38·0	
Inclination to the Ecliptic	10 48·9	
Perihelion Distance	1·687	
Semi-axis Major	3·444	
Eccentricity	0·510	
Periodic Time	6·39	Sidereal years.

Motion direct.

The author suggests that the comet may, perhaps, not have been moving long in its present orbit, and that, as in the case of the comet of 1770, we are indebted to the action of *Jupiter* for its present apparition. In fact, supposing the above elements to be correct, the aphelion distance is very nearly equal to the distance of *Jupiter* from the sun : also the time of the comet's being in aphelion was 1843·8 — 3·2 = 1840·6, at which time its heliocentric longitude was 234°·5 nearly, and the longitude of *Jupiter* was 231°·5 ; and, therefore, since the inclination to the plane of *Jupiter's* orbit is also small, the comet must have been very near *Jupiter* when in aphelion, and must have suffered very great perturbations, which may have materially changed the nature of its orbit.

VII. Observations of the Comet of Faye. By C. Rumker, Esq. Communicated by Dr. Lee.

Day, 1843.	Mean Time at Hamburg.	App. Right Asc. of the Comet.	App. N. Dec. of the Comet.	Number of Observations.
	h m s	° ′ ″	° ′ ″	
Dec. 1	10 33 46·0	80 11 55·78	5 14 44·4	14
1	12 39 53·3		5 13 52·2	Merid. Circle.
9	10 41 9·4	79 14 42·89	4 4 39·1	10
9	12 4 46·2		4 4 30·6	Merid. Circle.
10	9 52 15·2	79 7 18·72	3 58 8·2	22
11	9 51 5·1	79 0 7·74	3 51 37·2	17
13	8 30 46·5	78 45 45·10	3 39 56·7	12
15	11 45 26·0	78 30 22·45	3 29 11·2	11
15	12 17 58·0		3 28 40·8	Merid. Circle.
17	8 4 49·6	78 18 3·67	3 20 42·1	17

VIII. Observations of the Comet of Faye, made at Starfield. By W. Lassell, Esq.

The author thinks that the observations given may be relied upon to within one second of time, and eight or ten seconds of declina-

tion. They were made with the 9-feet equatoreal, used differentially, comparing the place of the comet with the stars 23 and 30 *Orionis*, and one or two small stars near them.

The following are the resulting places:—

Mean Time of Observation.	Apparent Right Ascension.	Apparent Declination.
1843 h m	h m s	° ′ ″
Dec. 12 9 57	5 15 29·1	+3 44 46·4
13 10 30	5 14 59·7	3 39 1·6
14 10 58	5 14 30·7	3 33 38·0
22 10 38	5 10 56·0	3 5 25·0

IX. The following Communications respecting the great Comet of 1843:—

1. Observations of the Comet, made by J. Burdwood, Esq., Master of H. M. Sloop Persian. Communicated by G. B. Airy, Esq.

The comet was seen very distinctly for several successive evenings in March, while the vessel was cruising off the western coast of Africa, between 0° 40′ east, and 0° 13′ west longitude; and between 5° 10′ and 5° 30′ north latitude. The following distances were observed with the sextant on the evening of March 7, at $7^h\ 10^m$ P.M.:—

Distance from Aldebaran	60° 29′
— Canopus	75 57
— Sirius	84 0
Length of Tail	27 25

2. Remarks on the Comet, as seen on Board the Lawrence, of Liverpool, on her passage from Sidney to Conception. By a Passenger. Communicated by W. Simms, Esq.

The comet was first seen on the 1st of March, at 8½ P.M., as a white streak of light, inclined at an angle of 40° to the horizon, and was imagined to be the zodiacal light. It was again seen on the 6th, when the tail was 50° in length, in two streams of light, the outside edges being clear and well defined. On the 9th, the nucleus was seen, and appeared as bright as stars of the third or fourth magnitude. It was seen at intervals till the 28th of March.

3. Abstract of an Article in *Silliman's Journal*, containing an Account of Observations of the great Comet, made near the time of its Perihelion Passage. By J. G. Clarke, Esq., of Portland.

Mr. Clarke measured the distance of the nucleus from the sun on the 28th of February, and states, that the nucleus and every part of the tail, as seen by him in strong sunshine, were as well defined as the moon on a clear day, and resembled a perfectly pure

white cloud, without any variation, except a slight change near the head, just sufficient to distinguish the nucleus from the tail at that point. The denseness of the nucleus was so great that Mr. Clarke has no doubt that it might have been visible upon the sun's disc, if it had passed between it and the observer. This apparent density he attributes to the foreshortening of the tail, and its being so directed to the earth that the nucleus must have been seen through a considerable mass of the matter of the tail. The following distances were measured with a reflecting instrument:—

	d	h	m	s				°	′	″
Feb.	28	3	2	15	P.M.	Distance of	Sun's farthest Limb from nearest Limb of Nucleus	4	6	15
	„	3	6	20	P.M.	„	Sun's farthest Limb from farthest Limb of Nucleus	4	7	30
	„	3	9	40	P.M.	„	Sun's farthest Limb from Extremity of Tail.........	5	6	30

Mr. Clarke supposes the first of these measures to be correct within 15″; the other two are given as near approximations. Allowance must, of course, be made for the motion of the two bodies during the time of observation. When the sun was on the meridian, the angle made by the line joining the centres of the sun and the nucleus with the lower vertical, on the eastern side, was about seventy-three degrees.

X. On the Deducing of the Parallax of *Mars*, and hence that of the Sun from the Geocentric Motion of the former when in opposition, and especially when near the Node of his Orbit. By S. M. Drach, Esq.

The author, after alluding to the method of determining the solar parallax from observations of the transits of the inferior planets over the sun's disk, states his method as follows:—

"The counterpart of the above is the simultaneous observation at different points of the earth's surface, of the time occupied by a superior planet, when near opposition and near the node, in passing through a certain interval of space, say about half a degree (the sun's diameter); but as this happens at night, comparison stars are to be used, and the interval assumed to be nearly equivalent to their distance. Thus, *e.g.*, if *Mars* be the object observed, and at Greenwich x minutes are occupied by it in describing an arc which it requires only y minutes to describe at the Cape of Good Hope, then will the difference $x-y$, properly applied, give the parallax of *Mars*, and hence that of the sun."

XI. A Letter from Sir J. F. W. Herschel, Bart., to Mr. Baily, dated 6th Sept., 1842, on the Increase in Magnitude of the Star η *Cygni*.

"I beg to call your attention to the star η *Cygni* (21 *Cygni*, Fl.; Piaz. xix. 344), which appears to have increased in magnitude very considerably since the date of Piazzi's observations. It is

now the principal star in the neck of the *Swan*, and of nearly the fourth magnitude,—very conspicuous to the naked eye, and marking, in fact, the only *very* distinctly seizable point between *Albireo* in the beak, and the bright star γ in the body. Now, Piazzi, from nineteen observations in right ascension, and eleven in declination, sets it down as of the 5·6 mag. It does not occur in the Astronomical Society's Catalogue. The star b^2 *Cygni*, which does occur in that Catalogue, is there set down as of the 5th mag. which is also what I make it, or, rather above than under; but η is now a much more distinguished star.

" I may also take this opportunity to mention that the star 34 *Cygni*, the celebrated variable star discovered by Janson in 1600, whose period is 18 years, is now at or near its maximum; at least, it is a star of full the 5th mag. and very nearly equal to b^2 and b^3.

" Bode, on the authority of Lalande, has placed in his maps a star of the 4th mag., with the letter *i* attached, near η *Lyræ*. I can find no star in the place laid down visible in an opera-glass. It is the star 153 *Lyræ*, of Bode's Catalogue.

" I cannot but suspect several other stars in this constellation of variation; at least, I find the greatest discordance between the actual aspect of many regions within its extent and the magnitudes as laid down by Bode. Harding's maps, however, agree better. In Harding's, however, η is marked of the same magnitude with b^2."

London:—Printed by Moyes and Barclay, Castle Street, Leicester Square.

ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *February* 9, 1844. No. 4.

Report of the Council of the Society to the Twenty-fourth Annual General Meeting, held this day.

The annual return of the General Meeting of the Society affords the Council the usual opportunity of congratulating the Fellows not only on the favourable position of our own Institution, but also on the increasing advancement of the science of Astronomy in general. New works have been published, and others are now in progress, that must tend to promote our favourite study; and instruments have been formed of an entirely new construction, and others are still being constructed, which in due time will be brought to bear upon many important points of our system, and thus lead to the improvement of practical and theoretical astronomy.

The Report of the Auditors, which is subjoined, will shew the state of the finances of the Society:

RECEIPTS.

	£.	s.	d.
Balance of last year's account	261	11	4
1 year's dividend on £900 Consols	26	4	4
1 year's ditto on £2061 13s. 4d. New 3½ per Cents	70	1	2
On account of arrears of contributions	65	2	0
62 annual contributions (1843–44)	130	4	0
2 ditto (1844–45)	4	4	0
6 compositions	126	0	0
4 admission fees	8	8	0
1 first year's contribution	2	2	0
Sale of Memoirs, by Mr. Hartnup, from Jan. 27, to Nov. 9, 1843	125	11	0
Ditto by Mr. Harris, from Nov. 9, 1843, to Jan. 22, 1844	5	12	6
	£825	0	4

EXPENDITURE.

	£.	s.	d.
Purchase of £123 4s. New 3½ per Cents	126	0	0
Moyes and Barclay, for printing Memoirs, Vol. XIV.	298	1	7
Ditto, for printing Monthly Notices, &c. for Session 1842–43	38	19	6
W. Rumfitt, for bookbinding, &c.	11	0	9
Carried over	£474	1	10

EXPENDITURE (*continued*).

		£.	s.	d.
Brought forward		474	1	10
W. Wyon, for 3 Gold Medals and cases		35	0	6
J. Hartnup, 11 months' salary as assistant-secretary, from Dec. 9, 1842, to Nov. 9, 1843		73	6	8
R. Harris, 2 months' salary as assistant-secretary, from Nov. 9, 1843, to Jan. 9, 1844		13	6	8
J. Hartnup, for commission on collecting £309 1*s*.		15	9	0
R. Harris, for commission on collecting £32 18*s*. 6*d*.		1	12	11
Charges on books, and carriage of parcels		2	18	4
Postage of letters		17	8	0
Porter's and charwoman's work, &c.		8	7	6
Tea, sugar, cakes, &c. for the evening meetings		13	13	0
Coals, candles, &c.		12	13	6
Sundry disbursements by the Treasurer		13	3	3
Taxes: Income tax	1 9 2			
Poor's rate	4 17 9			
Church rate	0 17 0			
Land tax	3 2 6			
Window duty	5 4 9			
		15	11	2
Balance in the hands of the Treasurer (Jan. 22, 1844)		128	8	0
		£825	0	4

The assets and present property of the Society are as follow:

		£.	s.	d.
Balance in the hands of the Treasurer		128	8	0
3 contributions of 5 years' standing	£31 10 0			
5 ——— of 4 ditto	42 0 0			
7 ——— of 3 ditto	44 2 0			
16 ——— of 2 ditto	67 4 0			
36 ——— of 1 ditto	75 12 0			
		260	8	0

£900 3 per Cent Consols.
£2123 7*s*. 1*d*. New $3\frac{1}{2}$ per Cent Annuities.
2 Gold Medals unappropriated.
Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

But, besides this property, there is another sum invested in the public funds, which is called "The Lee Fund," out of respect to the original donor, Dr. Lee, who in the year 1834 invested 100*l*. sterling in the 3 per Cent Reduced Annuities in the name of this Society, with a request that the interest might be given, by the Council, to the widow or orphan of any deceased Fellow or Associate of the Society who might stand in need of it. The amount of stock purchased was 109*l*. 8*s*. 9*d*.; and the dividends have on several occasions been bestowed on parties by whom it has been most thankfully received. As the Society are only Trustees for this sum, the dividends have not been brought into the annual account, but are appropriated from time to time, as circumstances arise, without any exposure of the names of the parties who may solicit the relief. The last appropriation has been recently made, so that there is, at present, no accumulation of dividend.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be best seen from the following abstract, continued from the Report of last year, viz.:

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1843	94	127	78	6	305	36	341
Since elected	3	1	...	...	4	...	4
Deceased...............	...	−2	−1	−1	−4	−3	−7
Resigned...............	...	−1	...	...	−1	...	−1
Removals	+3	−3	...	...	...	...	...
February 1844	100	122	77	5	304	33	337

With respect to the instruments of the Society, it may be proper to place on record the present state of them, viz.:

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* quadruple portable sextant,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Beaufoy* clock,
The *Herschelian* 7-feet reflector,
The *Greig* universal instrument,

are in the apartments of the Society.

The brass quadrant, said to be *Lacaille's*,

is in the apartments of the Royal Society, for safe custody.

The Standard Scale

is under the care of the Astronomer Royal, and with the consent of the Council, is in actual use for the formation of a new standard measure, under the direction of the Standard Commission.

The *Cavendish* apparatus

at present remains in the possession of Mr. Baily.

The remainder of the instruments are lent, during the pleasure of the Society, to the several parties undermentioned, viz.:

The *Fuller* theodolite, to the Lords of the Admiralty,
The other *Beaufoy* clock, } to the Royal Society.
The two invariable pendulums, }
The *Lee* circle, to Lord Wrottesley.
The *Wollaston* telescope, to Professor Schumacher.

The Council always regret when they have cause to announce the decease of any of the Fellows or Associates of the Society. On the present occasion they have to lament the death of seven of their number: namely, His Royal Highness the Duke of Sussex,

Professor Wallace, Mr. William Allen, and Mr. Arnold, on the home list; and Messrs. Bouvard, Cacciatore and Hassler, amongst the associates.

His Royal Highness Augustus Frederick, Duke of Sussex, K.G. &c. departed this life on the 22d of last April, in his seventy-first year. He was the sixth son of George III. and early devoted his mind to intellectual culture, passing a more than respectable career in the University of Gottingen. Hence his learning and accomplishments were very considerable; and what might be deficient in profundity, was compensated by strong perceptive faculties. He was of independent opinions, and unwearied in his advocacy of civil and religious liberty. For nearly half a century he was the persevering and zealous patron of every really charitable institution, every useful scheme, and every benevolent project. When the late Mr. Davidson left our shores for the dangers of a journey to Timbuctoo, "Bring this gentleman safely home again," said the Duke to Abou Bekr, the Negro companion, "and you shall have an asylum in this palace for the rest of your life."

As President of the Society of Arts, he was remarkable for the suavity and appropriate addresses with which he distributed the adjudicated prizes; and his eight years' presidency of the Royal Society was conducted with such general satisfaction, that his resignation was received with unfeigned regret. His extensive acquaintance with scientific and literary men, native and foreign, manifested a decided natural taste for their pursuits, and a just appreciation of their value. In his intercourse with the Fellows, he was kind and attentive without ostentation, and affable without condescension.

Although his means were by no means commensurate with his high station, his Royal Highness collected a splendid library, rich in every department of science and learning, and unexcelled in its biblical department. He enjoyed, in a high degree, the respect and regard of the British public; and this feeling was shared even by those who differed most strongly from the views which guided his course in political affairs. His, however, were hardly to be called political leanings, for his liberal aspirations emanated from a truly benevolent heart, wishing to promote the physical comfort and mental improvement of his fellow-creatures.

The cause of humanity has also experienced another serious loss in the death of Mr. William Allen, a member of the Society of Friends, President of the Pharmaceutical Society, and one of the original Members of this Society. This gentleman was born on the 19th of August, 1776, in London, and soon entered upon the useful profession of Chemistry. In this branch of knowledge he made such advances as to be identified with its scientific progress; and he became a distinguished Professor of Experimental Philosophy at Guy's Hospital, and the Royal Institution of Great Britain. He was connected with some of the nicest experiments of the day, in conjunction with Davy, Babington, Marcet, Luke Howard, and Dalton. Together with Mr. Pepys, he made the well-known inves-

tigations upon atmospheric air, and other gasses, which are recorded in the Philosophical Transactions; and in which he proved the identity of the diamond with charcoal.

What, however, distinguished him here, was his taste for Astronomy, as evinced by his elegant private observations, and his extensive astronomical library. In 1815, he published a neat little work, intituled *A Companion to the Transit Instrument*: it contains all the stars, from the first to the fourth magnitudes, together with the places of some of the most interesting of the double-stars, and a few nebulæ.

Mr. Allen united, in a remarkable degree, sound knowledge, suavity of manners, and sterling principle; and he deservedly possessed the esteem of all who knew him. He pursued his views with persevering industry; and travelled far and near in his search after the means of advancing the moral and religious education of the poor. He died on the 30th of last December, at Lindfield in Sussex, a scene of his zealous benevolence; for it was here that he endeavoured to shew how much aid might be afforded to the work of education, by the earnings of the scholars: and, with co-operation, he founded a colony of labourers in that neighbourhood, to whom he allotted land at a remunerating but easy rent, in order to improve their moral and physical condition.

Such was William Allen, whose life was devoted to the best interests of mankind, and whose name is known wherever the efforts of humanity are in activity.

Professor Niccolò Cacciatore was born at Castel-termine in the southern part of Sicily, of respectable parentage, on the 26th of January, 1770. In his boyhood he was educated by his paternal uncle, a Professor of Theology in Girgenti. At the age of 17 years he went to Palermo, and, under the care of the Canon De Cosmis, his townsman, he devoted himself to the study of Greek and the belles lettres. He then proceeded to study mathematics and the physical sciences without the assistance of a master. Piazzi became acquainted with him in the house of De Cosmis, and, surprised at his progress, took him to the Observatory to perfect him in his favourite study, and has made favourable mention of him in the preface to the Palermo Catalogue. In September 1798, he went to reside entirely with Piazzi, and there occupied himself exclusively with astronomy, so that, in 1800, he was appointed assistant at the Royal Observatory, and assisted in completing Piazzi's great Catalogue above-mentioned.

During the years 1803, 4, and 5, he repeated the observations on Maskelyne's 36 fundamental stars, which labour was published in the Sixth Book of the Royal Observatory of Palermo. On these results Piazzi wished to found a new catalogue, but suffered so much in his eyes in 1807, that he confided it to Cacciatore, and the work was published in 1814; on which he was honoured with Lalande's gold medal from the Royal Institute of France.

In 1810, he had been elected general examiner of the *Corpi Facoltativi* of Sicily, with the charge of instructing, in the higher

geodesy, the officers of the Topographical Office. In 1814, he was declared honorary professor in the Royal University. In 1817, Piazzi being called to Naples to direct the new Observatory there, Cacciatore was promoted to the direction of the Royal Observatory at Palermo. In 1819 accordingly, his observations and calculations of the fine comet of that year were published in his name.

But, in 1820, whilst he was assiduously prosecuting his researches on the proper motion of the stars, and on the thermometric cause of the observed difference of the obliquity of the ecliptic between summer and winter, the revolution occurred; when his house was pillaged, as well as the library of the Observatory, although in the Royal Palace; so that he lost every thing, even his manuscripts being partly carried off or destroyed. In trying to save the Observatory, he with difficulty saved his life, being with the greatest indignity thrust into prison. Yet in December of the same year he was deputed, as representative of Palermo, to the Parliament at Naples. He was elected an Associate of the Royal Astronomical Society of London, and was one of the 40 Associates of the Italian Society established in Modena. He was general secretary of the Academy of Sciences in Palermo, which he improved by introducing appropriate regulations.

In 1810 he married Emmanuela Martini, by whom he had five children. He had long felt a chronical affection of the brain, and finally suffered so much from the cholera that he was obliged to give up his astronomical labours, though he lingered until the 28th of January, 1841, when at two in the morning he expired in the arms of his wife and children. His son, Gaetano Cacciatore, is appointed his successor in the Observatory.*

The following is a list of his published works:

On the Comet of 1807.
On the Comet of 1819.
New Reflecting Circle of Simonoff, 1824.
Geognostic Observations on Monte Cuccio, 1824.
Geognostic Researches on Monte Cuccio, 1825.
Letter to Zach on Terrestrial Refractions, &c., 1825.
On Meteorology, 1825.
A Discourse on the Origin of the Solar System, 1826.
Letter to Visconti on some errors in Piazzi's Catalogue, 1827.
On the Barometric Rise in January 1828, in a Letter to Ferrusac.
Observations on Monte Cuccio, 1828.
Mineral Baths of Sclafani, 1828.
On Comparative Meteorology, in Latin, 1832.
Ditto, in Italian, 1832.
On the Summer Heat of Palermo, 1833.
On the Sirocco at Palermo, 1833.

* See further accounts of Niccolò Cacciatore in the *Annali Civili* of Sicily, No. 49, for January and February, 1841, page 72.

Difference of Longitude between Naples and Palermo, 1834.
On the Expected Return of Halley's Comet, 1835.
On Goniometry and Spherical Trigonometry, 1837.
On the Solar Spots, 1839.
Besides various articles in Periodicals and Scientific Correspondences.

William Wallace, LL.D., late Professor of Mathematics in the University of Edinburgh, was descended from a family in humble circumstances, which had been settled, for some generations, at the village of Kilconquhar in Fifeshire. His grandfather inherited a small property, the greater part of which he lost through injudicious management. His father established himself at Dysart, a sea-coast burgh in Fife, as a manufacturer of leather and shoes for exportation, and for some years carried on a considerable trade, which, however, was ruined by the breaking out of the American war. The subject of this memoir was born at Dysart on the 23d of September, 1768, and was the eldest of a numerous family.

In adverting to the circumstances of his early life, he used to relate that the first rudiments of his education were received from an aged widow in the town, who kept a school for children, and retailed small wares. About the age of seven, he was removed to a school of a better class, in which the principal branch of instruction was arithmetic. In this science, however, he had already been grounded by his father, and had made considerable proficiency in it before he was advanced to that department in the routine of his school progress. His attendance at school—for instruction it can scarcely be called—was discontinued when he had reached the age of ten or eleven years; and, according to his own statement, all he owed to the schoolmaster was the power of reading, and of forming, in a very indifferent way, characters by writing. His knowledge of arithmetic he owed to his father, and to his own strong liking for the subject.

In 1784, when in his sixteenth year, he was sent to Edinburgh to learn the trade of a bookbinder; and after a year or two of probation he entered upon a regular apprenticeship to this craft. But his passion for the acquisition of knowledge had been thoroughly roused by the perusal of some books which had fallen in his way; and, during the period of his apprenticeship, he devoted every spare moment to reading. These moments were, however, few. His master happened to be a person who had no sympathy with literary tastes, and no other concern about his apprentices than how to extract from them the greatest amount of labour. But his father, a man of considerable intelligence and strict religious principles, having removed with his family to Edinburgh, he had the comfort of residing, during this period, in the house of his parents, and the advantage of their society, encouragement, and moral superintendence, to which he professed himself to have been greatly indebted. His occupation, also, was in some respects favourable to the gratification of his tastes. Books of science

were constantly passing through his hands, and his curiosity could not be restrained from occasionally casting a glance at their contents. He had also acquired a few mathematical books of his own; and such were his ardour and enthusiasm in their study, that it was his constant practice to take his meals with one of them in his hand, and to carry one in his pocket, to read on his way to and from the workshop. By this assiduous application, before he reached the age of twenty, he had read and made himself master of Cunn's Euclid, Ronayne's Algebra, Wright's Trigonometry, Wilson's Navigation, Emerson's Fluxions, Robertson's Translation of La Hire's Conic Sections, and Keill's Astronomy. Of these books he cherished the remembrance, as the means by which he had been enabled to grope his way into the region of the mathematics.

Hitherto, Mr. Wallace's efforts to acquire knowledge had been made under the most disadvantageous circumstances; without sympathy from any one but his father, and without a companion or friend to appreciate his exertions or applaud his success. But he was now approaching the turning point of his fortunes. He happened to become acquainted with an elderly person, a carpenter by occupation, who was employed by the celebrated Dr. John Robison, the Professor of Natural Philosophy, as an assistant in his class experiments. This man, though a great reader of books, was no mathematician; but he had sat too near the feet of Gamaliel not to have imbibed a respect for the science, and for the pursuits of his young friend. With an excusable vanity, he was in the habit of boasting of his intimacy with the professor, to whom he proposed to introduce Mr. Wallace. The latter, however, with great good sense, declined the kindly meant offer until the term of his apprenticeship had expired, when, though still with some diffidence and hesitation, he was prevailed upon to take advantage of it. Armed with a letter from his humble patron, he waited upon the professor, who received him with great kindness, examined him with respect to his proficiency in geometry and the conic sections, and inquired into the circumstances of his life, and the means by which he had made so much progress in the mathematics. In the course of the conversation Dr. Robison considerately took occasion to warn him that the study of mathematics was not likely to lead to any thing advantageous in the world: the reply was, that he was aware of the fact; but being, as it seemed, doomed to a life of labour, he hoped to sweeten the cup by the pleasure to be derived from the possession of knowledge. The interview ended with an invitation from the Professor to attend the course of lectures on Natural Philosophy then about to begin. Sensible as he was of the advantages which he could not fail to derive from such instruction, it required no small sacrifice on his part to accept the offer; for, being then employed as a journeyman, the time thus occupied could only be commanded by the abstraction of an equal portion from his hours of rest or sleep. Every difficulty, however, gave way before a determined will. The class was diligently attended;

and he has been heard to say, that if he were asked which had been the happiest period of his existence, he would refer to that at which he attended the lectures on natural philosophy, when, for the first time in his life, he had the means of receiving sound instruction, and found himself in the company of young men devoted to the pursuit of knowledge.

Dr. Robison's next act of kindness was to introduce him to his colleague, Mr. Playfair, the Professor of Mathematics. Mr. Playfair was no less struck with the extent of his acquirements, and likewise offered him admission to the mathematical class. But attendance on two classes in one day being, in his circumstances, entirely out of the question, he was under the necessity of declining the offer, much, it may readily be believed, to his regret. Mr. Playfair, however, from this first interview, took a warm interest in his welfare, advised him with respect to the course of reading he should follow, supplied him with books from his own library, and continued his stedfast friend through life.

These details may appear trivial, or unnecessarily minute; but it can never be wholly uninteresting to trace the steps by which distinction in science or literature has been obtained when opposed by obstacles which might seem, and in ordinary cases prove to be, insurmountable. To the individual we are commemorating they were all-important: some may receive encouragement from his example; and science itself is placed in an advantageous light when we see men so eminent as Professors Robison and Playfair taking trouble with, and giving help and encouragement to, a friendless young man, who had no claim on their good offices, and no other recommendation to them, than his successful struggles in acquiring the elements of those sciences which they themselves cultivated with such distinguished success. On the other hand, the merit must have been of no ordinary kind which, to persons of their experience, appeared so remarkable.

About the time he was attending Dr. Robison's lectures he was induced, by the prospect of having the command of a greater portion of time than had yet been at his disposal, to exchange his occupation for that of warehouseman in a printing-office. While in this occupation Dr. Robison paid him a visit, and proposed to him to give private lessons in geometry to one of his pupils. This proposal opened up a new prospect to him, and admitted the first gleam of hope of his being able to emancipate himself from the drudgery of manual labour. He now also began to acquire a knowledge of Latin, and in this, as in the study of mathematics, his manner of turning time and opportunity to account may afford encouragement to those who are in pursuit of knowledge under difficulties. A part of his duty in the printing-office was to collect the successive sheets of a work from a series of heaps arranged round a circuit of tables. While engaged in this monotonous occupation, he fixed up upon the wall a Latin vocabulary, from which he committed to memory a certain number of words every time he passed it in making his round. In his study of Latin,

however, he received assistance from a student, to whom, in return, he gave instruction in mathematics.

After he had been engaged a few months in the printing-office, he entered into the employment of one of the principal booksellers of Edinburgh in the capacity of shopman. This change was advantageous in several respects. His circumstances were now considerably improved, and he found leisure, not only to pursue his favourite studies, but to increase his stock of knowledge by general reading, and even to give private lessons in mathematics in the evenings. While in this situation he contrived to get a few lessons in French, and commenced his acquaintance with the works of the Continental mathematicians.

In 1793, while in his twenty-fifth year, he took the resolution to give up his employment, and support himself by teaching mathematics privately. This plan probably succeeded to the full extent of his moderate expectations. He now attended a course of lectures by Professor Playfair; and although, as the course was intended for an audience far behind him in mathematical acquirements, he had little to learn, the example of Playfair's manner—dignified, eloquent, and impressive, in a degree rarely equalled—was of great use to him in after-life. At the same time he also attended a course of chemistry, and by assiduous diligence endeavoured to repair, to the utmost of his power, the deficiencies of his early education.

In 1794, Mr. Wallace, on the recommendation of Professor Playfair, was appointed to the office of assistant teacher of mathematics in the academy at Perth. In respect of emolument the appointment was of no great value, but it gave him a settlement in life, with reasonable leisure to prosecute his mathematical studies, of which he did not fail to take advantage. In 1796, he presented his first memoir to the Royal Society of Edinburgh, entitled, "Some Geometrical Porisms, with Examples of their Application to the Solution of Problems." This paper, which contained some new and curious porismatic propositions, afforded ample proof of original and inventive powers; while his manner of conducting the investigation shewed how accurately he had imbibed the spirit and methods of the ancient geometrical analysis. About the same time, on the request of Dr. Robison, he contributed the article "Porism" to the third edition of the *Encyclopedia Britannica;* and, a few years later, when a new and greatly enlarged edition of that work was undertaken, he was enlisted as a regular contributor, and undertook to furnish the principal mathematical papers.

During the vacations of the Perth academy he paid regular visits to Edinburgh, where he continued to cultivate the friendship of Robison, Playfair, and other scientific men, to whom his now recognised talents and mathematical attainments procured him introductions. The first mark of literary distinction he received was that of Corresponding Member of the Edinburgh Academy of Physics; a society which, though not known by its published transactions, was at that time remarkable by reason of the cluster of talented persons of whom it was composed, several of whom have since

attained the highest distinction in literature, philosophy, and public affairs. Such association could not fail to have a powerful effect in the developement of his mind, even though his residence at a distance from Edinburgh prevented him from attending many of the meetings.

In 1802, he presented a second paper to the Royal Society of Edinburgh, containing a new method of expressing the coefficients of the developement of the algebraic formula which represents the disturbing effect of the mutual action of two planets on each other. This was a contribution of great merit, and, immediately upon its publication, established his reputation as a mathematician of the first order. The volume of the *Transactions* in which it appeared was reviewed in the second number of the *Edinburgh Review*; and an able analysis of Mr. Wallace's paper was concluded with the following encomium: "We cannot conclude without expressing our sincere admiration of this excellent performance—excellent in every respect; and, trifling as it may appear to mathematicians, remarkable for a pure, perspicuous, and not inelegant style. It is a paper, equal, in our opinion, to whatever has been most admired of the greatest analysts. We remember nothing in the works of Euler or Lagrange which belongs to a higher order of excellence in the science." Mr. Wallace's method of developement depended ultimately upon the proportions which the perimeters of two ellipses bear to those of their circumscribing circles; and in order to facilitate its application, he gave, in an appendix, a very beautiful and quickly converging series for the rectification of the ellipse, applicable to every case of eccentricity, and to every length of an arc that can possibly occur in calculation. His merit with respect to this paper cannot be considered as having been diminished by the discovery he made some time after its publication, that in certain respects he had been anticipated by Legendre. The very little intercourse which at that time existed between this country and France, and the position of the author in a remote provincial town, are sufficient excuses for his not having been more accurately acquainted with the state of mathematical discovery on the Continent.

Mr. Wallace had been for several years a contributor to some of the periodical publications in England in which mathematical questions were proposed, as *Leybourn's Repository*, the *Gentleman's Mathematical Companion*, and others of the same class. To this circumstance he attributed an incident which had an important influence on his future life. In 1803, he received a letter, under a feigned name, in which he was informed that an instructor in mathematics was wanted for the Royal Military College, then established at Great Marlow in Buckinghamshire, and recommended, if he thought of being a candidate for the office, to make an immediate application. Inquiry being made in the proper quarter, the information was found to be correct, but he ascertained also that it would be necessary to make his application in person. In matters affecting his own interests the disposition of his mind was not sanguine;

and, as in the present case he had no influence to employ, and no other recommendation to carry with him than his skill in mathematics, his chances of success appeared so small that he would have been deterred by the length and inconveniences of the journey from thinking more of the subject, had he not been encouraged by his friend Professor Playfair. On his arrival at the Military College he found there were several competitors; but the persons who had to decide on the respective qualifications of the candidates gave their decision in his favour, and he was accordingly appointed to the office.

Mr. Wallace held this appointment upwards of sixteen years, during which period, the whole of his leisure time was unremittingly devoted to scientific study and literary labour, the fruits of which appear chiefly in his numerous contributions to the two great Encyclopedias then publishing in Edinburgh. This species of writing, which is not particularly well adapted to form the basis of a permanent reputation, was in a manner forced upon him by the circumstances of his position. On his appointment to the Perth Academy he had married, and after he joined the Military College his family began to increase rapidly. The inconveniences he had suffered from the defects of his own early education rendered him only more solicitous that his children should not labour under any disadvantages in this respect, and, as they grew up, he placed them at schools in Edinburgh. His official income being insufficient for this expense, he was led to engage in the works now referred to, rather with a view to add to his means, and to enable him to discharge a sacred duty, than for the sake of any distinction he was likely to get by them. No individual, perhaps, was ever less influenced by considerations of a worldly nature, or more ready to bestow time and labour upon objects from which he could neither receive nor expect any remuneration whatever.

In 1808, he contributed a paper to the Royal Society of Edinburgh, entitled "New Series for the Quadrature of the Conic Sections, and the Computation of Logarithms," and containing some very remarkable formulæ for the rectification of circular arcs, with analogous expressions for the sectors of the equilateral hyperbola and the logarithms of numbers; all deduced from elementary principles, and without the use of the differential calculus or any equivalent method. At the time the paper was published, he believed the series to be entirely new, but he discovered afterwards that some of them had been previously given by Euler.

Mr. Wallace's services at the Military College were held in great estimation by the superior Officers, who frequently availed themselves of his practical sagacity in the adoption of regulations having respect not only to the course of instruction, but the general management of the establishment. One of the results of this deference to his recommendations (more particularly interesting to the Society), is the small observatory attached to the College, for the instruction of the officers of the senior department in practical astronomy. The plan of the building was originally furnished by

Dr. Robertson of Oxford; but the superintendence and arrangement of all the details of construction were confided to Mr. Wallace, who visited most of the Observatories in the neighbourhood of London, for the purpose of acquiring hints and information. A transit-instrument, an astronomical circle by Ramsden, a reflecting circle, and a clock by Hardy, were procured, and some other instruments were ordered, but countermanded from an apprehension of opposition to the estimates in the House of Commons. Although an Observatory of this kind cannot be expected to produce results of any direct advantage to astronomy in the present state of the science, it must still be regarded as no unimportant appendage to a national establishment for the instruction of Officers for the public service.

In 1819 a vacancy occurred in the Mathematical Chair of the University of Edinburgh, through the death of Professor Playfair, and the appointment of Mr. Leslie to succeed him in that of Natural Philosophy, and Mr. Wallace resolved on presenting himself as a candidate. The patronage belongs to the magistrates of the city, who, having in general no pretensions to be capable of estimating degrees of merit in abstract science, necessarily form their opinions from the testimony of others, or notions of general fitness, and are liable to be acted upon by influences of various kinds. In the present case a very keen contest took place; for another competitor (a man of general talent and great respectability, though unknown as a mathematician) was strenously supported by a strong political party. The struggle terminated, however, in his election by a large majority of the voters. This was the crowning object of his ambition. Ever since his appointment to the Perth Academy, he had fixed his regards on a professorship in a Scottish university as the goal of all his exertions; but his elevation to the Chair of the Gregorys, of Maclaurin, Matthew Stewart, and Playfair, probably did not enter at that period into his most sanguine anticipations.

Mr. Wallace had reached the age of fifty-one when he was appointed to the mathematical professorship in Edinburgh; but he still retained both mentally and bodily all the energy and activity of his younger years. He held the office till 1838, when he resigned on account of ill-health, having been unable to perform his duties in person during the three previous sessions. Upon his resignation the honorary title of Doctor of Laws was conferred upon him by the University, and at the same time he received a pension from Government which he enjoyed during the few remaining years of his life, in consideration, as the warrant stated, of his attainments in science and literature, and his valuable services, up to a very advanced period of life, first in the Military College, and afterwards at the University of Edinburgh.

For some years after his establishment at Edinburgh, a considerable portion of his time was occupied in the preparation of his lectures, on which he bestowed great pains. When the new edition of the *Encyclopedia Britannica* was commenced, he undertook

the revision of all the mathematical papers he had contributed, as well as some of those which had been written by Dr. Robison; and several of the more important treatises, particularly, Algebra, Conic Sections, and Fluxions, were remodelled and almost entirely re-written. To the *Transactions of the Royal Society of Edinburgh* he contributed a paper, in 1823, on the Investigation of Formulæ for finding the logarithms of trigonometrical quantities from one another; one in 1831, entitled "Account of the Invention of the Pantograph, and a Description of the Eidograph;" and one in 1839, on the Analogous Properties of Elliptic and Hyperbolic Sectors. His last contribution to that Society, published in Vol. XIV. of the *Transactions*, was entitled, "Solution of a Functional Equation, with its Application to the Parallelogram of Forces, and to Curves of Equilibration." This paper, in addition to the investigation of series adapted for calculation, contains a set of tables, to ten decimal places, of the corresponding values of the amplitude, ordinate, and arc of a catenary, which are important in an engineering point of view, as they afford the data required for constructing arches having the forms of equilibrated curves. Similar tables, to eight places, had previously been given by Mr. Davies Gilbert in a paper on the mathematical theory of suspension bridges, in the *Philosophical Transactions* for 1826; but the numbers were found by Mr. Wallace to be erroneous, generally, in the three last decimal figures.

Mr. Wallace is the author of a paper in Vol. IX. of our *Memoirs* containing two elementary solutions of Kepler's problem by the angular calculus. In the *Transactions of the Philosophical Society of Cambridge*, Vol. VI., there is also a paper by him under the title of "Geometrical Theorems and Formulæ particularly applicable to some Geodetical Problems." For this subject he had a particular predilection; and in 1838, while confined to a sick-bed, he composed, and afterwards published at his own expense, a separate work entitled, "Geometrical Theorems and Analytical Formulæ, with their Application to the Solution of certain Geodetical Problems." This volume, which he appropriately dedicated to his friend Colonel Colby, contains the substance of his paper in the *Cambridge Philosophical Transactions*, with the addition of a considerable number of extremely elegant formulæ, most of them new, and some of them important in the practice of the higher geodesy.

Professor Wallace took great delight in all the practical applications of his science, and had a strong turn for mechanical invention. His attention having been directed to the imperfections of the Pantograph, he invented, in 1821, an instrument on a different principle to supply its place, to which he gave the name of *Eidograph*. This instrument answers the same purposes as the common pantograph, to which, however, it is greatly superior, both in the extent of its applications and the accuracy of its performance; for, while the similarity of the copy to the original, in all its parts, is preserved with geometrical accuracy, the copy may be reduced or enlarged in almost any proportion; or, by a particular modifi-

cation of the instrument, it may even be reversed, and transferred immediately to metal or stone. This ingenious instrument, which would seem to be admirably adapted to the purposes of the engraver, was first described by him in Vol. XIII. of the *Edinburgh Transactions* to which reference has already been made. He has also described, in the Appendix to his *Conic Sections*, an *Elliptograph*, or instrument for describing an ellipse by continued motion, founded on a very beautiful property of the ellipse first pointed out, we believe, by him, namely, that the curve is organically described by any given point (not in the circumference) in the plane of a circle which rolls along the concave circumference of another fixed circle, the radius of which is twice that of the rolling circle. And in an Appendix to his *Geometrical Theorems* he has given the description of an instrument which he invented for the graphical solution of an important problem in surveying, viz. to determine the position of a station, having given the angles made by lines drawn from it to three other stations in the same plane, whose positions are known. This instrument, which he called a *Chorograph* (the problem which it solves having been proposed as a chorographical problem by Richard Townley in No. 69 of the *Philosophical Transactions*), is simple, compact, portable, and inexpensive; and in these respects has considerable advantages over the station-pointer, generally used for the same purpose.

Among the objects connected with the advancement of science to which Professor Wallace gave his aid, after his appointment to Edinburgh, there is one which it would be unpardonable to pass over without notice in this place,—we allude to the Observatory now established there. Ever since the time of Maclaurin there had existed a small astronomical observatory in Edinburgh, but no provision was made for regular observation, nor, indeed, did it contain any instruments fit for the purpose. Through the exertions, chiefly of Professor Playfair, funds were at length raised, by private subscription, for the erection of an observatory adapted for observations of the most accurate kind. Mr. Playfair did not live to see the building completed, or means provided for obtaining instruments, or carrying on systematic observations; but Mr. Wallace, on becoming his successor, entered fully into his views, and, in concert with a few other individuals, used all his influence and exertions towards bringing the scheme to maturity. At length, after years of expectation and delay, the Government was prevailed upon to take the observatory under its protection, furnish it with instruments of the first class, appoint an astronomer and assistant, and provide for the regular publication of the observations. In bringing about this arrangement, Mr. Wallace's aid and recommendation were of essential service ; and if any thing was wanting to complete the satisfaction which he felt at the result, it was to see the observatory placed under the care of his friend Professor Henderson, of whose distinguished merits as an astronomer it would be superfluous to speak to those who are in the habit of attending our meetings, or reading our *Memoirs*.

Although the works which Mr. Wallace has left behind him assure him a high place as an original and inventive mathematician, the talents with which he was endowed by nature were, doubtless, rendered less productive than they would have been by his want of early education, his residence during the best years of his life in the country at a distance from congenial society, and, perhaps, still more from the circumstance of so much of the time which his laborious public duties left at his disposal having been consumed in the preparation of his numerous treatises for the Encyclopedias. These treatises being mostly of an elementary kind, and composed for the purpose of explaining the principles of the various branches of mathematical science, afforded little scope for originality. They possess, however, all the qualities which give value to the class of writings to which they belong; being remarkable for lucidity and precision of style, perspicuity of arrangement, elegance of demonstration, and admirable adaptation for self-instruction. The article "Conic Sections" in the last edition of the *Encyclopedia Britannica* has been translated into Russian, and used as a text-book in some of the schools for the instruction of naval Cadets in that empire. It has also been published as a separate work, and is one of the most elegant geometrical treatises on the subject in existence. Some of his other articles, besides their intrinsic value, had the accessory merit of being the first which were published in this country on the model of the French school, when the French mathematics were greatly superior to our own. His article "Fluxions," in *Brewster's Encyclopedia*, was the first systematic treatise in our language in which the differential notation was used. The date of the publication is 1815; but, as a point of history, it may be worth remarking, that this notation had been adopted several years previously, both by himself and his illustrious colleague, Mr. Ivory, in their contributions to the *Mathematical Repository;* and some instances of its use occur in an English work of much older date, Harris's *Lexicon Technicum.*

Mr. Wallace had made himself intimately acquainted with every department of mathematical knowledge, but the branch which he cultivated with the greatest affection was the ancient geometrical analysis. Of this subject he was a perfect master. His taste having been formed by the writings of Simson, Stewart, and Playfair, he had an unbounded admiration of the elegance and correctness of the Greek geometry; and he took credit to himself for having introduced the *Elements of Euclid* to the Military College, and restored them, as a class-book, to the University of Edinburgh. Another branch in which he excelled was the angular calculus, which he enriched with various new series and methods of considerable importance to the computer. All his memoirs exhibit ingenuity and fertility of invention, excellent taste, and an intimate acquaintance with those parts of analysis with which they are connected in its most improved state.

The perspicuity and methodical arrangement which distinguish his writings were equally conspicuous in his academical prelections.

An intimate acquaintance with the history of scientific discovery, and the various applications of mathematical science, joined with a thorough knowledge of the particular subject under consideration, a retentive memory, and a ready invention, rendered his lectures eminently instructive. They were delivered without the slightest attempt at ornament or effect; but they seldom failed to place the subject before the student in a strong, clear, and full light, and were animated with a genuine zeal for the progress of his pupils and the advancement of his science. His Chair had been raised to a high degree of celebrity by a long line of illustrious predecessors, and it sustained, while occupied by him, no diminution either of efficiency or reputation.

Professor Wallace was not more distinguished by his mental endowments than for his moral virtues and private worth. In every relation of life his conduct was exemplary. In his family and domestic circle he was greatly beloved. In his general intercourse with the world he was upright, sincere, and independent. In society, his habitual cheerfulness and good humour, amiable manners, benevolent disposition, and a never-failing fund of anecdote, rendered him a delightful companion and a universal favourite. Generous and liberal in all his sentiments, he entertained no envy of the discoveries of his contemporaries; no jealousy of the reputation of younger men; but was ready at all times to applaud and encourage merit, wherever, and in whatever shape, it made its appearance. For such of his pupils as manifested any remarkable capacity or application he entertained an esteem almost amounting to affection; and he was always ready to use his influence, which was considerable, in order to forward their views in life or render them any service. In every measure affecting the public good, or the scientific renown of his country, he took a warm interest. He was the means of procuring a monument to be erected in Edinburgh to Napier, the celebrated inventor of logarithms; and the last occupation of his life was to investigate the administration of some of the public charities of the city.

Mr. Wallace was one of the original non-resident Fellows of this Society. He was also a Fellow of the Royal Society of Edinburgh; a Corresponding Member of the Institution of Civil Engineers; an Honorary Member of the Cambridge Philosophical Society; and a few weeks before his death he was elected an Honorary Member of the Royal Irish Academy. After an illness which had for several years prevented him from mixing in society, he died at his residence in Edinburgh on the 28th of April, 1843, in the seventy-fifth year of his age, respected by all, and sincerely regretted by a wide circle of personal friends.

The Fellows have been already informed of the resignation of Mr. John Hartnup, the Assistant Secretary to this Society, in consequence of his having been appointed Superintendent of an Astronomical Observatory recently established at Liverpool; for which situation Mr. Hartnup was well qualified, not only from his

former pursuits in a similar situation, but also from those habits of accuracy and that zeal for the science which he has always shewn. But, although the Council must at all times lament the loss of an active and intelligent officer, they congratulate the Society on having obtained a successor in Mr. Richard Harris, whose able assistance, on various occasions, at the Royal Observatory at Greenwich, has been highly spoken of by the Astronomer Royal, and augurs well for the benefit and advantage which the Society is likely to derive from his cordial co-operation.

The Fellows are probably aware that an Act of Parliament was passed in the last session, exempting certain Societies from any county, borough, parochial, or other local rates on the premises occupied by them for the purpose of science, literature, or the fine arts; provided, however, that a certificate of exemption be obtained and verified in the manner specified in such act. The requisite certificate for the present apartments of this Society has been obtained, and regularly registered; so that the Society is now exempt from the rates above alluded to, which they have hitherto been obliged to pay.

The Council have to regret that they have not yet been able to procure the Apartments on the basement story of the building, which they have so long been expecting; although there is no doubt that the rooms are at this moment wholly unoccupied. The President of the Society, as far back as the month of July last, addressed the Treasury on the subject; but at present he has not received any reply.

It is well known to most of the Fellows that the Council have, from time to time, proposed certain alterations in the Rules, or Bye-laws, of the Society, which have been regularly approved and sanctioned at the annual general meetings. A new edition of the Bye-laws being now required, it has been considered expedient to embrace the opportunity not only of embodying these alterations amongst the general sections and articles, but also of inserting a few additional alterations, in order to preserve a greater uniformity in the general wording and spirit of the Bye-laws. These additional alterations will be submitted to you for your consideration, and in case of your approval, will, with those previously sanctioned, be incorporated in their respective places, and thus become an integral portion of the general Bye-laws.

In the last annual Report, the Council stated that they had considered it advisable to alter the numerical typography, that had been so long in general use, by a return to the adoption of the old method of forming the Arabic figures, which had been so long laid aside. That measure has now been fully carried into execution by the Society, and the whole of the figures in the ensuing volume of the *Memoirs* will be formed in the manner suggested in that Report. The whole of the figures also employed in printing the three large catalogues of stars, now in the press, will consist of this new type; and it is hoped that these specimens of distinctness and legibility will soon come into more general use with the public, and ultimately tend to drive out the uncouth and scarcely legible

figures that have so long encumbered and deformed our numerical tables.

Since the last annual Report, the Council have had the gratification of accepting, on the part of the Society, and incorporating with the *Memoirs*, Mr. Baily's volume of Catalogues, containing the labours of Ptolemy, Ulugh Beigh, Tycho Brahé, and Hevelius, in that department of astronomy. This volume, as the Fellows are aware, is entirely the work of Mr. Baily, and is printed at his expense. The Council, in announcing this new obligation of the Society to Mr. Baily, feel that they only just need to remind the meeting, that the munificence of the present, in a pecuniary point of view, gratefully as it should be acknowledged, is not the point to which our acknowledgments should be most specially directed. Though the saving of expense has certainly prevented an unusual pressure on the Society's funds (for the Council would have felt bound to publish so valuable a communication, presented in the ordinary way), yet the state of the accounts which you have heard read to day, shews that the pressure would not have been unbearable. But the combination of knowledge and industry necessary to face the formidable task of collating, revising, and annotating, this collection of catalogues, with perfect unity of purpose and plan, it is not in the power of any collective body to command: had it been so, this useful labour would not have remained unperformed so long, notorious as was the difficulty, not only of consulting, but even of procuring, the catalogues in question. Nor must we forget, in connexion with this work, the edition of Flamsteed's catalogue which appeared a few years ago, in the life of that astronomer by Mr. Baily, who has thus given new access to the contents, new life to the history, new correctness to the matter, of all the most celebrated star-catalogues, from the earliest epoch of systematically recorded observation to the time when instrumental methods and corrections began to assume their present form. To this we must add that he has imposed new duties upon those who write and speak about the history of astronomy; errors and misconceptions inevitable in those who could only procure accounts of several of these catalogues at second-hand, are from this time unpardonable.

During the past year, nearly the whole of the terrestrial globe (embracing the several continents of Europe, Asia, Africa, and America) has been either gratified or alarmed at the appearance of one of the most splendid and extraordinary comets that have ever yet been placed on record. This comet does not seem to have been noticed prior to the time of its perihelion passage (February 27), but on the following day it was, in some places, seen nearly from the time of sun-rise to sun-set, and even at mid-day with the naked eye: a circumstance which affords the strongest proof of its great splendour. For many subsequent days it was the object of universal attention and admiration; and the numerous accounts of its appearance and the various observations and remarks thereupon, which have been received from all parts of the world, have

already been printed in the *Monthly Notices* of the Society. It seems to have excited much consternation in some places, but was universally remarked for its splendour, its velocity, and the great length of its tail, during the whole time of its visible existence, but more especially about the time of its first appearance.

Another comet, but not visible to the naked eye, has also recently made its appearance, and was first discovered by M. Faye at the Royal Observatory at Paris. Although this comet is far inferior in splendour to the one just mentioned, it is in another respect more interesting to the astronomer, inasmuch as it appears to be one that performs its revolution in little more than six years; thus adding another of those remarkable bodies whose periodical returns may ultimately tend to throw some light on the wonderful system of the universe.

In the annual Report of the Council in 1838, it was stated that a Committee had been appointed by the British Association to wait on the Government for the purpose of obtaining means to procure the reduction of all the lunar observations made at the Royal Observatory at Greenwich. That application was made and cheerfully responded to by the Government; and the funds adequate for this purpose were placed in the hands of the Astronomer Royal, by whose active superintendence and control this important and laborious operation has been at length brought to a close. The results of these computations are now almost ready for the press, and the public will soon be gratified by the publication of a body of information that must tend to throw considerable light on the elements of the lunar theory, and thus exhibit one of the most splendid proofs of the utility and advantage of this national establishment.

Connected with the same subject, and emanating in some measure therefrom, the Council are pleased in being able to announce that the Government has also complied with the request of the Astronomer Royal, to construct another building, attached to the Royal Observatory, for the erection of an altitude and azimuth instrument, intended for the sole purpose of making a greater and a more regular number of observations of the moon in various parts of her orbit; in order that all the practical means might be afforded for obtaining a more perfect knowledge of the lunar theory. The instrument here proposed is in some measure novel in its construction, and has been suggested by the Astronomer Royal, who anticipates certain advantages that are unattainable, or scarcely to be expected, in instruments of the usual form and construction. By means of this new instrument it is proposed that on every day of the year, when the state of the atmosphere will permit, the moon shall be observed at convenient hours, and in every suitable position, according to a plan suggested by the Astronomer Royal for securing the most important results.

Titles of Papers read before the Society, between February 1843 and February 1844.

1843.

Mar. 10. Occultations observed at Port Royal Dockyard, Jamaica. By Capt. Alexander Milne, H.M.S. Crocodile. Communicated by the Rev. George Fisher.

Observations of the Beginning and End of the Solar Eclipse on the 8th of July, 1842, in the Fort on the left bank of the Shanghai River, near the town of Woosung, on the Coast of China. By Capt. Sir Everard Home, Bart. Communicated by the Rev. George Fisher.

Translation of a Letter from M. Hansen to R. W. Rothman, Esq., accompanying a copy of a printed paper on the Perturbations of the Heavenly Bodies moving in very Eccentric and very Inclined Orbits.

On the Application of the Method of Least Squares to the Determination of the most probable Errors of Observation in a portion of the Ordnance Survey of England. By Thomas Galloway, Esq. A.M. F.R.S., one of the Secretaries of the Society.

A communication received by the President from the Rev. Baden Powell, on an easy and convenient Method of Imitating the Corona, or Glory, that surrounds the body of the Moon, during the time of total darkness, in Total Eclipses of the Sun.

April 12. Extract of a Letter from M. Bessel to Professor Henderson, dated November 14, 1842. Communicated by Professor Henderson.

Letter from Professor Henderson to the Secretary, dated March 23, 1843, on the great Comet of 1843.

Elements of the great Comet of 1843, with an Ephemeris, from the Observations of March 20, 22, and 25, reported in Professor Schumacher's Circular. By Professor Henderson.

Letter from Lieut. G. B. G. Downes, R.E. to Professor Henderson. Communicated by Professor Henderson.

Letter from Professor Henderson to the Secretary, on the great Comet of 1843.

Letter from T. Forster, Esq., dated Bruges, March 22, 1843, on the great Comet.

Second Letter from T. Forster, Esq., dated Bruges, March 28, 1843, on the great Comet.

Letter from J. Nasymth, Esq. to Sir J. F. W. Herschel, Bart., on an early Observation of the Train of the great Comet. Communicated by Sir J. F. W. Herschel, Bart.

Letter from M. C. L. Littrow, Director of the Observatory of Vienna, on the great Comet. Communicated by T. Galloway, Esq.

Two Circular Letters from Professor Schumacher, dated March 26 and March 31, 1843, on the great Comet.

Notices of the Comet from the Islands of St. Vincent's and St. Christopher's.

May 12. A Memoir on Astronomical Drawing. By Charles Piazzi Smyth, Esq. Communicated by Capt. W. H. Smyth, R.N.

On a Revision of the Boundaries of the Constellations, as usually drawn on Celestial Maps and Globes. By F. Baily, Esq., President of the Society.

Account of an Observation of the Solar Eclipse of the 7th of July, 1843, made at Starfield Observatory, together with an Account of some Chronometrical Experiments made to determine the Longitude of that Observatory. By W. Lassell, Esq.

Professor Schumacher's third, fourth, and fifth Circulars on the great Comet of 1843.

Circular Letter from Professor Schumacher on a Telescopic Comet discovered by M. Mauvais on May 3, 1843.

June 9. On a Self-acting Dividing Engine. By William Simms, Esq.

Recomputation of Roy's Triangulation for connecting the Observatories of Greenwich and Paris. By W. Galbraith, Esq.

Occultations of Fixed Stars and the Planet *Jupiter* by the Moon. Observed at Hamburgh by C. Rumker, Esq.

Notes on the Appearance of the great Comet of 1843, made during a Voyage from the Cape of Good Hope to England. By M. Close, Esq., Commander of the Ship Ellenborough.

A Letter from John Belam, Esq., Master of H. M. Sloop Albatross, on the great Comet of 1843. Communicated by G. B. Airy, Esq., Astronomer Royal.

Observations of the Comet by S. C. Walker, Esq. and Professor Kendall, at the Observatory of the High School at Philadelphia. Communicated by Lieut.-Col. Sabine.

Notes on the Comet, accompanied by a Pencil Sketch. By Capt. Hopkins, commanding the East India Company's Ship Seringapatam, on a voyage from the Cape of Good Hope. Communicated by Sir J. F. W. Herschel, Bart.

Extract of a Letter on the great Comet from Lieut. D. W. Tyler, R.E., dated St. Kitts, March 6, 1843.

Letter from J. T. Austin, Esq., dated Funchal, Madeira, April 8, 1843, accompanying a Sketch of the Comet. Communicated by Sir J. F. W. Herschel, Bart.

Notes on the Comet, as seen by M. Montojo, at San Fernando. Communicated by Sir J. F. W. Herschel, Bart.

Account of the Comet, as seen on board the Childe Harold on her voyage from Bombay to London. By Lieut. W. S. Jacob, Bombay Engineers.

Letter from T. Forster, Esq., on the Comet, dated Bruges, April 22, 1843.

Account of the Comet, as seen on board the Ship Malabar, on her passage from the Cape of Good Hope. By R. Pollock, Esq. Commander.

Letter on the Great Comet from H. A. Cowper, Esq. H. M. Consul at Pernambuco in Brasil, dated March 9, 1843.

Observations of the Great Comet, made at the Royal Observatory, Cape of Good Hope, by Charles Piazzi Smyth, Esq. Communicated by Sir J. F. W. Herschel, Bart.

Abstract, by the Secretary, of Newspaper Accounts of the Great Comet, which have been forwarded to the Society.

Observations of the Comet, made at the Observatory of Trevandrum, accompanied by a Drawing. By J. Caldecott, Esq. Director of the Observatory.

Letter from Professor Kendall, containing Observations of the Comet, made at Philadelphia. Communicated by Lieut.-Col. Sabine.

Observations of Distance of the Comet from known Stars, made at Demerara. By Captain Geale, of the Ship Isabella, Lieut. A. S. Glascott, R.N. and J. Donald, Esq. Communicated by Sir J. F. W. Herschel, Bart.

Some Account of the Comet in a Letter from J. Gimblett, Esq. Communicated by Sir. J. F. W. Herschel, Bart.

Extract of a Letter from Lieut.-Col. Harvey, 14th Light Dragoons, dated Poona, March 13. Communicated by Professor Narrien.

Nov. 10. Description of a small Observatory, constructed at Poona in 1842, accompanied by Observations of Eclipses, &c., of *Jupiter's* Satellites. By Lieut. W. S. Jacob, Bombay Engineers.

Letter respecting the Great Comet, from S. C. Walker, Esq., dated Philadelphia, May 23, 1843. Communicated by Sir J. F. W. Herschel, Bart.

Observations of the Great Comet. By Captain Tucker, R.N. Communicated by the Lords Commissioners of the Admiralty.

Observations of the Comet made at Auckland, New Zealand, accompanied by a Map of its progress

amongst the Stars. By John Collyer Haile, Esq. Communicated by G. B. Airy, Esq. Astronomer Royal.

Notes on the Comet, extracted from the Journal of Capt. G. Rodney Mundy, R.N. Commander of Her Majesty's Ship Iris. Communicated by G. B. Airy, Esq.

Extracts from a Daily Journal of Remarks and Observations on the Comet, as seen at Van Dieman's Land. By Lieut. Kay, R.N. Communicated by Lieut.-Col. Sabine.

Extract of a Letter from A. Abbott, Esq., containing Remarks on the Comet, as seen at Madeira, accompanied by Two Drawings. Communicated by Sir J. F. W. Herschel, Bart.

Observations of the Comet. By Captain P. P. King, R.N., made at Port Stephens, New South Wales. Communicated by Captain Beaufort, R.N.

Abstract of an Article on the Comet in *Le Cernéen*, a Newspaper, published at Port Louis, Mauritius, March 14, 1843. Communicated by G. B. Airy, Esq.

An Account of the Comet, extracted from the *Colonial Observer*, of March 8, 1843, published at Sydney, New South Wales. Communicated by F. Baily, Esq.

Copy of a Letter, addressed to the Editor of the *New Zealand Colonist*. By Captain W. M. Smith, R.A.; and Extracts from Three other Letters from New Zealand, containing Accounts of the Comet. Communicated by F. Baily, Esq.

Observations of the Comet, as seen at Sea off the Island of Timor. By J. A. Murray, Esq. Communicated by J. B. Airy, Esq.

Pen-drawings of the appearance of the Comet. By Mrs. Grant. Communicated by Sir J. F. W. Herschel, Bart.

A Letter from R. H. Williams, Esq., containing an Extract from Notes on the Comet, made at the Observatory of Batavia. Communicated by G. B. Airy, Esq.

Remarks on the Comet, as seen at Galveston, Texas. By W. Bollaert, Esq. Communicated by F. Baily, Esq.

Letter from the Rev. W. S. Mackay to Sir J. F. W. Herschel, dated Calcutta, June 10, 1843, on the appearance of the Comet, and the variability of the Star η *Argûs*. Communicated by Sir J. F. W. Herschel, Bart.

Observations of the Comet made at the Mauritius, in March, 1843. By W. Lloyd, Esq.

Occultations observed at Ashurst in the year, 1843. By R. Snow, Esq.

Right Ascensions and Declinations of the Comet of Mauvais, observed at Hamburg. By C. Rumker, Esq.

Communicated in a Letter to F. Baily, Esq., dated July 25, 1843; and a Letter to Dr. Lee, dated August 18, 1843.

On the Divisions of the Exterior Ring of the Planet *Saturn*. By the Rev. W. R. Dawes.

Dec. 8. On the Apparent Magnitudes of the Fixed Stars. By Charles Piazzi Smyth, Esq. Communicated by Capt. W. H. Smyth, R.N.

Description of an Astronomical Time Watchcase. By the Rev. Professor Chevallier.

Mean Places, for Jan. 1, 1842, of Fifty Telescopic Stars, within Two Degrees North Polar Distance, observed in the years 1842 and 1843, at Markree, in the County of Sligo. By E. J. Cooper, Esq., and A. Graham, Esq.

On the Orbits of several Ancient Comets. By J. R. Hind, Esq. of the Royal Observatory, Greenwich. Communicated by the Rev. R. Main.

Approximate Elements of the Orbit of the Comet, discovered by M. Faye. By Professor Henderson.

Two Circular Letters from Professor Schumacher on the Comet, discovered by M. Faye. Communicated by F. Baily, Esq.

Results of Observations made with a Sextant and a Pocket Chronometer, for determining the Latitude and Longitude of the Apartments of the Society. By J. Hartnup, Esq. Communicated by Captain W. H. Smyth, R.N.

1844.

Jan. 12. On the Advantages of employing Large Specula and Elevated Situations for Astronomical Observations. By C. Piazzi Smyth, Esq. Communicated by Captain W. H. Smyth, R.N.

Observations of the Planet *Uranus*, made in the year 1843. By C. Rumker, Esq. Communicated by Dr. Lee.

Letter from Professor Schumacher to F. Baily, Esq., dated January 5, 1844, inclosing the Elements of the New Comet, computed by Dr. Goldschmidt.

On the Orbit of the Comet of Faye. By Professor Henderson.

Letter from Professor Henderson, announcing an additional Observation of the Comet of Faye.

Elements of the Comet of Faye, computed by J. C. Adams, Esq. of St. John's College, Cambridge. Communicated by Professor Challis.

Observations of the Comet of Faye. By C. Rumker, Esq. Communicated by Dr. Lee.

Observations of the Comet of Faye, made at Starfield. By W. Lassell, Esq.

Observations of the Great Comet of 1843, made by J.

Burdwood, Esq. Master of Her Majesty's Sloop Persian. Communicated by G. B. Airy, Esq.

Remarks on the Great Comet, as seen on board the Lawrence of Liverpool, on her passage from Sydney to Conception. By a Passenger. Communicated by W. Simms, Esq.

Abstract of an Article in *Silliman's Journal*, containing an Account of Observations of the Great Comet, made near the time of its Perihelion Passage. By J. G. Clarke, Esq. of Portland.

On the Deducing of the Parallax of *Mars*, and hence that of the Sun, from the Geocentric Motion of the former when in Opposition, and especially when near the Node of his Orbit. By S. M. Drach, Esq.

A Letter from Sir J. F. W. Herschel, Bart. to F. Baily, Esq., dated September 6, 1842, on the Increase in Magnitude of the Star η *Cygni*.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
L'Académie Royale des Sciences de l'Institut de France.
Bureau des Longitudes.
British Association.
Royal Academy of Munich.
Royal Irish Academy.
Italian Society.
Imperial Academy of St. Petersburg.
American Philosophical Society.
Royal Society of Göttingen.
Royal Academy of Lisbon.
Royal Academy of Brussels.
Royal Academy of Berlin.
L'Administration Imperial des Mines de Russie.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Geological Society of London.
Zoological Society of London.
Linnean Society of London.
Society of Arts.
Institution of Civil Engineers.
United Service Institution.
Yorkshire Philosophical Society.
Manchester Philosophical Society.

The Observatory at San Fernando.
The Observatory at Milan.
The Observatory at Dorpat.
The Radcliffe Trustees.
The Editor of the Athenæum Journal.
The Editor of the Quarterly Journal of Meteorology.
The Editors of the London Physiological Journal.

G. B. Airy, Esq. *Ast. Roy.*
M. Bachelier.
F. Baily, Esq.
M. E. Biot.
M. J. B. Biot.
A. Booth, Esq.
Professor Challis.
Professor Chevallier.
E. J. Dent, Esq.
M. De Vico.
Professor Encke.
T. J. M. Forster, Esq.
T. Galloway, Esq.
Count Graberg da Hemsö.
J. D. Hailes, Esq.
J. O. Halliwell, Esq.
Professor Hansen.
Professor Henderson.
J. Herapath, Esq.
W. B. Hunt, Esq.
Dr. Lamont.
M. Le Verrier.
James Lockhart, Esq.
T. Malby, Esq.
M. C. A. Peters.
M. Plana.
M. Plantamour.
W. Pole, Esq.
Professor Quetelet.
F. Robinson, Esq.
J. Rowbotham, Esq.
C. Rumker, Esq.
W. Rutherford, Esq.
M. Santini.
Professor Schumacher.
M. Slavinsky.
R. Snow, Esq.
Lieut. W. S. Stratford.
Professor Struve.
R. Taylor, Esq.
W. Vaughan, Esq.
Colonel Visconti.

At the conclusion of the reading of the Report of the Council, Mr. Galloway brought forward, for the approval of the meeting, sundry alterations, chiefly verbal, in certain of the Bye-laws of the Society, which the Council thought expedient to be made, previously to the reprint of the same, which is now necessary. All the alterations, as explained by Mr. Galloway, were unanimously agreed to.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: Francis Baily, Esq. F.R.S. — *Vice-Presidents:* George Biddell Airy, Esq. M.A. F.R.S. *Astronomer Royal;* Augustus De Morgan, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; The Right Hon. Lord Wrottesley, M.A. F.R.S. — *Treasurer:* George Bishop, Esq. — *Secretaries:* Thomas Galloway, Esq. M.A. F.R.S.; Rev. Robert Main, M.A. — *Foreign Secretary:* Captain W. H. Smyth, R.N. K.S.F. D.C.L. F.R.S. — *Council:*

Samuel H. Christie, Esq. M.A. F.R.S.; George Dollond, Esq. F.R.S.; Bryan Donkin, Esq. F.R.S.; Rev. George Fisher, M.A. F.R.S.; John Lee, Esq. LL.D. F.R.S.; Edward Riddle, Esq.; Captain James Clark Ross, R.N. F.R.S.; William Rutherford, Esq.; Lieut.-Colonel Edward Sabine, F.R.S.; Lieutenant William S. Stratford, R.N. F.R.S.

Thaddeus Foley, Esq. Mathematical Master, Royal Naval School, Camberwell, was balloted for, and duly elected a Fellow of the Society.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. VI. *March* 8, 1844. No. 5.

FRANCIS BAILY, Esq., President, in the Chair.

The following communications were read :—

I. Observations of the Comet of Faye.

1. Observations made at Starfield. By W. Lassell, Esq.

These observations were communicated in letters, dated Feb. 6 and March 7; and the following is a tabular statement of them :—

Day.	Mean Solar Time.	Right Ascension of Comet.	Declination of Comet.	No. of Obs.	Stars of Comparison.
1844.	h m	h m s	° ′ ″		
Jan. 12	11 24	5 8 47·0	+3 36 35	2	23 Orionis and ∗ (*c*).
13	9 7	5 8 56·1	3 40 33	1	∗ (*c*).
15	8 30	5 9 25·6	3 49 30	1	∗ (*d*).
22	7 45	5 11 58·0	4 27 32	1	∗ (*e*).
25	8 34	5 13 27·6	4 46 2	1	8 Monocerotis.
Feb. 19	9 46	5 34 41·5	7 32 58	2	α Orionis.
22	8 52	5 38 2·3	+7 52 22	2	α Orionis.

The following information respecting the above observations, and those contained in the *Monthly Notice* for January last, has been furnished by the author.

They were made with a power of 96 on a micrometer eye-piece applied to the 9-feet Newtonian reflector. The micrometer has two stout parallel wires permanently fixed at right angles to the moveable and parallel spider's lines, their internal edges being about 8″ apart. One revolution of the micrometer is assumed to be $= 18''\cdot298$. The interval of the transits of the comet and star over the thick wire was observed, the comet being so placed in the field that its nucleus should be concentric with the large zero-hole of the micrometer, and the distance in space between the zero and the star of comparison was measured with the micrometer. The comet has been throughout compared with one or other of the stars 23 *Orionis*, 30 *Orionis*, α *Orionis*, and 8 *Monocerotis*, either directly, or indirectly by means of intermediate telescopic stars.

In all cases, the author has communicated the intervals in time and the distance in N. P. D. in revolutions of the micrometer between the comet and the stars of comparison. The assumed places of the above-mentioned stars are as follows:—

23 Orionis	1843 Dec. 12	R.A.	=	$5^h\ 14^m\ 40^s{\cdot}3$
		Decl.	=	$+3^\circ\ 23'\ 22''{\cdot}4$
30 Orionis	Dec. 22	R.A.	=	$5^h\ 18^m\ 42^s{\cdot}0$
		Decl.	=	$+2^\circ\ 57'\ 20''{\cdot}2$
8 Monocerotis	1844 Jan. 25	R.A.	=	$6^h\ 15^m\ 32^s{\cdot}4$
		Decl.	=	$+4^\circ\ 40'\ 3''{\cdot}7$

The place of α *Orionis* is taken from the *Nautical Almanac.* In some instances, the distance in declination between the comet and one of the above-mentioned stars was measured intermediately by two and sometimes by three telescopic stars. The observations are not corrected for the effects of refraction and parallax.

2. Right Ascensions and North Polar Distances of the Comet of Faye, from Observations at the Royal Observatory, Greenwich. Communicated by G. B. Airy, Esq. Astronomer-Royal.

Day of Observation.	Mean Solar Time for R. A.	True Right Ascension of Comet.	No. of Obs. in R. A.	Mean Solar Time for N.P.D.	True N.P.D. of Comet.	No. of Obs. in N.P.D.	Remarks.
1843.	h m s	h m s		h m s	° ′ ″		
Nov. 29	12 30 46	5 21 36·05	14	12 38 40	84 25 36·0	11	
	......	......	...	12 48 37	84 25 32·1	1	Meridian Observation with the mural circle.
	17 33 9	5 21 28·19	6	17 33 9	84 28 9·9	6	
Dec. 11	9 1 7	5 15 59·50	7	9 1 7	86 8 25·2	7	
15	14 4 37	5 13 58·87	7	14 28 14	86 31 40·1	1	
1844. Jan. 15	9 55 30	5 9 24·43	2	9 54 10	86 9(32·6)	1	
22	8 17 27	5 11 55·62	6	8 16 29	85 33 5·1	5	
26	11 29 0	5 14 (4·00)	2	11 29 0	85 7(19 0)	2	
Feb. 8	8 11 30	5 23 39·71	1	8 11 30	83 42 24·7	1	Meridian Observation with transit instrument and mural circle.
19	11 46 9	5 34 45·58	2	11 46 9	82 26 38·9	2	
20	8 41 32	5 35 46·15	6	8 44 20	82 20 26·8	5	
22	8 18 13	5 38 2·19	10	8 12 26	82 8 1·1	6	

The above observations, with the exceptions mentioned in the remarks, were made with the south and the east equatoreals of the Observatory, the observations of Nov. 29 and Dec. 11 being made with the latter and all the rest with the former instrument. In general, the observations consisted of comparisons, in the same position of the polar axis of the instrument, of the comet with neighbouring stars in both elements, the differences of right ascension of the star of comparison and the comet being determined by the time of their transits across the declination-wire, and the differ-

ence of north polar distances by the reading of the declination circle of the east equatoreal, and of the sector-arc of the south equatoreal. The only exceptions to this rule are the results of Jan. 15 and Jan. 26, 1844 (included in brackets), which depend on the readings of the hour-circle and declination-circle of the south equatoreal, and which may be affected with errors of 3^s or 4^s in right ascension, and of $1'$ in north polar distance.

The results have been rigorously cleared of the effects of refraction and parallax, the distances of the comet from the earth being taken from an ephemeris, by Professor Henderson, in the *Monthly Notice* of this Society for January last, as far as it extends.

The places of the stars of comparison have been taken either from the Catalogue of Santini, contained in Vol. XII. of the *Memoirs* of this Society, or from meridian observations made since at the Royal Observatory. The only element which is at all doubtful is the right ascension of the star of comparison for the observations of Feb. 20, which has been deduced from the time of its transit across the central wire of the telescope of the mural circle. It is believed that this determination is not in error to the amount of 1^s, and the result may be used without scruple at this stage of the correction of the orbit. On Feb. 19, 20, and 22, the comet was excessively faint, and it required a great deal of caution to observe it. The single results are, however, for those evenings, on the whole very consistent, the north polar distances observed on Feb. 22 being subject to greater uncertainty than the rest. It was extremely difficult to bring the comet to the equatoreal wire of the instrument on that evening, though the times of transit on the same evening were observed with tolerable accuracy.

3. Right Ascensions and Declinations of the Comet of Faye, observed with the Equatoreal at the Observatory of Trinity College, Dublin, 1844. By Mr. Charles Thompson, Assistant. Communicated by Sir W. Hamilton.

Day.	Sidereal Time.	Right Ascension.	Declination.
1844.	h m s	h m s	° ′
Jan. 10	4 3 46	5 8 33·0	+3 25·8
12	4 4 12	5 8 50·0	3 36·1
13	4 15 49	5 8 55·9	3 41·0
14	5 46 0	5 9 8·4	3 47·3
15	4 15 3	5 9 26·6	3 50·4
18	5 2 20	5 10 18·4	4 5·5
22	6 13 22	5 12 1·6	4 27·4
23	6 10 19	5 12 25·2	4 34·9
24	5 55 49	5 12 54·3	4 40·8
25	6 1 51	5 13 27·6	+4 46·6

4. Observations of the Comet of Faye. By C. Rumker, Esq. Communicated by Dr. Lee.

Day.	Mean Time at Hamburg.	App. Right Asc. of the Comet.	App. N. Dec. of the Comet.	Number of Observations and Remarks.
1843.	h m s	° ′ ″	° ′ ″	
Dec. 1	10 33 46·0	80 11 55·78	5 14 44·4	14
1	12 39 53·3	......	5 13 52·2	Merid. Circle.
9	10 41 9·4	79 14 42·9	4 4 39·1	10
9	12 4 46·2	......	4 4 30·6	Merid. Circle.
10*	9 52 15·2	79 7 18·7	3 58 8·2	22
11	9 51 5·1	79 0 7·7	3 51 37·2	17
13	8 30 46·5	78 45 45·1	3 39 56·7	12
15	11 45 26·0	78 30 22·4	3 29 11·2	11
15	12 17 58·0	......	3 28 40·8	Merid. Circle.
16	8 23 31·7	78 24 44·3	3 25 0·7	1: doubtful.
17	8 4 49·6	78 18 3·7	3 20 42·1	17
1844. Jan. 21	8 8 0·4	77 52 12·9	4 20 55·1	2
21	10 10 4·2	......	4 12 26·9	Merid. Circle.
22	9 45 31·6	77 59 21·5	4 28 1·3	18
23*	9 58 8·4	78 6 39·7	4 33 58·1	14
26	8 18 15·5	78 30 11·1	4 52 7·2	3

5. Observations of the Comet of Faye made at the Observatory, Durham; Lat. North 54° 46′ 6″; Long. West 6^{m} 18^{s}. Communicated by the Rev. Temple Chevallier.

The equatoreal telescope employed is by Fraunhofer, of 6·375 inches aperture and of about 9 feet focal length. The magnifying power was 65; and, as the comet would generally bear no illumination, the observations were made with a double ring micrometer.

As a perfect observation of this kind gives eight instants of contact of the comet with the outer and inner circumferences of the rings, and as many for a star which crosses the same field, it determines their difference of right ascension with considerable accuracy. But it does not generally give the means of ascertaining their difference of declination with the same precision. Hence the right ascensions are more accurate than the declinations.

The comet was observed four times on the meridian with a transit instrument. The telescope has an aperture of 3·2 inches, and is of about 4 feet focal length; a power of 60 being employed. It has upon its axis a circle of 2 feet diameter, furnished with two microscopes.

* The observations marked with an asterisk are believed to be the best. The meridian observations are, owing to the necessity of partly illuminating the field, not so much to be relied on.

As the comet would bear no illumination upon the meridian, its declination could be obtained only by estimating when it was in the middle of the field, and its right ascension by observing the time when it quitted the field of view. Both determinations were consequently only approximate.

When it was possible, the comet was referred to a star which crossed the same field. At other times, it was referred to a known star near to it.

The following is a synopsis of the observations :—

Day.	Greenwich Mean Solar Time.	Right Ascen. of Comet.	Declination of Comet.	No. of Obs.	Stars of Comparison.	Remarks.
1843.	h m	h m s	° ′ ″			
Dec. 1	9 40	5 20 47·44	+5 16 30	3	......	Uncertain.
2	9 6	20 22·07	5 6 10	2	Anonymous	
4	10 51	......	4 39 22	1	......	Very uncertain.
11	9 12	15 56·06	3 50 43	4	γ Orionis	
12	10 6	15 25·20	......	2	......	
13	8 26	15 0·90	3 42 51	5	......	
15	11 33	14 0·35	3 30 25	6	23 Orionis	
21	9 6	11 27·40	3 6 16	3	Ditto	
24	8 30	10 28·00	3 1 0	1	......	
26	7 48	9 50·10	3 0 56	4	ϱ Orionis A Orionis	
27	9 58	9 33·30	3 3 1	4	ϱ Orionis	
1844. Jan. 18	9 37	10 34·70	4 17 38	2	Ditto	Only approximate.
23	9 38	12 32·30	4 33 49	3	Ditto	
24	11 55	12 59·10	+4 41 24	2	Ditto	

All the declinations are subject to some uncertainty. On Dec. 2, the comparison-star was observed on the meridian, and its right ascension was found to be $5^h\ 23^m\ 28^s{\cdot}94$, and its declination $+ 5^\circ\ 11'\ 5''{\cdot}21$.

6. Observations made at Hartwell, Bucks. By the Rev. J. B. Reade. Communicated by Dr. Lee.

II. Elements of the Comet of Faye. By Professor Henderson.

From observations made at Cambridge on December 16 and January 15, and at Greenwich on February 8, the following elements of the comet's orbit have been obtained, which are likely to be more correct than those formerly given :

	° ′ ″
Epoch of Mean Anomaly for December 31, 1843, Mean Noon at Greenwich	11 32 14
Longitude of Perihelion	43 39 46
Eccentricity ... sin.	34 0 38

Log. of Semi-axis Major 0·56533
Mean Daily Motion ...... 503″·52

Time of Revolution ...... 7·0468 sidereal years.

Longitude of ascending Node .... 211° 59′ 12″
Inclination 11 28 48

Motion direct.

Ephemeris for 8^h *Mean Greenwich Time.*

Day.	Right Ascension.	Declination.	Distance from Sun.	Distance from Earth.
1844.	h m s	° ′		
Feb. 8	5 23 36	+6 18	2·008	1·325
12	5 27 32	6 45	2·029	1·380
16	5 31 36	7 11	2·051	1·437
20	5 35 52	7 37	2·073	1·495
24	5 40 20	8 2	2·096	1·555
28	5 45 4	+8 27	2·118	1·616

T. Henderson.

Edinburgh, Feb. 14, 1844.

III. Elements of the Comet of Faye. By J. R. Hind, Esq. Communicated by the Rev. R. Main.

The following elliptical elements were computed from the Paris observation on Nov. 24, an observation at Hamburg on December 17, and one at Kensington on January 15. The corrections for parallax and aberration were applied according to the method described by Gauss in the *Theoria Motûs Corporum Cœlestium.* The longitudes were reduced to the mean equinox on the 1st January, 1844. The data employed in the calculations stand as follows:—

Greenwich Mean Time.	Comet's Geocentric Longitude.	Comet's Geocentric Latitude.	Earth's Heliocentric Longitude (corrected).	Log. Rad. Vect. of Earth (corrected).
1843.	° ′ ″	° ′ ″	° ′ ″	
Nov. 24·7051100	80 29 39·8	−16 38 54·8	62 19 15·1	9·9942268
Dec. 17·3089757	77 34 51·4	19 36 39·5	85 16 14·0	9·9929530
1844.				
Jan. 15·5521604	76 36 59·6	−19 1 16·5	115 4 10·1	9·9929275

Hence the following elliptical elements:—

Epoch 1844, January 1^{d}·0 Greenwich Mean Time.

Mean Anomaly	9° 56′ 36″·67	
Longitude of Perihelion on the Orbit	53 19 52·4	From the Mean Equinox.
Longitude of Ascending Node...............	208 24 18·3	
Inclination	11 7 8·7	
Angle of Eccentricity	31 54 52·15	

Log. of Semi-axis Major	0·5582124
Mean Daily Sidereal Motion	516″·04548
Period of Sidereal Revolution	2511^d·403 or about 6 years 11 months.

Log. of Semi-axis Minor	0·4870372
Log. of Semi-parameter	0·4158620
Log. of Perihelion Distance	0·2315531
Log. of Eccentricity in Seconds	5·0375958

Log. $\sqrt{a(1+e)}$	0·3712607
Log. $\sqrt{a(1-e)}$	0·1157766

IV. Letter from Professor Encke (translated), dated Berlin, Feb. 19, 1844. Communicated by G. B. Airy, Esq. Astronomer-Royal.

"The comparison of the Berlin observations of the *Comet of Pons*, made in the year 1842, with the elements which were derived from the observations up to the year 1838 and from the mass of *Mercury* thence deduced, and with the total disturbances up to 1842, has shewn, that the remaining errors are quite insignificant; and I find, in fact, for the less accurate observations at the commencement, when the comet was very faint:—

	$\Delta\alpha$	$\Delta\delta$
Feb. 9	− 1″·7	+ 18″·2
11	+ 17·9	− 1·3
12	+ 16·9	+ 4·7
March 3	− 1·6	− 4·6

And for the later and more accurate observations:—

March 11	+ 4″·8	+ 3″·6
20	+ 3·7	− 5·3
23	+ 9·3	− 8·3
24	+ 0·5	+ 2·2
April 6	+ 7·5	− 4·2
7	+ 1·0	+ 3·4

both sets of errors in right ascension and in declination being expressed in seconds of space. Now that I am preparing the *Ephemeris* for 1845, it would be highly important for me to compare the whole of the observations of 1842, especially those of the Cape of Good Hope. Up to the present time, I have received no communication concerning them. Should the results of these observations be known to you, I should be much obliged if you would do me the favour to communicate them.

"The present remarkable comet we have seen up to the 13th of February, but we shall probably be able to follow it still farther.

"The last observations were,

	h m s	R.A. ° ′ ″	Declination. ° ′ ″
Feb. 7	7 15 34	80 39 57·8	+ 6 10 51·9
10	10 59 8	81 22 21·5	6 31 54·9
13	7 40 6	82 3 54·6	+ 6 51 42·8

V. Occultations of Fixed Stars by the Moon, observed at Hamburg. By C. Rumker, Esq. Communicated by Dr. Lee.

Day.	Star's Name.	Phenom.	Observed Mean Solar Time at Hamburg.
1843.			h m s
May 9	ϵ Leonis.........	Imm.	9 34 56·3
		Em.	10 21 27·9
Aug. 12	9 Piscium	Imm.	10 43 54·2
		Em.	11 12 15·2
	κ Piscium	Em.	11 33 6·1
17	τ Arietis.........	Em.	12 28 2·3
Nov. 11	ζ Geminorum...	Imm.	13 11 9·6
		Em.	14 13 46·9
1844. Jan. 23	9 Piscium	Imm.	6 55 46·8
		Em.	7 22 11·3
Jan. 29	τ^2 Arietis.........	Imm.	5 58 45·5

VI. Farther Remarks on the Revision of the Southern Constellations. By Sir J. F. W. Herschel, Bart.

The idea, originally proposed of entirely re-modelling the southern constellations, has (after very mature consideration and much discussion, and after consulting the opinions of some of the most eminent continental astronomers, which have been found very adverse to the idea of so decided a change) been laid aside; at least in so far as regards the new catalogue of stars, now printing under the authority of the British Association. It is conceived, however, that if the nomenclature of the constellations, generally, be ever destined to undergo a systematic change at all (and many reasons may be adduced for considering such a change desirable) the first and most important step towards it will be found in the work above alluded to, and in the catalogues, now publishing simultaneously with it on the same system of nomenclature, which clear the ground of all existing confusion; and, by assembling into one distinct view, and under names and numbers at least definite and recognised, all the individuals of which the new groups must be composed, it will be easy at any future time to pass, by a single table of synonyms and by one decided step, from one to the other system, whenever the convenience and consent of astronomers may dictate the propriety of a change. Such views, if entertained, would render the nomenclature of the present catalogues so far provisional that a more rational and convenient system of groups (confined not to the southern hemisphere, but extending over both) may yet be contemplated by astronomers. Nevertheless, so long as the ancient system is at all retained, a general and scrupulous adherence to the nomenclature here adopted is most earnestly recommended to the astronomical world, as the only mode of escape

from a state of confusion at present quite intolerable. As regards the southern constellations, the following are the principles proposed; viz.

1°. That all the constellations adopted by Lacaille be retained, and his arrangement of the stars preserved; subject, however, to certain alterations hereafter specified.

2°. That all the stars, having a doubtful location, such as those which Lacaille (after the manner of Ptolemy) has considered as αμοϱφωτοι (unformed), be included within the boundaries of either one or other of the contiguous constellations, so as to preserve a regularity of outline and nomenclature.

3°. That all the rest of Lacaille's stars be placed within the boundaries laid down by him, with the following exceptions:— First, A few stars which are located too far from the border of the constellations in which they are registered, to admit of an uniform contour of the lines. Secondly, Such stars as have been previously observed by Ptolemy or Flamsteed, and by them located in other constellations, or which interlace and are confusedly mixed with such previously observed stars.* Thirdly, The four stars that are placed by Lacaille in the end of the *Spear of Indus*, but which are now assumed to form part of the constellation *Pavo*, in order to render the contour of these two constellations less circuitous.

4°. That the letters, selected by Lacaille, be adopted in preference to those introduced by Bayer in *Argo*, *Centaurus*, *Ara*, and *Lupus*. That the Greek letters (with a few exceptions) be retained only as far as stars of the 5th magnitude inclusive. That no Roman letters be at present used, except in the subdivisions of *Argo*, subsequently mentioned.

5°. That *Argo* be divided into four separate constellations, as partly contemplated by Lacaille; retaining his designations of *Carina*, *Puppis*, and *Vela*; and substituting the term *Malus* for *Pixis Nautica*, since it contains four of Ptolemy's stars that are placed by him in the *mast* of the ship.

6°. That the original constellation *Argo*, on account of its great magnitude and the subdivisions here proposed, be carefully revised in respect of lettering, in the following manner:—First, In order to preserve the present nomenclature of the principal stars, all the stars in *Argo* (that is, in the general constellation, regarded as including the subdivisions above-mentioned) indicated by Greek letters, by Lacaille, to be retained, with their present lettering, under the general name *Argo*. Secondly, All the remaining stars, to be

* A single exception to this rule occurs in the case of the last star in the constellation *Piscis Australis*, in Ptolemy's catalogue, which Bayer has denoted by the letter κ, and which is presumed to be the same as that which has been designated by Lacaille as *γ Gruis*. As there is some ambiguity, however, in the position of this star in Bayer's map, it is assumed (like some other stars already mentioned) as common to both constellations, in order to adjust this discordance; and, in the catalogue above-mentioned, Lacaille's designation of *γ Gruis* is retained, on account of its forming the principal object in the head of that constellation.

designated by that portion of the ship in which they occur, such as *Carina*, *Puppis*, *Vela*, and *Malus*, and to be indicated by the Roman letters adopted by Lacaille, as far as the fifth magnitude inclusive. And no two stars, far distant from each other in the same subdivision, to be indicated by the same letter; but, in cases of conflict, the greater magnitude is to be preferred; and, when they are equal, the preceding star to be fixed upon.

7°. That the constellations, which Lacaille has designated by *two* words, be expressed by only *one* of such words. Thus, it is proposed that the several constellations indicated by Lacaille as *Apparatus Sculptoris*, *Mons Mensæ*, *Cælum Scalptorium*, *Equuleus Pictorius*, *Piscis Volans*, and *Antlia Pneumatica*, be called by the respective titles of *Sculptor*, *Mensa*, *Cælum*, *Pictor*, *Volans*, and *Antlia;* contractions which have on some occasions been partially used by Lacaille himself, and are very convenient in a registry of stars.

VII. Extract (translated) from a Letter of Professor Bessel to Sir J. F. W. Herschel, Bart. dated Königsberg, January 22, 1844. Communicated by Sir John Herschel.

"I think it may be interesting to you, or to one or other of your astronomical friends who occupy themselves with meridional instruments, to be made acquainted with a result which I have obtained from a theoretical investigation, the object of which was to determine the effect of gravity upon the figure, and consequently on the divisions, of a circle fixed in the vertical plane. The effect will manifestly be, that the radii in the upper part of the circle will be shortened; those in the lower part lengthened; and all, with the exception of the vertical ones, bent downwards. These changes, the magnitude and law of which must depend on the special construction of each circle, in two instances known to me (namely, those of two meridian instruments by Repsold, each of which is furnished with two circles, at opposite ends of the axis, each being read by four microscopes), have become prominently sensible; giving rise to this effect, namely, that on turning the instrument through 180°, a different measure is given by its two circles. From this result it follows that the supposition of the amount of change being insensible, or of the influence of gravity on the circle being eliminated by the four readings, is without foundation; and that there is, therefore, cause for apprehending in general that every circle gives an erroneous measure of the zenith-distances, and erroneous to an extent which, in respect of the existing means of pointing its telescope in a given direction, and reading the divisions, is by no means insensible.

"Now, as it is necessary to be able to determine this influence to such a degree of approximation as to be in a situation to judge whether it is possible to adopt any mode of using the instrument by which the results shall be freed from it, I have been led to undertake the solution of the following statical problem:—'To

determine the figure of equilibrium of a circle placed in the vertical plane.' This problem is manifestly considerably complicated. For a circle, having *m* radii, and each pair connected together, not only by the circular rim, but also by a direct connexion, there is (in respect of both circles) an aggregate of $4m$ elastic lines, both expansible and flexible, for the determination of which $3 \times 4m = 12m$ constants are necessary; besides which, $6m$ unknown quantities also come into consideration; namely, the co-ordinates of each of the particular points in which two or more of the lines are united, and the directions of the lines with respect to the same; consequently, three unknown quantities for each of the $2m$ particular points. The problem, therefore, includes $18m$ unknown quantities; and, in the case of Repsold's circles, which have 10 radii, 180. The equations necessary for their determination will be obtained from the condition that the sum of the forces which act not only upon the particular points, but also upon every point of a line, arising from gravity and the connexion of the different parts, shall be in equilibrium.

"The general solution of this problem, which is limited neither by the assumption of a symmetry of figure, mass, or elasticity, nor by that of the absence of primitive tension between the different parts of the whole, may be reduced, as I find, to the solution of $3m$ linear equations, and, on the supposition of symmetry, to three such equations. But this last supposition very probably corresponds to no actual case. It is also probable that in no case can we obtain a knowledge of the deviations from actual symmetry, which it is necessary to possess, in order to obtain a result which shall give the required influence in numbers. I find the following general proposition respecting the law of this influence, which is restricted by no particular supposition.

"If the angle between the initial radius of the circle and the vertical be denoted by u, the angle between the same radius and a point of the division by v, then the variation of this angle arising from gravity, in the vertical plane of the circle, is expressed by $fv.\cos u + f'v.\sin u$, where fv, $f'v$, are functions of v, independent of the position of the initial radius, and dependent only on the construction of the circle.

"This proposition is of considerable importance for practical astronomy. We readily infer from it, that the influence of gravity upon a zenith distance entirely vanishes when the latter is determined by the mean of four observations, namely, observations of the object itself and of its image reflected from a horizontal mirror, repeated in reversed positions of the axis of the instrument. From the same proposition we also easily deduce the mode of arranging the observations through which the errors of division must be determined, in order that their results may be also independent of the influence of gravity.

"It follows also from this that the astronomer who uses a circle is in possession of the means of rendering his results entirely independent of the influence of gravity; and that results obtained by

different instruments, when this method is followed, must accurately agree. It is likewise to be remarked, that the power of reversing the axis is an advantage with which the independence of the zenith distances of the influence of gravity is connected (and without which such independence could not be obtained).

" Although of no use in a practical point of view, it may not be uninteresting to investigate the numerical result which the theory now developed gives in a particular case, on the supposition of symmetry. For this purpose, I took the case which corresponds to the dimensions of my 3-feet circle by Repsold, and found that one of its radii, the direction of which corresponds to a zenith distance z, alters in length about $- 0''\cdot2274 \cos z$: its extremity deviates from its direction at the initial point about $+ 0''\cdot6726 \sin z$; and its direction at the extremity of the arc from that at the initial point by $- 0''\cdot5124 \sin z$. Although the supposition of symmetry upon which these results depend is probably only correct in respect of the exterior figure of certain parts of the whole instrument, this calculation may give an idea of the extent to which the influence of gravity will attain; an extent which, from the complicated nature of the problem, we should not be able, without a calculation, to estimate.

" You may easily suppose that the conclusion I have just obtained, namely, that the attainment of any required degree of precision in results depends only on the observer, appears to me to be the strongest motive for the zealous prosecution of observations. In the ever-memorable days I passed with you at Collingwood, you expressed the opinion, that the prospect of completely attaining the object of any undertaking is of itself sufficient to render it in a high degree attractive."

VIII. A Letter from J. R. Crowe, Esq., British Consul-General of Norway, concerning a Literary Society established at Alten, near Hammerfest. Communicated (with a letter) by Dr. Lee.

Mr. Crowe visited England in the summer of 1843, and gave information concerning the existence of a Society consisting of Swedes, Englishmen, and Germans, at Alten, near Hammerfest, in Finmark, under the patronage of a Swedish clergyman, the pastor of that district. This Society was in possession of some instruments which had been left there by some French gentlemen of science who were sent to Lapland by Louis Philippe a few years ago. Regular observations of the barometer and thermometer had been instituted according to the plan suggested by Sir John Herschel, and Mr. Crowe was of opinion that if an observatory could be established the Society would cheerfully undertake the working of it.

Dr. Lee munificently furnished the Society with an achromatic transit instrument of 30 inches focal length, and an aperture of 2 inches, on an iron stand, and with a circle 6 inches in diameter, reading to 1′, and with a collection of books on various depart-

ments of science: an astronomical clock was also ordered of Mr. Dent, but it was not ready to be sent by the same vessel which conveyed the instruments. Owing to the exertions of Dr. Lee, other fellows of this Society were induced to contribute books, and the nucleus of a good library was thus formed. Mr. Crowe took with him from England two minimum thermometers with the intention of placing one on the top of Storvandsfield, the highest mountain in the neighbourhood of Alten, and the other on the highest point of the southern extremity of Spitzbergen; but he arrived too late to carry the latter part of his plan into effect. The difficulties of the ascent of Storvandsfield are thus described by Mr. Crowe :—

"The ascent to Storvandsfield was very difficult; so much fresh snow had fallen as to impede even the snow-shoes, which the party were obliged to use. The task, however, was accomplished, and the thermometer safely fixed on the highest point. It was just in time, as one of those sudden gales of wind sprang up, peculiar to high mountain regions, driving and whirling the snow before. For hours the party were exposed to considerable danger, and by the time it did lull, Mr. Greive, who had volunteered to superintend the task, was so knocked up, as to be unable to proceed, and the guides had to carry him; fortunately, a shelter of loose stones, erected by the nomadian Laplanders, was reached, where they deposited him, while one of the guides descended for further assistance. Happily, the cold was not intense, so that a warm bed and rest perfectly recovered him."

Alten is in north latitude 69° 38′, and in longitude 23° 43′ east, and thus by its geographical situation, highly important for certain classes of observations, being the most northern place in the world at which an observatory is established. The following observations have been uninterruptedly carried on, viz. :—

Of the barometer; the thermometer (with the maximum and minimum temperatures; the pluviometer; the galvanometer; declination magnetometer; land winds and approximate force; of clouds, their direction, approximate velocity, and general description; and of the Aurora Borealis.

Erratum in the Monthly Notice for February last.

The Council take the earliest opportunity to correct a misstatement in their Report to the General Meeting of February last, concerning the Reduction of the Ancient Greenwich Lunar Observations at the Royal Observatory. It is stated at page 44, that "the work has been at length brought to a close." This is not the case, but it is in a state of great forwardness; the tabular places have been computed throughout, and the labour of an additional year will probably complete the reduction of the observations.

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ROYAL ASTRONOMICAL SOCIETY.

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FRANCIS BAILY, Esq., President, in the Chair.

Lieut.-Colonel John Hambly Humfrey, late Royal Staff Corps, was balloted for, and duly elected a Fellow of the Society.

M. Emile Plantamour, Director of the Observatory at Geneva, and Professor of Astronomy in the Academy at Geneva, was balloted for, and duly elected an Associate of the Society.

The following communications were read :—

I. Additional Observations of Faye's Comet, made at the Observatory of Trinity College, Dublin, by Mr. C. Thompson, accompanied by an explanation of the method of observation and reduction. Communicated by Sir W. Hamilton.

The telescope of the equatoreal instrument with which the observations were made,* was purchased from Cauchoix of Paris, the aperture of the object-glass being rather more than 5 inches, and the focal length $4\frac{1}{2}$ feet.

The circles were made and divided by Mr. Jones of London; the hour-circle to intervals of 10′, and the declination-circle to intervals of 5′; the former is usually read off to seconds of time, and the latter to minutes (or sometimes to half-minutes) of space. The clock has a mercurial pendulum, and goes pretty well.

The comet was usually observed twice in one evening, both circles being read off each time. The comparisons in both elements of the comet and the star were made in an unmoved position of the polar axis. The clock was compared carefully each night with the transit clock. The deviation of the polar axis of the instrument is considerable, but its amount is known, and the numerical corrections for this and other smaller instrumental errors are believed to be tolerably constant.

The observations are cleared of the effects of refraction, but not

* Monthly Notice for March 1844, page 55.

of parallax, no assumption having been made with respect to the comet's distance from the earth.

During the month of January the comet was compared with ρ *Orionis*, whose assumed apparent right ascension is $5^h\ 5^m\ 10^s{\cdot}33$, and assumed apparent north polar distance $87°\ 19'\ 45''{\cdot}6$ throughout. In February, the comet was compared with α *Orionis*, whose place is taken from the *Nautical Almanac.*

A very full tabular statement of the reduction of the observations accompanied the explanation.

The following are the additional observed places of the comet:—

Day.	Sidereal Time.	Right Ascension of Comet.	North Polar Dist. of Comet.
1844.	h m s	h m s	° ′ ″
Feb. 15	7 24 21	5 30 14·37	82 51 38·0
19	5 47 2	34 34·18	25 54·3

II. Meridian Observations of the Moon and Moon-culminating Stars, made at Hamburg during the years 1838 and 1839. By C. Rumker, Esq. Communicated by Dr. Lee.

III. Elements of the Comet of Mauvais. By M. Götze. Communicated by Dr. Lee.

The elements are

$$T = 1843, \text{ May } 6{\cdot}0332292 \text{ Greenwich Mean Time.}$$

$$\text{Log. } q = 0{\cdot}2083948$$

$$\left.\begin{aligned} \varpi &= 281°\ 27'\ 47''{\cdot}58 \\ \Omega &= 157\ \ 14\ \ 51{\cdot}45 \end{aligned}\right\} \text{On the ecliptic, and from the Mean Equinox of 1843, July 3.}$$

$$i = 52\ \ 44\ \ \ 0{\cdot}98$$

The co-ordinates referred to the ecliptic, and the mean equinox of 1843, July 3, are:—

$$\begin{aligned} x &= r\,[9{\cdot}9783885]\sin(v + \ \ 19°\ 58'\ \ 0''{\cdot}36) \\ y &= r\,[9{\cdot}8320335]\sin(v + 269\ \ 30\ \ 33{\cdot}00) \\ z &= r\,[9{\cdot}9008179]\sin(v + 124\ \ 12\ \ 56{\cdot}13) \end{aligned}$$

The places of the comet deduced from the above elements are compared with the places deduced from observations made at various observatories, from March 3 to September 2, 1843, and the agreement is very close throughout.

IV. Observations of the Comet of Encke, made at the Royal Observatory of the Cape of Good Hope in May 1842. Communicated by Thomas Maclear, Esq.

The comet was first seen by Mr. Mann on May 2, and con-

tinued visible till May 22, though on that day it was too faint for observation.

The instrument employed was Dollond's 46-inch achromatic telescope of $3\frac{1}{4}$ inches aperture, mounted equatoreally on a portable stand, and armed with an annular micrometer. The interior and exterior radii of the ring were determined with great exactness from a great many experiments. The times were noted by a half-seconds' chronometer compared at proper intervals with the transit clock. Attempts were generally made to observe at both the exterior and interior edges of the ring, and the observations for each diameter have been computed separately, and the results differ widely from each other. The observations are given in detail, and the apparent places of the stars of comparison, which have been since observed with the meridian instruments, are also given.

The following table contains the places of the comet deduced from the observations:—

Date.	Cape Mean Time.	Right Ascension.	Declination.
1842.	h m s	h m s	° ′ ″
May 2	17 25 23·7	1 18 53·63	− 8 26 28·5
4	17 10 4·6	1 10 35·42	10 14 18·1
4	17 10 4·6	1 10 36·06	10 14 30·0
4	16 58 18·1	1 10 37·05	10 14 40·0
4	16 58 18·1	1 10 37·22	10 14 30·0
5	17 24 3·7	1 6 36·82	11 8 5·0
11	16 15 28·6	0 45 3·26	15 31 6·0
13	16 19 0·7	0 38 34·86	16 47 48·0
17	15 31 14·3	0 26 29·07	19 3 57·0
17	15 34 36·8	0 26 27·81	19 4 1·0
17	15 31 14·3	0 26 29·62	19 3 53·0
17	16 27 26·6	0 26 21·09	19 5 56·0
17	16 42 45·4	0 26 19·19	19 6 12·0
17	16 46 19·5	0 26 18·63	19 6 29·0
18	16 28 58·8	0 23 26·73	19 37 16·0
19	15 56 25·3	0 20 37·23	20 8 2·0
20	16 17 47·4	0 17 40·54	20 39 16·0
20	16 38 16·8	0 17 33·53	−20 41 37·0
21	17 29 33·8	0 14 41·62	......

V. An Account of the Erection of the Herschel Obelisk at the Cape of Good Hope, accompanied by the Report of Colonel Lewis, and a Plan of the same. By Thomas Maclear, Esq.

At the last meeting of the Committee appointed to superintend

the erection of the Herschel Obelisk, held on the 2d of March, 1842, it was resolved that a plan of the same, together with that part of Colonel Lewis's Report, which refers to its construction and erection at Feldhausen, should be forwarded by Mr. Maclear to the Royal Astronomical Society of London, with a request of the Committee that the same might be published in the *Memoirs* of the Society.

Mr. Maclear's engagements did not give him the requisite leisure for complying with the request of the Committee, and for collecting the additional information respecting the history of the obelisk, till the end of the year 1843.

The following is an abstract of the explanation furnished by him :—

Sir John Herschel, during his residence at the Cape, was President of the South African Literary and Scientific Institution. When he was about to leave the colony, the members expressed a desire to present him with some token of remembrance; and, at a full meeting, a few days before his departure, a gold medal was presented, with the impress of the institution on one side and a suitable inscription on the reverse. The feelings excited on that interesting occasion strongly evinced how much the members regretted the loss of their president and their admiration of one whose talents place him so far above ordinary men, and whose private life was a pattern of every domestic virtue.

The sum subscribed having exceeded the expense of the medal, another subscription-list was opened with the intention of raising a fund for the purpose of placing a substantial structure on the site of the 20-feet reflector in the garden of Sir John's late residence at Feldhausen. The proposal was accordingly laid before Sir George Napier, who entered warmly into the project, and placed his name at the head of the list annexed to a handsome subscription. In the course of a few days the sum subscribed amounted to 190*l*.

At a general meeting, held on the 28th of November, 1838, the erection of the obelisk was finally determined on; and a committee was appointed to carry its erection into effect.

A fruitless attempt to procure a granite column at the Cape, of proper workmanship and within the resources of the Committee, led to the adoption of a suggestion, that one of Craigleith stone, from the quarry near Edinburgh, might be obtained without difficulty and of superior finish. A resolution was accordingly passed by the Committee, which, together with a plan of the proposed obelisk, was forwarded to Professors Forbes and Henderson, of Edinburgh, with a request that those gentlemen would kindly undertake the necessary superintendence of the work; a request which they acceded to with alacrity; and the obelisk, in packing cases, arrived in Table Bay in the month of August 1841, where it was safely landed under the guidance of Colonel Lewis.

The following is the report of Colonel Lewis on the erection :—

"In excavating the foundation, which was of black sand, it was found necessary to go down 4 feet 10 inches to arrive at the iron-

stone gravelly bed, the substratum of the country about Feldhausen. The masonry foundation was formed of concrete, built up in courses of 12 or 14 inches, and composed of iron-stone, gravel, and lime-mortar, well grouted together. On this masonry bed a granite platform 9 feet 6 inches square was laid, and the small column fixed by Sir John Herschel on the site of the 20-feet reflector. This mark was removed for a few days, in order to bring the masonry foundation to a proper height, but the mark was relaid with mathematical correctness by Lieut. Laffau, Royal Engineers.

"Previously, however, to relaying the Herschel mark, the suggestion of the Committee of construction was adopted of placing under it several silver and copper coins, a few inscription medals, and medals of the South African Institution, struck in silver for the occasion; and on the obverse were engraved some notices, statistical and geographical, of the colony; the discoveries of Capt. Ross in the South Polar Regions in 1841; and the operation of remeasuring the arc of the meridian in 1842. These subjects were beautifully executed by Mr. Piazzi Smyth, assistant-astronomer, and hermetically sealed in glass bottles. Also there were deposited a map of the colony and engravings of nebulæ observed at Slough from 1825 to 1833, by Sir John Herschel, and a plan of Mr. Maclear's triangulation connecting the site of Feldhausen with the Royal Observatory and the site of La Caille's observatory, in Strand Street, Cape Town.

"The bottle was carefully fixed in a block of teak-wood, scooped out on purpose.

"When the granite platform was brought to its level, and the Herschel mark refixed and filled in with cement, it was necessary to erect heavy shears of large spars, to place the stones of the obelisk, composed of large blocks of Craigleith stone, some weighing two tons, sent from Scotland by Professors Forbes and Henderson, who kindly took this charge. This was accomplished with some trouble and expense, and the base of the obelisk was laid with the faces corresponding with the four cardinal points. The whole was completed on the 15th of February, 1842, in presence of some of the Committee and several of the subscribers and friends of Sir John Herschel, who attended on the occasion of placing the top stone of the obelisk.

"The obelisk has the base 6 feet square by 6 feet in height, and the pyramidal part stands 12 feet above the base. On the east face is an opening shewing the Herschel mark, designating the site of the 20-feet reflector. The opening will be closed with a bronze plate, containing the inscription of the purpose for which the obelisk is erected."

VI. On Loud Beats of Clocks used in Observatories. By J. S. Eiffe, Esq.

This paper gives an explanation of a simple and easily applied method of obtaining very loud beats for the astronomical clock. The author alludes to the difficulty which has been hitherto ex-

perienced in obtaining any sound in the beat of astronomical clocks at all approaching that which is necessary for distinctness under ordinary circumstances. He then adverts to the necessary irregularities in the clocks of the ordinary construction, arising from the largeness and imperfection of the workmanship of the brass-work, and hints at a method which he has devised, whereby the present large circle of seconds can be retained with a small and delicate movement, such as those used in marine chronometers, and without any intermediate wheel.

But without a method to enable the observer to increase at pleasure the loudness of the beats, it is manifest that the beats of such a clock would not be loud enough for the ordinary purposes of an observatory; and the author's invention, described in the present paper, is intended to supply this deficiency by the introduction of an apparatus to produce loud beats simultaneously with the escape of the seconds' wheel from one pallet to the other, which is totally independent of the size and weight of metal of the works of the clock. The mode of constructing the apparatus is as follows:—

Two pieces of thin brass are placed at the sides of the frame-work of the clock, in length the same as the space between the pillars; in width, about two inches or more at pleasure; these pieces of brass are placed horizontally, at about the same altitude from the base as the axis of the escape-wheel pinion, and at right angles to it, or nearly so. They should be made of such a size as would insure a sound, distinct, sharp, and short. The little tables can be made to any size, as is mentioned above. Upon these tables or plates two hammers ply, supported by arbors at the same elevation as all the others. The pivots should be made small for easy motion. The hammers are intended to beat upon the middle of each brass table simultaneously with the drop proper of the escape-wheel: through the agency of the pendulum, they are lifted alternately by the heels of the anchors of the pallets, assisted by a passing spring similar to that used in the chronometer escapement. It has just been observed, that the arbors which support those little hammers are placed at the same elevation from the base of the brass frame-work of the clock as the escape-wheel arbor, but at the sides, and as near to the edge as possible. About the centre, or midway between them, are affixed brass collets, about 1-8 of an inch in thickness, and 1-4 of an inch in diameter. Two slender pieces of spring are secured to the collets by screws passing through square holes formed longitudinally, to secure power of adjustment for bringing the arms into proper contact with the anchor of the pallets. The little hammers beat upon the plates or tables at one end, and at the other the lifting action takes place, assisted by the passing spring. The strokes upon these brass tables have a peculiar sharpness of tone, which can be accounted for in some measure, when it is considered that they are very different from the sounds produced by the teeth of the wheel itself; in the dead-beat escapement the teeth have a sliding

motion in the moment of drop, but not impulse, for it is well known that that is subsequent to the sound. The exertions to obtain sound have for some years been considerable; but the result has been a poor recompense, and unimportant after all. Thus then, and by such application, simple and easy to be understood, is it proposed to obtain sound, so loud as to be *distinct in the stormiest night;* but as the constant connexion of such apparatus would neither be desirable as concerns the action of the clock, nor pleasant to the ear as a companion, a mode has been introduced of readily detaching it altogether. By a certain method, which shall be explained, the hammers are raised from the tables at one end, and the arms at the other entirely disengaged from the anchor of the pallets, without the least inconvenience or disturbing action to the clock itself. This is very pleasing, and greatly to be desired, and perhaps may be considered of nearly equal importance with the invention itself by which a loud beat is obtained. The apparatus within is immediately, and at pleasure, acted upon through the agency of a bolt, which is placed vertically, immediately over the 60 minutes, or about two inches back, sufficiently long to reach a spring of hard brass, which is about half an inch wide, and which passes transversely over the frame-work of the clock, and is fixed securely to the back board of the clock-case. Now the mode in which the spring unites its action with the rest of the apparatus is by slight cross-bars, which extend to the extremities of the sides of the frame, so that the ends are immediately over the hammers, with which they are connected by silk threads. Therefore, by pressing down the bolt before named, the hammers are allowed to fall into action, and do their duty simultaneously with the teeth of the wheel upon the pallets. While the little hammers are in action, the teeth of the wheel are no longer heard, and the motion and view of the seconds hand, as well as the ear of the inquirer, will satisfy him as to the usefulness of the invention.

In conclusion, and to endeavour to dispel any doubts that may arise as to the probable advantages to be obtained by such assistance to the observatory, the opinion and satisfaction expressed by word and by letter, on those advantages, by the distinguished individual who is at the head of our national observatory, will, it cannot be doubted, be interesting to this learned Society. The Astronomer Royal declares by letter, that he has examined the plan, and is enabled to say that it answers completely for its proposed purpose; and that it appears likely to be very useful. Moreover, that the rate of the clock will not necessarily be disturbed during the time of its connexion—though that will greatly depend on certain conditions.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *May* 10, 1844. No. 7.

John Lee, Esq., LL.D., in the Chair.

Josiah Rees, Esq., of Guildford Street, and William Pole, Esq., Professor of Civil Engineering, Elphinstone College, Bombay, were balloted for, and duly elected Fellows of the Society.

The following communications were read:—

1. The Right Ascensions of the Principal Fixed Stars, deduced from Observations made at the Observatory, Cape of Good Hope, in the Years 1832 and 1833. By Thomas Henderson, F.R.SS. L. and E., Professor of Practical Astronomy in the University of Edinburgh.

This catalogue contains the right ascensions of 174 principal fixed stars, reduced to January 1, 1833; being the same stars of which the declinations, observed by Mr. Henderson at the Cape, are published in Vol. X. of the *Memoirs*, with the addition of θ^2 *Eridani*, α *Persei*, and θ *Ursæ Majoris*. The observations of right ascension (with the exception of a small number) were made by the assistant-astronomer, Lieutenant William Meadows, R.N. The transit instrument, by Dollond, is 9 feet $9\frac{1}{2}$ inches in focal length, with an object-glass of 5 inches in diameter. The magnifying powers employed were 88 and 132; chiefly the latter. The clock was by Hardy, having one of his scapements, and a mercurial compensation pendulum. During the period of the observations its rate was as uniform as the rates of most transit-clocks in use at observatories. The reductions of the right ascensions to January 1, 1833 (when the sun's mean longitude was 281°), were computed from the tables in the supplements to the *Nautical Almanac* for 1832 and 1833. The coefficient of aberration was assumed to be $20''\cdot5$; and the proper motions in right ascension were taken into account, when they appeared to amount annually to $0^s\cdot01$ of time. The right ascensions given in the catalogue are the means of all the determinations from observations made above and below the pole without distinction. The annual precessions are calculated for the beginning of 1833, from the *Tabulæ Regiomontanæ*. The right ascensions of the present catalogue are compared with those of the same stars in the catalogues of Lacaille, Bradley, Piazzi, Rumker, Johnson, and Pond (all reduced to the same epoch), and the differences exhibit the effect of the proper motion in right ascension for each of the stars respectively.

From these differences, combined with the differences of declination given in Mr. Henderson's former catalogue above referred

to, the annual proper motions of the stars in the present catalogue are obtained. A table is given, which contains such of them as appear to have proper motions not less than 0″·1 of arc; they amount to 35. The following are those whose annual proper motions exceed 0″·4.

Star.	Total Annual Proper Motion.	Annual Proper Motion in Right Ascension (in Time).	Annual Proper Motion in Decl.
	″	sec.	″
α Centauri	3·58	− 0·464	+ 0·82
β Hydri	2·17	+ 0·716	+ 0·29
α Canis Majoris	1·34	− 0·034	− 1·25
θ Centauri	0·79	− 0·047	− 0·54
γ Pavonis	0·74	+ 0·013	+ 0·73
ι Scorpii	0·69	− 0·050	− 0·29
α Phœnicis	0·47	+ 0·014	− 0·44
β Trianguli Australis	0·44	− 0·030	− 0·40

II. **Observations on the Appearance of the Comet of 1843, made at Cape Coast Castle, on the Coast of Africa. By G. Maclean, Esq. President of the Colony. Communicated by Captain Beaufort, R.N., Hydrographer to the Admiralty.**

The comet was first seen at Cape Coast Castle on the evening of Friday, the 3d of March, at about a quarter to seven. Part of its tail only was then visible, bearing W.S.W., and making an angle of about 70° with the horizon, towards the south. It was of the same brightness throughout, and its breadth, which was little more than a degree, so far as it could be seen on account of both extremities being concealed by clouds, was also uniform.

March 4. This evening the whole of the comet was visible, although no nucleus could be distinguished. Its head, or what appeared to be so, almost touched the horizon, near the star *iota* in the tail of the whale; and its tail extended about 22° from that point in the direction of the constellation *Columba Noachi*.

March 5. Several glimpses of what appeared to be a nucleus were perceptible through the telescope of a theodolite. It appeared as a bright point, of the colour of *Venus*, but exceedingly small. Being invisible through the telescope of the sextant, distances could not be ascertained with any degree of precision.

March 6. The appearance of the comet was the same as the preceding evening. March 7, the brightness of the head and the length of the tail were much increased, the latter extending upwards of 34° in the direction of the constellation *Lepus*. Several stars were visible to the naked eye through the tail. On the 9th and 10th, the appearance of the comet was much the same as on the 7th; on the 17th it was visible, but the nucleus was very indistinct. The tail extended about 43° in the direction of *Sirius*.

March 19. This night was clear, and the outline of the comet

very plainly marked. The bright spot or condensation in its head was distinctly perceptible to the naked eye. On the 22d, although the sky was very clear, the nucleus was with difficulty perceptible, from which it appeared that the comet was increasing its distance from us with immense rapidity. The tail terminated midway between the stars ζ, η, and θ *Leporis*, and κ *Orionis*.

After this time the comet decreased in brightness and size every night. On the 23d its tail was about 38° in length; on the 26th about 35°, reaching a little past κ *Orionis*. Through ordinary land-glasses it still appeared as if there was a condensation of brighter matter in the centre of the head. The comet continued visible on clear nights till about the 10th or 12th of April, appearing as a thin haze; but after the 1st no observations could be taken with the sextant.

The following are the observed distances of the comet from *Sirius* and *Aldebaran*. They are given without any correction, just as they were read off from the sextant; and the observations were made about seven in the evening.

Day.	Distance from Sirius.	Distance from Aldebaran.	Day.	Distance from Sirius.	Distance from Aldebaran.
1843.	° ′	° ′	1843.	° ′	° ′
March 3	94 15	69 45	March 19	54 24	34 7
5	90 35	67 12	22	50 0	30 42
6	87 18	63 51	23	48 22	29 51
7	83 48	60 36	26	44 53	26 40
9	77 37	54 30	27	43 39	25 40
10	73 50	51 57	28	42 45	24 59
17	58 50	37 52	29	40 58	24 28
18	56 16	36 50	April 1	38 8	22 58

III. Extract of a Letter from J. R. Crowe, Esq., British Consul-General of Norway, to Dr. Lee, dated Alten, February 22, 1844. Communicated by Dr. Lee.

"The observatory at Alten, as you are aware, is the most northern in the world. We shall very shortly commence the series of preparatory transit observations you suggest. We had intended to have done so before; but the cold, since the sun revisited us on the 2d instant, has been so intense that we have not been able; the eye was no sooner brought near the glass than the latter was covered with a coating of ice. It was even dangerous to touch the metal. The thermometer has been varying from 24° to 28° below zero of Celsius, that is, from 12° to 20° below the zero of Fahrenheit, or from 44° to 52° belowing the freezing point . . . . It will be interesting to see what the minimum thermometer will exhibit at the top of the Storvandsfeldt by the end of next month. We shall then attempt the ascent, as by that time the surface of the snow becomes so hard as to bear walking upon with impunity."

IV. Elliptic Elements of Bremicker's Comet. Computed by Mr. William Götze. Communicated by Mr. Rumker, and also by Professor Schumacher.

Perihelion Passage, 1840, Nov 13, Berlin Mean Time.

Ascending node	248° 55′ 57″·15	On the ecliptic, and from the mean equinox of 1841, Jan. 0.
Inclination	57 57 51·59	
Distance of perihelion from node............	133 36 8·33	
Angle of eccentricity ...	76 5 21·52	
Log. perihelion distance	0·1705436	
Log. semi-axis major ...	1·7032559	

With the above elements, the following co-ordinates of the comet to the equator have been computed from the mean equinox of Jan. 0, 1841, assuming, according to Bessel, the mean obliquity = 23° 27′ 36″·06, viz.

$$x = r\,[9{\cdot}7865707]\,\sin(v + 97°\,36'\,53''{\cdot}34)$$
$$y = r\,[9{\cdot}9989828]\,\sin(v + 14\;41\;53{\cdot}70)$$
$$z = r\,[9{\cdot}8998221]\,\sin(v + 105\;42\;19{\cdot}84)$$

The places of the comet, calculated from the above elements, are compared with the places observed at Berlin and Hamburg, and the differences are inconsiderable. Mr. Rumker states that Mr. Götze is now engaged in computing the remainder of the observations of this remarkable comet made at Bonn, Milan, &c.; after which he will proceed to determine with greater accuracy the sidereal revolution.

V. Elements of the Comet of Faye, 1843–4. Corrected by the Observations at Greenwich on Jan. 22, Feb. 19, 20, and 22; Paris, on Dec. 2; Cambridge, on Dec. 8 and 16; at Berlin, on Dec. 17; Hamburg, on Jan. 23; and Starfield, on January 13, and Feb. 19 and 22. By J. R. Hind, Esq. Communicated by the Rev. R. Main.

Epoch, 1844, Jan. 1·0, Greenwich Mean Time.

Mean longitude	60° 30′ 0″·2	
Longitude of perihelion on orbit.....	50 34 19·0	Mean equinox.
Argument of latitude in perihelion...	201 24 13·1	
∴ ☊	209 10 5·9	
Inclination	11 21 2·8	
Angle of eccentricity	33 30 52·3	

Mean daily sidereal motion... 477″·56329
Log. semi-axis major.......... 0·5806505
Period of sidereal revolution, 7yrs·43
Direct.

The elements represent the Greenwich observation on the 20th of February with errors of — 2″·3 in longitude and — 11″·0 in latitude.

The observation at Berlin on Dec. 17 gives + 8″·4 in longitude and + 24″·1 in latitude.

The period agrees exactly with that found by Plantamour, and very nearly with the results of Le Jeune, Faye, &c.

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ROYAL ASTRONOMICAL SOCIETY.

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Lord Wrottesley, Vice-President, in the chair.

Frederick Walter Simms, Esq. F.G.S., Civil Engineer, of Hythe, Kent, was balloted for, and duly elected a Fellow of the Society.

The following communications were read:—

I. Some remarks on the Telescopic Appearance of the Moon, accompanying a Model and a Drawing of a Portion of her Surface. By James Nasmyth, Esq.

The model and drawing submitted to the Society by the Author, represent a portion of the moon's surface of 190 by 160 miles, situated in the upper part of her left limb as seen in an inverting telescope, having in the centre the large crater marked in the Berlin chart No. 29, and named "Maurolicus."

The scale of the drawing is one-eighth of an inch to a mile. The telescopes employed were two Newtonian reflectors, one of 8¼ inches aperture, and 9 feet focal length, and the other of 12 inches aperture, and 13 feet focal length, the powers employed being 240 and 360. The author has, for the last four years, confined his attention almost exclusively to the nature and structure of the lunar disc, and he selected the portion above mentioned as a subject for a model by reason of its comprising in a small space most of the chief features which so remarkably distinguish her surface.

The model was constructed with a view of illustrating the close relationship which appears to exist between the structure of the lunar surface and that of a considerable portion of the earth, in regard to the similarity in the results of vast volcanic action.

The author, in reference to the nature of the peculiarities of the surface of the moon, first remarks on the *vast size* of the lunar craters as compared with those on the surface of the earth. Of these there exist some of the enormous magnitude of 150 miles in diameter, besides other circular formations, such as the "Mare Serenitatis," and "Mare Crisium," which are from 200 to 300 miles in diameter, and which evidently owe their form to volcanic action of prodigious central energy. This enormous effect, compared with that of volcanic agency on the earth's surface, will appear less surprising when we consider that the mass of the moon is scarcely the $\frac{1}{70}$th part of that of the earth, and that, consequently, the weight

of the materials acted on by the volcanic force is diminished very considerably compared with bodies on the earth's surface: the probable want of atmospheric resistance will also assist in accounting for the immensely greater effects produced. The beautiful and almost perfectly circular form of the majority of the lunar craters may be due to the absence of wind or other disturbing causes, permitting the discharged materials to perform the course due to the impulse comparatively free from all impediment.

There are several portions of the moon's surface which indicate that considerable *time* has elapsed between the formation of one crater and that of another, this being proved by the fact of the circular mound of the one overlaying that of the other. [The author here introduces a pen sketch illustrating this remark.]

Next to the circular form of the craters, the author considers that there is no feature more striking than the small cones or mounds which we observe in the centre of most of the craters. These he considers to be the result of the last expiring efforts of the volcanic action, as we find it to be the case in Vesuvius and other terrestrial volcanoes. Other cases exist in which there is no such central cone; but these may have resulted from the more sudden termination of the volcanic action which had permitted the fluid sooner to float across the bottom of the crater, and to form that plain, smooth surface which may be seen in a few cases. One has been, however, observed by the author in the upper part of the right limb of the moon, in which the lava had apparently kept flowing up so gently to the last as to leave the crater brim-full. [This is illustrated by a pen drawing.] The ruts or channels which may be distinctly observed in the sides or banks of the outside circular mounds, and which frequently extend to a considerable distance, prove that the matter discharged has not been entirely of a solid nature. Blocks of solid materials also appear to have been discharged with vast force and in vast quantity. They may, in many cases, be observed lying about the bases of the larger craters, where the surface is rendered quite rough by the quantity of such detached fragments.

The last peculiarity adverted to by the author consists in the bright lines which generally converge to a centre, and in which we frequently find a crater of very considerable magnitude. "Tycho," "Copernicus," and "Kepler," are remarkable examples of this appearance. The material of these bright lines is evidently of a much more reflective nature than the contiguous or general surface of the moon, and, in most cases, the interior of the crater to which they converge is equally resplendent. The author considers them to be derived from the same original cause which produced the central volcano from which they appear to diverge. It appears to him that they are produced by the flow of the molten lava through the vast cracks resulting from the great primary upheaving action which had burst upwards the solid surface of the moon, cracking it as a pane of glass does when broken by any pointed object. The centre of disruption has evidently been under the

great central volcano. The cracks have diverged on all sides from this centre of action; and the molten lava immediately flowing up would come forth in greatest quantity from the centre, and there result in and produce the great crater, while the radiating cracks would yield smaller portions *simultaneously* all along their course, however extended.

II. Observations of the Solar Eclipse of 1843, December 21, made at the Observatory of Trevandrum. By John Caldecott, Esq.

The account of the eclipse is communicated in a letter to Mr. Baily, dated February 21, but was unfortunately too late for the mail of that month. Mr. Caldecott having satisfied himself that the eclipse would be total, in latitude 11° $45'$ north, and longitude 5^h 3^m east, determined on proceeding to the neighbourhood of Tellicherry (on the coast), for the purpose of observing it. It was necessary to be very precise in this calculation, and also to be very certain of the geographical position of the spot selected for observing the eclipse, on account of the almost exact correspondence in size of the sun's and the moon's discs. Mr. Caldecott, in the first place, proceeded by sea to Mahe (a little to the south of Tellicherry), and thence up the Mahe river towards its source, and having, by a careful survey of the river, ascertained its position with respect to Mahe (whose latitude and longitude he had previously ascertained), he selected a clear and open space on a rising ground, about three miles north of it, and there pitched his tent at midnight of December 20. He had brought with him an excellent 30-inch achromatic telescope, having an object glass of $2\frac{1}{4}$ inches aperture, with an eyepiece giving a magnifying power of about 50, protected by a smoked glass gradually increasing in intensity from one end to the other, and sliding easily in a groove, wherewith to observe the eclipse; a sextant and artificial horizon, with a good pocket watch for time; and two native assistants, with an actinometer for observing the rate of decrease of solar radiation.

At daylight on the 21st (civil reckoning), the author mounted his telescope on a stand having a very smooth parallactic motion, and made all the necessary preparations, taking a set of altitudes of the sun's lower limb at about 7^h A.M., and stationing the native observers at the actinometer. He began watching the point of the sun's disc, where the edge of the moon's limb was to impinge at 7^h 35^m, the sky being at this time quite clear in every direction, with the exception of a few light fleecy clouds which hung about the sun, but which cleared away before the eclipse became total. The observations that follow were taken almost verbatim from the author's notes written during the progress of the eclipse. At 7^h 38^m 49^s, corrected mean solar time, civil reckoning, the contact was evident, though the real first contact was probably two or three seconds earlier. With one digit eclipsed the border of the moon was fully as tremulous as that of the sun, and no hollows or coruscations were to be seen. No appearance of any *inherently* bright spots about the moon's disc was observed about this time,

and no spots on the sun. The cusps were perfectly sharp and distinct; the colour of the sun a pearly white, inclining to a metallic or silvery white. With eleven digits eclipsed there was no appearance of a corona, but, shortly after, three protuberances were observed near the southern cusp, and none on the other. [This is illustrated by a drawing.] As the conjunction of the discs approached, Mr. Caldecott watched very closely for the "beads," but saw none until the very instant of conjunction, when the whole of the southern limb of the sun broke up into most beautiful beads of silvery light, with a clear but very fine line of light joining them, and extending somewhat beyond them, until lost in the corona of the opposite limb. [This is illustrated by a drawing.] These beads for the second or two of their remaining, appeared to form, break up, and form again, and resembled globules of mercury of different sizes in a state of violent agitation.

They never entirely disappeared, so that the sun wanted the *smallest* imaginable quantity of being *totally* eclipsed here. The corona was very faint and of a greenish yellow colour, occasioned doubtless by seeing it through a very thin film of smoke, for the total removal of the darkening shade had not been provided for. It was never quite dark, or too dark to read off the actinometer: *Venus*, *Saturn*, and *Arcturus* were seen by the author's servants, who had been instructed to watch for stars. Mr. Caldecott saw nothing of the luminous protuberances noticed by Mr. Baily and Mr. Airy, but the greatest obscuration was so momentary in this eclipse, that there was no time for the eye to give more than a glance round the disc before the sun and moon were again separating, and strong light was restored. Shortly after 9^h A.M. the author took another set of altitudes of the upper limb of the sun, and the appearances during the separation of the two bodies were observed every few minutes, but nothing worthy of remark was seen. The actinometer readings and the resulting radiation are given in an accompanying table, together with some occasional readings of a thermometer taken at irregular intervals during the progress of the eclipse. The impression on Mr. Caldecott's mind is, that the beads are the effect of the incessantly varying refraction of the atmosphere on the two limbs, causing each of them to have an apparently dancing or *bubbling* motion, and thereby producing the appearance of spots of light, with intermediate dark spaces or bands, after the body of the sun is in reality covered by that of the moon, just as a planet or bright star when dipping into the horizon is frequently seen on a fair, clear night at sea to disappear and reappear repeatedly. This appearance of beads he believes to have been observed hitherto only at the eastern and western limbs, and, as these approach to and recede from each other *gradually*, the dark spaces or lines are more distinct for the moment of their appearance than in this case, where the limbs just grazed along each other, as it were, and in which, therefore, the bright spots assumed more of the form of globules, altering their shape, size, and position, as the two bodies moved past each other, with inconceivable rapidity.

The corrected observed time of the termination of the eclipse was $10^h\ 28^m\ 51^s{\cdot}5$ A.M.

The Longitude of the place of observation is $5^h\ 3^m\ 4^s$ East.

The Latitude .... $11^\circ\ 44'\ 38''$ North.

III. Sextant Measures of the Sun at the Eclipse of the Sun on the 21st December, 1843. By Captain Sir Edward Belcher, R.N. Communicated by Captain Beaufort, R.N.

The observations consist of nineteen measures of the breadth of the illuminated portion of the sun during the progress of the eclipse. The longitude of the place of observation was $124^\circ\ 12'\ 30''$ east, the latitude $24^\circ\ 21'\ 20''$ north.

IV. On a Graphical Method of Predicting Occultations. By J. I. Waterston, Esq. Communicated by Captain Beaufort, R.N.

The following is the author's account of his method:—

"The following is a description of a graphical method of predicting occultations, which I have found useful in drawing the attention of some of my pupils to this valuable method of determining meridian distance.

"The prediction of the time is almost an essential preliminary to the observation, and the computation required for this, in the usual way, is such as to put it altogether beyond the reach of many who would otherwise be perfectly able and willing to make the observation, and who have many valuable opportunities in the course of their profession of improving geography by such means. The method is simply delineating the essential points in the orthographical projection of the moon's motion and that of the observer in a plane perpendicular to the direction of the star. This is simplified to the utmost by means of scales, and I find that, with a little practice, my pupils have no difficulty in predicting the time of observation to 1 or 1½ minutes, and this without much care being taken in the drawing. From several occultations, which I predicted and observed here, and from numerous examples taken from the Astronomical Society's *Transactions*, I find that, with ordinary care, the predicted time may be depended upon within one minute, and that the time occupied in doing so varies from 10 to 15 minutes. As this is sufficiently near for all the wants of the observer, it may, perhaps, serve to supersede the method of computation, which, in its simplest form, is an irksome task, and probably tends to make occultations less frequently observed than they otherwise might be.

"With officers duly initiated, and observatories at work, observing all that are visible as a regular part of their duty, the most valuable results to geography might be anticipated.

"I have appended a rule for computing the moon's right ascension and the consequent error of assumed longitude, which is derived from the same method of orthographical projection. It is rigidly correct in principle (with the exception of taking small arcs instead

of their sines, which in no case affects the result in any sensible degree), and the results of computation may be depended upon within a hundredth part of a second of right ascension if the data are correct, and a considerable error in the estimated longitude, will, I think, be found to have as little injurious effect as in any other method of computation.

"*Bombay, May 1, 1844.*"

V. Some Remarks on the Great Comet of 1843, as seen in the neighbourhood of Paramatta, New South Wales. By the Rev. W. B. Clarke. Communicated by Sir John Herschel.

The principal point to which the author directed his attention, during the time of the visibility of the comet, was the fact of the existence of a small train of light inclined at a small angle to the large train.

His own observations were corroborated by those of the Bishop of Australia, who had made distinct notes of its appearance. His lordship says:—

"On the evenings of Thursday the 2d instant, and again on Saturday the 4th, my attention was drawn to the remarkable spectacle of a definite portion of the tail being deflected from the axis, or direction in which the general body of light continued to proceed. Perhaps, about one-sixth of the train might be thus drawn aside from that which may be termed the natural direction, so as to form therewith, at the point of separation, an angle which I should calculate to be about three degrees .... Five-sixths of the whole body of light continued without interruption in the ordinary direction, the remainder deviating from it in the manner here stated."

VI. Observations made at the Observatory of Hamburg. By C. Rumker, Esq. Communicated by Dr. Lee.

The observations consist of:

1. Continuation of the Meridian Observations of the Moon from 1840, August 7, to 1841, February 12.

2. Observations of the Eclipse of the Moon of 1844, May 31.

3. Observations of *Pallas* and *Ceres* during their opposition, May 1844.

VII. Scheme of Planetary Elements. By S. M. Drach, Esq.

The Chairman reported to the Meeting, that her Majesty's Government had recently put the Society in possession of the remainder of the apartments under the ground floor; and that, after some necessary repairs, they will be ready for the reception of various instruments, books, &c. belonging to the Society, which have been hitherto deposited in various places without that regard to order and arrangement, which it will be possible to give them with the present enlarged accommodations of the Society.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *November* 8, 1844. No. 9.

G. B. Airy, Esq., President, in the Chair.

The Secretary read the following extract from the Minutes of Council, dated September 20, 1844:—

"The office of President having become vacant since the last Meeting of the Council, in consequence of the death of Mr. Baily, it was moved by Mr. Sheepshanks, seconded by Mr. De Morgan, and resolved unanimously,

That Mr. Airy be appointed to fill the office of President of the Society until the next Annual General Meeting.

Resolved also,

That Mr. Christie be appointed Vice-President in the room of Mr. Airy, until the next Annual General Meeting."

Among the presents received since the last Meeting of the Society, the following were announced:—

I. An equatoreal telescope, by Smeaton, presented by Mrs. Somerville, and accompanied by the following note:—

"*September* 12, 1844.

"Sir,—May I request you will have the goodness to present to the Royal Astronomical Society, in my name, an equatoreal, which was made by the late Mr. Smeaton the engineer, and left to me by his daughter, Mrs. Dixon, at her death, who had frequently told me that it was her father's invention, and the first instrument of the kind that had been constructed.

"I remain, Sir,

"Your obedient servant,

"Mary Somerville."

"Thomas Galloway, Esq.
"Secretary."

II. A cast taken from Sir Francis Chantrey's marble bust of Mrs. Somerville, presented by Frederick S. Archer, Esq.

III. Several copies of "Plane Tables for conveniently laying down Portions of a Celestial or Terrestrial Map," constructed by the late Rev. Francis Wollaston, presented by his daughter, Miss A. H. Wollaston.

The President announced that the office of Judge (for Great Britain) of the fulfilment of the conditions necessary for the award of the Comet Medal, offered by his Majesty Christian VIII., king of Denmark, having become vacant, his Majesty had been pleased to appoint him (the Astronomer Royal) to succeed to that office, and he read to the Meeting the conditions under which the medal would be granted from the *Monthly Notices*, Vol. III., p. 132.

For the convenience of Fellows and others who may not have the volume of the *Monthly Notices* at hand, the regulations are here reprinted, with the alterations which have been made in consequence of the death of Mr. Baily and Dr. Olbers of Bremen :—

"His Majesty the King of Denmark has been pleased to found a gold medal, of the value of twenty ducats, to be given to the first discoverer of a telescopic comet, subject to the following conditions, which are, in some respects, different from those published in the year 1832.

"1. The medal is to be given to the person who may first discover a telescopic comet (that is, a comet not visible to the naked eye at the time of its discovery), and not of known revolution.

"2. The discoverer, if in any part of Europe except Great Britain, must send *immediate* notice to Professor Schumacher, of Altona; and if in Great Britain, or any other quarter of the globe except Europe, must send *immediate* notice to G. B. Airy, Esq., Astronomer Royal, Royal Observatory, Greenwich.

"3. Such notice must be sent by the *first* post after the discovery, and in case no post should be established in the place, then by the *first* conveyance that presents itself, without waiting for more observations. A strict attention to this condition is absolutely necessary, for, when it is not complied with, the medal will not be awarded at all, if there be only one who has seen the comet; and, where it has been seen by more than one, it will be given to the discoverer next in order of time who does comply with this condition.

"4. The first notice should contain, not only the time of the discovery, as nearly as the same can be ascertained, in order to avoid any disputed claims, but also the best possible determination of the position of the comet, and the direction of its course, if these points can (even approximately) be ascertained from the observations of one night.

"5. If the first night's observations are not sufficient to determine all these points with sufficient accuracy, the discoverer must, as soon as he gets a second observation, send *another* communi-

cation as above directed, together with a statement of the longitude of the place, if it should not be a known observatory: but the hope of getting a second observation will not be admitted as an excuse for delaying the communication of the first.

"6. The medal is to be adjudged twelve months after the discovery of the comet, and no claim can be admitted after that period has elapsed.

"7. Professor Schumacher and Mr. Airy are to determine whether a discovery is to be considered as established or not: but should they differ in opinion, Professor Gauss, of Göttingen, is to decide between them."

The President then announced that, in pursuance of a resolution of the Council, which had been duly intimated to the Fellows, as required by the Bye-Laws, the business of the Ordinary Meeting would now be concluded, and that a Special General Meeting would be held immediately, for the purpose of hearing read a Memoir of the late President, which Sir John Herschel, upon the request of the Council, had undertaken to prepare.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *November* 8, 1844. No. 10.

At a Special General Meeting of the Society, called in pursuance of a Resolution of the Council on the 20th of September last,

G. B. Airy, Esq., President, in the Chair,

Sir J. F. W. Herschel read the following Memoir:—

In the performance of the melancholy duty imposed on me by the wishes of the Council, that I should endeavour, on this occasion, to place before the assembled Members of this Society a sketch of the scientific life and character of our late lamented President, I have been careful both to examine my own competency to the task, and to consider well the proper limits within which to confine myself in its execution. In the first of these respects, indeed, though tolerably familiar with some of the leading subjects which I shall have to touch upon, there are others on which I have seriously felt the want of a longer interval for preparation. On these, of course, I shall take care to express myself with becoming diffidence; and in so vast a field of laborious inquiry and of minute, yet important research as I shall have to range over, it may easily be supposed I have more than once found occasion to wish that the duty had fallen into abler hands. A duty, however, it is, and a very sacred one, which we owe to departed merit, to society, and to ourselves, to fix as speedily as possible, while its impress is yet fresh and vivid upon us, its features in our minds with all attainable distinctness and precision, and to store them up beyond the reach of change and the treachery of passing years.

As respects the limits within which I feel it necessary to confine myself on this occasion, it is to astronomers to whom I have to speak of an astronomer—to members of a large and, in the simplicity of truth I may add, a highly efficient public body—of an officer to whom more than to any other individual, living or dead, it owes the respect of Europe. To make what I have to say complete as a biography, however interesting to us all, however

desirable in itself, is very far either from my intention or my power. Nor is the time fitting for the attempt. The event is too recent, the particulars which can be collected at the present moment too scanty, the grief of surviving relations too fresh, to admit of that sort of close and pertinacious inquiry into facts, anecdotes, documents, and evidence, which personal biography requires to be satisfactory. In this respect, therefore, a mere sketch is all that I can pretend to give.

Francis Baily was born on the 28th of April, 1774, at Newbury, in the county of Berks. His father was Mr. Richard Baily, a native of Thatcham, in the same county, who became established as a banker at Newbury. He married Miss Sarah Head, by whom he had five sons and two daughters. Francis, who was the third son, received his education at the school of the Rev. Mr. Best, of Newbury, an establishment of considerable local reputation, where, although probably little of an abstract or mathematical nature was imparted, the chief elements of a liberal and classical education were undoubtedly communicated. From his early youth, he manifested a propensity to physical inquiry, being fond of chemical, and especially of electrical experiments,—a propensity sufficiently marked (in conjunction with his generally studious habits) to procure for him, among his young contemporaries, the half-jesting, half-serious *soubriquet* of "the Philosopher of Newbury."

It does not appear that he received any further instruction beyond the usual routine of an establishment of the kind above mentioned, so that, in respect of the sciences, and especially of that in which he attained such eminent distinction, he must be regarded as self-educated. This taste for and knowledge of electricity and chemistry were probably acquired from Dr. Priestley, with whom, at the age of seventeen, he became intimately acquainted, and of whom he always continued a warm admirer. But that his acquaintance with the subject was considerable, and his attachment to it permanent, may be concluded from the fact, that Mr. Welsh, the organist of the parish church of Newbury, who had a very pretty electrical apparatus, and at whose house I remember myself to have first witnessed an electrical experiment, is stated to have imbibed his taste for that science, and to have acquired its principles, from his example and instructions at a somewhat subsequent period.

He quitted Mr. Best's school at fourteen years of age, and, having chosen a mercantile life, which accorded with the views of his parents, he was sent to London, and placed in a house of business in the City, where he remained till his twenty-second year, when, having duly served his time, and either not feeling an inclination to the particular line of business in which he had commenced his life, or being desirous of the general enlargement of mind which travel gives, or from mere youthful love of adventure and enterprise, he embarked for America on the 21st October, 1795, which, however, he was not destined to reach without twice incurring the most imminent danger from shipwreck, both on our

own coast, under most awful circumstances, on the Goodwin Sands, and off New York, which he was prevented from reaching, being driven to sea in a gale, and, after endeavouring in vain to reach Bermuda, was driven into Antigua, whence he subsequently embarked for Norfolk, in Virginia.

In America, he remained one or two years, travelling over the whole of the United States and through much of the western country; in which travel he experienced, at various times, much hardship and privation, having, as I remember to have heard him state in conversation (and which must have referred to this period of his life), passed eleven months without the shelter of a civilized roof. During his residence in America, he was not unmindful of his intellectual and social improvement, having not only read much and observed much, as a copious journal which he transmitted home proves, but formed the acquaintance of some eminent persons, among whom may be mentioned Mr. Ellicot, the Surveyor-General of the United States, from whom he obtained some curious information bearing on the periodical displays of meteors on the 12th November, of which that gentleman observed a superb instance in 1799, and from whom it is not impossible he may have acquired a taste for observations of a more distinctly astronomical and geographical nature.

Whatever may have been the more direct object of this journey, if indeed it had any other than to gratify a youthful inclination for travel and adventure, it does not appear to have exercised any material influence on his after-life, since, on his return to England, in place of immediately entering into business, he continued to reside for some time with his parents at Newbury, which, however, at length he quitted for London, to engage in business as a stock-broker, being taken into partnership by Mr. Whitmore of the Stock Exchange. The exact date of this partnership I have not been able to learn. I believe it to have been 1801; but that it must have been prior to 1802, may be concluded from the subject of his first publication, which appeared in that year, viz. *Tables for the Purchasing and Renewing of Leases for Terms of Years certain and for Lives, with Rules for determining the Value of the Reversions of Estates after any such Leases.* This work (as well as the next) is preceded by a highly practical and useful Introduction, and followed by an Appendix, which shews, that at the age of twenty-eight he had become well versed in the works of the English mathematicians, and had also consulted those of foreign ones. It speedily attained a standard reputation on account of its intrinsic utility, and went through several editions. His next work, a pamphlet in defence of the rights of the Stock-Brokers against the attacks of the City of London, printed in 1806, at all events shews him at that time to have become identified in his feelings and interests with that body of which he lived to be an eminent and successful member. A similar conclusion may be drawn from his next publication, which appeared in 1808, *The Doctrine of Interest and Annuities Analytically Investigated and Explained,* a work than which no

one more complete had been previously published, and which is still regarded as the most extensive and standard work on compound interest. It was speedily followed by other works on the same subject, viz. in 1810, by *The Doctrine of Life Annuities and Insurances Analytically Investigated and Explained;* to which, in 1813, he added an Appendix. This is a work in many ways remarkable, and its peculiarities are of a highly characteristic nature; method, symmetry, and lucid order being brought in aid of practical utility in a subject which had never before been so treated, and old routine being boldly questioned and confronted with enlarged experience. A friend of great mathematical attainments and extensive practical acquaintance with subjects of this nature, thus characterizes it:—" It is not easy to say too much of the value of this work in promoting sound practical knowledge of the subject. It was the first work in which the whole of the subject was systematically algebraized; the first in which modern symmetry of notation was introduced; and the first modern work, since Price and Morgan, in which the *Northampton Tables* were not exclusively employed, and in which the longer duration of human life was contended for; and the first in which some attempt was made to represent by symbols the various cases of annuities and assurances, afterwards more systematically done by Mr. Milne." In the Appendix to this work, a method originally proposed by Mr. Barrett of forming the tables, by which cases of temporary and deferred annuities, formerly requiring tedious calculations, become as easy as the others, and which, in the improved form subsequently given to it by Mr. Griffith Davies, has come into very general use in this country, was, by the penetration of Mr. Baily, given to the public, but for which it would probably have been altogether lost. It may serve to give some idea of the estimation in which this work was held, that when out of print, its copies used to sell for four or five times their original price. A chapter of this work is devoted to the practical working of the several life assurance companies in London, containing some free remarks on several points of their practice. Mr. Babbage has subsequently followed in the same line (as he has also advocated extending the estimation of the duration of life to still more advanced ages). However unpleasing it may be to public bodies, especially commercial ones, to see practices of whose injustice they may perhaps have been unaware, convicted of it, and made matter of public animadversion, there can be no doubt that criticisms of this kind, when really well grounded and expressed with temperance and moderation, are both salutary to the parties concerned, and merit, in a high degree, the gratitude of the public. A higher praise is due to the candour and boldness of openly entering the lists on such occasions, and despising the anonymous shield of which so many avail themselves.

But while devoting his attention thus assiduously to matters of direct commercial interest, he could yet find time for other objects of a more general nature. Astronomical pursuits had already begun to assume in his eyes that attraction, which was destined ultimately to

draw him aside entirely from business, and to constitute at once the main occupation and the chief delight of his life. As every thing to which he turned his thoughts presented itself to them, if I may use the expression, in the form of a palpable reality, a thing to be turned and examined on all sides—to be reduced to number, weight, and measure—to be contemplated with steadiness and distinctness, till every thing shadowy and uncertain had disappeared from it, and it had moulded itself, under his scrutiny, into entire self-consistency, the practical branches of astronomical calculation early became, in his hands, instruments of the readiest and most familiar application, as the touchstones of the truth of its theories and the means of giving to them that substantial reality which his mind seemed to crave as a condition for their distinct conception by it. His first astronomical paper, on the celebrated solar eclipse, said to have been predicted by Thales, which was written in November 1810, and read before the Royal Society on the 14th March, 1811, affords a remarkable instance of this. That eclipse had long been a disputed point among chronologists. It was easy to perceive, and accordingly all had perceived, that an eclipse of the sun, so nearly central as to produce great darkness, being a rare phænomenon in any part of the globe, and excessively so in any precisely fixed locality, must afford a perfectly certain means of determining the date of a coincident event, if only the geographical locality be well ascertained, and some moderate limits of time within which the event must have happened be assigned, and provided the means were afforded of calculating back the moon's place for any remote epoch. In this case, both the locality and the probable historical limits were sufficiently precise; and the account of Herodotus, which agrees only with the character of a total and not of an annular eclipse (as Mr. Baily was the first to remark) still further limits the problem. But the tables of the moon employed by all prior computists were inadequate to carry back her place with the requisite exactness, nor was it till the publication of Burg's *Lunar Tables* that the means of doing so were in the hands of astronomers. The course of Mr. Baily's reading at this period (being then, no doubt, employed in collecting the materials for the Chronological Tables in his *Epitome of Universal History*, which appeared not long after) brought him necessarily into contact with this subject. He perceived at once both the uncertainty of all former calculations of this eclipse, and the possibility of attacking it with a fresh prospect of success. None, however, but a consummate astronomical calculator would have ventured on such an inquiry, which involved the computation of all the solar eclipses during a period of seventy years, six centuries before the Christian era. These calculations led him to assign, as the eclipse in question, that of September 30, B.C. 610, which was central and total, according to these tables, at the very point where all historical probability places the scene of action.

Most men would have regarded such a result, obtained by so much labour, with triumphant complacency: not so Mr. Baily.

His habit of examining things on all sides, instead of permitting him to rest content with his conclusion, led him on to further inquiry, and induced him to calculate the phænomena of another total eclipse recorded in ancient history, that of Agathocles, which happened August 15, B.C. 310, an eclipse of which neither the date nor the locality admits of any considerable uncertainty, and which, therefore, appeared to him well fitted to test the accuracy of the tables themselves. Executing the calculation, he found indeed a total eclipse on the year and day in question, and passing near to the spot, *but not over it*. An irreconcilable gap of about 3°, or 180 geographical miles, remains between the most northerly limit of the total shadow, and the most southerly supposable place of Agathocles's fleet. Although this may justly be looked upon as a wonderful approximation between theory and historical fact (indicating, as it does, a correction of only 3′ in the moon's latitude, for an epoch anterior by more than 21 centuries to that of the tables), yet it did not escape Mr. Baily's notice, nor did his love of truth permit him to conceal the fact, that no presumed single correction of the tabular elements will precisely reconcile *both* eclipses with their strict historical statement. There seems, however, no reason to doubt that the eclipse of 610 B.C. is, in fact, the true eclipse of Thales. It seems extraordinary that neither Professor Oltmanns, who investigated the eclipse of Thales about two years subsequently, and who came to the same conclusion, nor M. Saint Martin, who read an elaborate memoir on the same subject to the French Institute in 1821, should have made any mention of this very remarkable paper of Mr. Baily.

The *Epitome of Universal History*, of which mention has already been made, was published in 1813, and intended to accompany an *Historical Chart* published the year before, an extension and improvement of Dr. Priestley's, in which the political alterations of territory are represented through the whole of history. It is an easy and useful work of reference, in which the number and accuracy of the dates, and the utility of the appended tables, are especially valuable. There can be little doubt that the object of this work was much less to produce a book than to systematise and concinnate the author's own knowledge. When such a task is undertaken by a mind at once vigorous in its grasp, and simple, practical, and natural, in its points of view, it can hardly fail to result in a picture of the subject where all the parts are truly placed, and easily apprehended by the general reader. The chart with its explanation, forming a distinct work, was in considerable request, and went through three editions in five years.

About the 22d of January, 1814, occurred the celebrated fraud of De Beranger, that being the assumed name of an impostor employed to bring important but false intelligence from the scene of war abroad, for the purpose of influencing the price of the British funds. The imposture was so adroitly managed that many bargains were made on the strength of this intelligence, and much confusion caused. In the detection and exposure of this fraud,

Mr. Baily had a considerable share, and was appointed by the committee of the Stock Exchange to get up the evidence against the perpetrators,—a task which he is said to have performed in so masterly a manner, that no more complete and conclusive chain of evidence was ever produced in a court. The result of these inquiries, and the steps taken in consequence, were made the subject of three Reports of the above-mentioned committee, drawn up by him, and printed in that and the subsequent year.

From this time, astronomy appears to have been continually engaging more and more of his attention. The subject of eclipses and occultations with their connected calculations, together with that of the improvement of the Nautical Almanac, which, whatever might be said on specific points, had certainly, at that time, begun to fall considerably behind the requisitions of astronomical, and even of nautical science, were those with which he may be said to have commenced his more active astronomical career. But I wish to call attention at present to two pamphlets which he published in 1818 and 1819 respectively, which will afford occasion for some remarks of moment. The first of these is a notice of the annular eclipse of September 20, 1820, whose path lay along the whole medial line of Europe from north to south. Two points in this tract merit our attention. In it he adopts a practice, which he subsequently on a great many occasions adhered to, of introducing in the way of prefatory statement a brief but very clear sketch of the history of the subject, and the observations of former astronomers. These little historical essays are for the most part extremely well drawn up, and highly interesting, and shew a perfect knowledge of the subjects treated of, drawn from very extensive reading. The next point, and one of more importance, is the studious consideration shewn to observers possessed of slender instrumental means, in pointing out to them modes and forms of observation by which those means might be rendered available and useful. At no period of his life himself possessing any large and elaborate instrument or luxurious appliances, one of his constant aims was to render astronomical observation popular and attractive by shewing that much of a highly useful character might be accomplished with even moderate instruments. There is no question more frequently asked by the young astronomer who has possessed himself of one or two tolerably good instruments which he desires to employ his time upon, than this, "How can I make myself useful?" nor any which can be more readily answered by a reference to the innumerable notices on almost every point of practical astronomy which Mr. Baily from this time forward for many years continued to scatter profusely to the public, and which have probably done more to create observers, and to cherish and foster a taste for practical astronomy among Englishmen, than any single cause which can be mentioned.

In 1819 he printed for private distribution a translation of Cagnoli's memoir on a "Method of deducing the Earth's Ellipticity from Observations of very Oblique Occultations," with an appendix

recommendatory of the method which is precisely such as requires for its perfect execution only a sufficient telescope, a moderately good clock, and an observer diligent in watching opportunities. This was, no doubt, Mr. Baily's chief reason for translating and distributing it, and for subsequently following it up by his chart and catalogue of the Pleiades, through which the moon had to pass at each lunation in 1822 and the following years, thereby affording admirable opportunities for applying the principle in question. I should not, however, have thought it necessary, in the midst of so many claims on our notice, to draw especial attention to this work, but for one passage in it deeply interesting to all of us. I mean that in which he alludes to the formation of an Astronomical Society as an event earnestly to be desired.

"It is much to be regretted," he observes, "that in this country there is no association of scientific persons formed for the encouragement and improvement of astronomy. In almost all the arts and sciences institutions have been formed for the purpose of promoting and diffusing a general knowledge of those particular subjects......the beneficial effects of which are too evident to be insisted on in this place. But astronomy, the most interesting and sublime of the sciences......cannot claim the fostering aid of any society.......The formation of an ASTRONOMICAL SOCIETY would not only afford this advantage, but would in other respects be attended with the most beneficial consequences," &c. &c.

It is thus that coming events cast their shadows before them. But looking back from this point, as it were, to the then embryo state of our corporate existence, it would be ungrateful not to associate with the name of Francis Baily that of Dr. Pearson, as having at or about the same time made the same suggestion. It was happily and speedily responded to, and on Wednesday the 12th of January, 1820, a preliminary meeting of the fourteen founders of our Institution took place, which resulted in its final establishment, and in which during the first three years of its existence Mr. Baily filled the office of secretary, in other words, undertook and executed the more laborious and essential duties. The establishment of this Society may, indeed, be considered as a chief and deciding epoch in his life, and to have furnished, though not the motive, yet, at least, the occasion, for the greater part of his subsequent astronomical labours. Looking to it, as every one must do, as a most powerful instrument for the advancement of the science itself, and the propagation of a knowledge of and a taste for it among his countrymen, he yet appeared to regard it as something more than simply as a means to an end. He made it an object of personal attachment and solicitude, which led him to watch over its infant progress with parental care, and to spare no exertion in its behalf. As years passed on, and as the Institution flourished (as every institution must do which is constituted on sound principles, whose members are loyal to those principles, and willing to work heartily in its cause), this sentiment, so far from diminishing, seemed to grow upon him till he regarded its welfare

and interests as identical with his own. I shall reserve a more distinct statement of our obligations to him for a more advanced period of this notice; but in a narrative of his life it becomes impossible from this epoch to separate the Astronomical *Society* from astronomical *science*, in our estimate of his views and motives, or to avoid noticing the large and increasing devotion to its concerns of his time and thoughts. To the Transactions of the new Society he became, as might be expected, a frequent and copious contributor. In the interval between the first establishment of the Society and the year 1825 (the reason for this limit will presently be seen), he contributed five papers, viz.: "On the Meridian Adjustment of the Transit Instrument;" "On the Determination of Time by Altitudes near the Prime Vertical;" "On the Solar Eclipse of September 20, 1820;" "On the Mercurial Compensation Pendulum;" and "On the Determination of Longitudes by Moon-culminating Stars." The two first-mentioned of these turn on somewhat elementary points of astronomical observation, and contain tables, and suggest facilities, which he had found useful in his own practice. The eclipse was observed by him at Kentish Town, where not being annular, he must have felt severely the sacrifice, imposed probably by the calls of business, of the opportunity of witnessing by a short continental trip, a phenomenon which had engaged so much of his thoughts. His paper on the Mercurial Pendulum, though practical in its object, was of a much more elaborate kind than any thing which had previously emanated from him, with exception of his memoir on the eclipse of Thales. It contains a minute and excellent view of the whole subject of this most useful compensation; is prefaced (*more suo*) with a clear synoptic view of the then actual state of the subject, and goes into the whole subject of the expansion of the materials, the formulæ for determining with more precision than heretofore the proportional length of the mercurial column, and the mode of adjustment both for rate and compensation. This paper must certainly be regarded as a very valuable one, and an astronomer can hardly be said thoroughly to understand his clock who does not possess it. The object of the paper on moon-culminating stars is to recommend, facilitate, and render general, that most useful and widely available method of determining the longitude on land.

About this period, also, Mr. Baily began, and thenceforward continued, to be a frequent contributor to the *Philosophical Magazine*, published by Messrs. Tilloch and Taylor, of articles interesting in a great variety of ways to the practical astronomer. These articles are so numerous, and so miscellaneous in their subject matter, that it would be vain to attempt any detailed account of them, within such limits as I must confine myself to. Nor, indeed, is it requisite to do so; as many of them, however useful at the time, have now ceased to present any especial interest, apart from their general object, which was that of diffusing among the British public a knowledge of the continental improvements in the art of observing, and the practice of astronomical calculation, and placing

in the hands of our observers and computers a multitude of useful tables and methods, which, though sure to work their way ultimately into use, were undoubtedly accelerated in their introduction into English practice by coming so recommended. More special objects were those of recommending to general attention and use certain eminently practical methods, such as those of determining latitudes by the pole-star, longitudes by moon-culminations and occultations, copious lists of which were, on several occasions, either procured from abroad and reprinted here, or calculated by himself for the purpose.

The circulation of notices, also, of other remarkable expected phenomena, with a view to procuring them to be observed,—the description of newly invented foreign instruments, or of such as had been long known but little used in England,—the analysis of foreign astronomical publications,—every thing, in short, which could tend to excite curiosity, to cherish emulation, and to render the British astronomical mind more excursive and more awake than heretofore, found a place in these contributions; of which so constant and copious a fire was kept up, as may well excite our surprise at the industry which sustained, no less than our admiration of the zeal which prompted it.

A volume of astronomical tables and formulæ, printed in 1827 for private distribution (as was frequently his custom), and then largely circulated, but since published with corrections, is of the utmost convenience and value, and will be highly prized by every astronomer who may be fortunate enough to possess a copy, as a work of ready and continual reference for all the data and coefficients of our science. A series of zodiacal charts was also commenced by him, but I am not able to say if more than one plate was engraved.

One of the most practically important and useful objects, however, to which Mr. Baily's attention was about this period turned, was the facilitating, by tables properly contrived for the purpose, the reductions of apparent to mean places of the fixed stars. It seems almost astonishing that these computations, which lie at the root of all astronomy, and without which no result can be arrived at, and no practical observer can advance a single step, should have remained up to so late a period as the twentieth year of the nineteenth century, in the loose, irregular, and troublesome state which was actually the case, and *that* not from their theory being ill understood, but from their practice not having been systematized. Each of the uranographical corrections had to be separately computed by its own peculiar tables, and with coefficients on whose magnitude no two astronomers agreed. The latter evil, indeed, might be tolerated at a time when the tenth of a second of space was not considered of so much consequence as at present, but the calculations were formidable and onerous in the extreme to private astronomers, whatever they might be rendered in public establishments by habit and the use of auxiliary tables. So far as the fundamental stars were concerned, the subject had for some time attracted attention, and had begun to receive its proper remedy by

the publication, by Professor Schumacher in Denmark, of their apparent places for every tenth day; and by the laudable exertions of Sir James South in our own country, who, for some years, prepared and circulated similar tables for every day, not without urgent representations of the necessity of taking it up as a public concern, which was at length done. But for stars out of this list, except about 500 somewhat facilitated by Zach, there was no provision of any kind, nor any auxiliary tables to have recourse to; so that sidereal astronomy, beyond the bounds of this favoured list, might be almost said to be interdicted to the private astronomer, owing to the excessive irksomeness of these calculations. This was precisely the sort of case for Mr. Baily to take pity on. He perceived a desert where, with a moderate expenditure of capital, a plentiful harvest might be made to grow, and forthwith proceeded to remedy the evil. Accordingly, with the aid of Mr. Gompertz, he investigated the subject generally, and succeeded in devising a method of arranging the terms of the corrections for aberration, solar and lunar precession, adapted to the purpose, and identical in principle with that adopted by M. Bessel, who, on his part, was at the same time, and, actuated by the same motives, engaged on the subject unknown to Mr. Baily. The latter had actually proceeded to the computation of his tables, when the labours of Bessel reached his knowledge, who had, moreover, included the precession under the same general mode of expression. Mr. Baily, with characteristic frankness and candour, immediately acknowledged this as an improvement in advance of his own idea, and at once adopted and recommended it for general use. He did more, he carried out the idea into a wide and most useful field; and in the Catalogue of the Astronomical Society he has put the astronomical world in possession of a power which may be said, without exaggeration, to have changed the face of sidereal astronomy, and must claim for him the gratitude of every observer. It detracts nothing from the merit of Mr. Baily, or from his claim to be considered the author of this precious work, that the numerical computations were chiefly executed by Mr. Stratford, and the expenses borne by the Astronomical Society. The conception was all his own, and the work prefaced, explained, and superintended, in every stage of its progress, by himself alone. The gold medal of this Society was awarded to him for this useful work.

On the 22d February, 1821, Mr. Baily was elected a Fellow of the Royal Society. He was also a Member of the Linnean and Geological Societies, but I am unable to state the precise date of his election in either.

In 1825 he retired from the Stock Exchange, after a career in which his consummate habits of business, his uprightness, intelligence, and prudence, had established his fortune, and might, if continued, have led him on to any eminence of worldly wealth. But there was that in his disposition which the mere acquisition of wealth could not satisfy. All that he had before done for his favourite science seemed only preparatory to what he might do; and with

the best years of his intellectual life before him, and with objects worthy of his efforts now opening to his view in that direction, he resolved henceforward to devote himself to their pursuit, though at the sacrifice of prospects whose attractions always prove irresistible to minds of a lower order. In thus calmly measuring the relative worth of intellectual and worldly pursuits, and stopping short in the full career of success, when arrived at a point which his undazzled judgment assured him to be the right one, he afforded an example of self-command as uncommon as it was noble. In the satisfaction which the decision afforded him, and the complete fulfilment of those aspirations which led him to form it, we have one proof (if proofs be wanting) how entirely a well-chosen and elevated scientific pursuit is capable of filling that void in the evening of life, which often proves so intolerably irksome to men who have retired early from business from mere love of ease or indolence. On no occasion did he ever appear to regret the sacrifice he had made, or even to regard it as a sacrifice.

No desire of listless ease or self-indulgence, however, could by possibility have mixed with Mr. Baily's motives in taking this step; for immediately on doing so he entered on a course of devoted and laborious exertion, which continued without interruption during the remainder of his life, and of which the history of science affords few examples. The mass of work which he got through, when looked at as such, is, in fact, appalling, and such that there seems difficulty in conceiving how it could be crowded into the time; the key to which is, however, to be found in his admirably conceived methodical arrangement of every piece of work which he undertook, and his invaluable habit of finishing one thing before he undertook another.

At this epoch, or very shortly subsequent to it, he purchased and took up his permanent residence in his house in Tavistock Place, excellently adapted in every respect both to his future comfort and convenience as a place of abode, and for those important and delicate researches of which it was destined to become the scene; standing, as it does, insulated in a considerable garden, well-enclosed on all sides, and, from the nature of the neighbourhood, free from any material tremor from passing carriages. A small observatory was constructed in the upper part, for occasional use and determination of time, though he never engaged in any extensive series of observation. The building in which the earth was weighed and its bulk and figure calculated, the standard measure of the British nation perpetuated, and the pendulum experiments rescued from their chief source of inaccuracy, can never cease to be an object of interest to astronomers of future generations.

In endeavouring, according to the best of my ability, to give some account of the astronomical labours of Mr. Baily subsequent to this period, it will no longer be advisable to adhere, as I have hitherto done, to the chronological order in which they were undertaken and executed. It will rather be preferable (with exception

of a few memoirs and publications of a miscellaneous nature) to consider them under distinct heads, according as they refer to one or other of the following subjects, viz.:—

1. The Remodelling of the *Nautical Almanac;*
2. The Determination of the Length of the Seconds-Pendulum;
3. The Fixation of the Standard of Length;
4. The Determination of the Density of the Earth;
5. The Revision of Catalogues of the Stars;
6. The Reduction of Lacaille's and Lalande's Catalogues; and,
7. The Formation of a New Standard Catalogue.

The Nautical Almanac.—The end of the eighteenth and the commencement of the nineteenth century are remarkable for the small amount of scientific movement going on in this country, especially in its more exact departments. It is not that individuals were not here and there busied in extending the bounds of science even in these, but they met with little sympathy. Their excursions were limited by the general restriction of view which had begun to prevail, and by a sense of loneliness and desertion (if I may use such an expression) arising from that want of sympathy. Mathematics were at the last gasp, and astronomy nearly so; I mean in those members of its frame which depend upon precise measurement and systematic calculation. The chilling torpor of routine had begun to spread itself largely over all those branches of science which wanted the excitement of experimental research. I know that I have been blamed on a former occasion for expressing this opinion, but it is not the less true, though we may now happily congratulate ourselves that this inanimate period has been succeeded by one of unexampled activity. To break the dangerous repose of such a state, and to enforce that exertion which is necessary to healthy life, there is always need of some degree of friendly violence, which, if administered without rudeness, and in a kindly spirit, leads at length the revived patient to bless the disturbing hand, however the urgency of its application might for a moment irritate. It is in this light that we are to regard the earnest and somewhat warm remonstrances of Mr. Baily on the deficiences which had long begun to be perceived and felt in the *Nautical Almanac,* in its capacity of an astronomical ephemeris.

The subject once moved gave rise to a great deal of discussion, from more than one quarter, which was from time to time renewed for some years; but as I have no intention to make this notice an occasion of dilating on any matter of a controversial nature, I shall merely add that, on the dissolution of the late Board of Longitude, followed almost immediately by the death of Dr. Young, on whom the charge of its superintendence rested (the new Berlin Ephemeris, by Encke, having also recently appeared, in which many of the principal improvements contended for were adopted), it seemed fitting to the Lords Commissioners of the Admiralty to place unreservedly before the Astronomical Society the

subject of a complete revision and remodelling of that great national work—a high proof of confidence, which speaks volumes for the good sense, prudence, and activity which had continued to pervade its administration during the ten years which had now elapsed since its first institution.

It is hardly necessary to add that this important business received the most unremitting attention from Mr. Baily, as well as from every other member of the Committee, in all its stages. To him also was confided the task of drawing up the final report of the Committee appointed to carry out the wishes of the Admiralty, which will be found in the fourth volume of our *Transactions*, and which is a model of good sense, clearness, and lucid arrangement. The Report was immediately acted upon by Government, and the result was the present British *Nautical Almanac*; a work which, if it continue to be carried on, as I trust it ever will, on the principles which prevailed in its reconstruction, will remain a perpetual monument to the honour of every party concerned in it.

The Pendulum. — The seconds-pendulum having been constituted the legal source from which, in the event of the loss of the national standard of length, the yard might at any time be recovered, it may be easily imagined with what intensity of interest the announcement was received among all conversant with these fundamental determinations, that a very material correction had been entirely overlooked in the reduction of the experiments, on which the Act of 5 Geo. IV. c. 74 was founded. This correction is, in fact, no other than the correction due to *the resistance of the air*, and, placed in this light, it would seem somewhat wonderful that such an oversight could have been committed; but it had been customary to consider the effect of resistance on the time of vibration to be wholly confined to its influence in diminishing the arc, and this secondary effect being allowed for in the formulæ employed to compute what is called the correction for the arc of vibration, the primary or direct effect of resistance dropped altogether out of notice, or, rather (owing to an entire misconception of the nature of the mechanical process by which resistance is operated), had been supposed to be altogether inappreciable in its amount. The real effect of resistance, though under a somewhat confused statement as to its nature, had, however, been long before noticed, and its amount even ascertained with tolerable correctness by the Chevalier Buat, in 1786; but his experiments and theory had so entirely fallen into oblivion as to have escaped the notice not only of Captain Kater, but of his own countrymen, Borda and Biot, and were unknown even to Bessel himself, who, in 1828, rediscovered the correction in question, and, for the first time, made it an integrant feature in the modern system of pendulum reductions. The light in which this correction was placed by Buat, and even in some respects by Bessel, tended not a little, in my opinion, to obscure the clear perception of its nature, by representing it as due to a certain portion of air adhering to and bodily *dragged along* by the pendulum in its motion, thus adding to its inertia without ad-

ding to its relative weight when corrected for buoyancy; and in this view, also, Mr. Baily regarded it. That this is not a complete and adequate view of the subject is easily made a matter of ocular inspection, by causing a pendulum to vibrate, or any body to move, near the flame of a candle, when it will be at once evident that the movement of the air consists in the continual transfer of a portion of air from the front to the rear of the body, by performing a circuit half round it. Its hydrodynamical investigation, therefore, is of an infinitely higher order of difficulty than the ordinary problems of resistance, which turn upon a theory of molecular impulse, simple indeed, but very far from satisfactory. It properly refers itself to the theory of sound, and has, in fact, been so investigated in an admirable memoir by Poisson.*

But to return from this digression (which, however, will not have been without its use, if it shall tend to diffuse clear conceptions of the subject, and to disentangle from one another corrections which seem to have got unduly mixed up together in the minds of practical inquirers). No sooner were the ideas of M. Bessel promulgated in England than Captain Sabine, whose attention was pointedly directed to a subject which had occupied so large and active a portion of his life, resolved to ascertain the true amount of this new, or newly mentioned, correction, in the only way in which it could be effectually done, viz. by vibrating the pendulum *in vacuo*, which he accordingly effected by a series of highly interesting experiments, carried on at the Royal Observatory at Greenwich, and recorded in the *Philosophical Transactions*, in a paper read March 12, 1829. His result makes the total reduction to a vacuum about one and two-thirds of that usually called "the correction for buoyancy." It should, however, be borne carefully in mind that the particular correction now in question has, in fact, nothing whatever to do with the buoyancy correction, either in its mode of production or its form of expression, and ought, therefore, to be very studiously kept apart from it in all theoretical views, though of course they must be numerically amalgamated in the "reduction to a vacuum."

Meanwhile the attention of Mr. Baily had, about the same time, been called to the pendulum, in consequence of the contemplated expedition about to sail under the command of Captain Foster, on that memorable and most unfortunate expedition which cost him his life. It was on this occasion, and with a view to the use of this expedition, that Mr. Baily (still acting for the Astronomical Society, whose aid had been requested in suggesting useful objects of inquiry) devised that capital improvement in the

* If this view of the subject be correct, as I am persuaded it is, it seems not impossible that, by making a section of the pendulum coincident in form with the "wave-formed outline" of Mr. Russel's ships, the resistance correction might be annihilated altogether, or so nearly as to render it quite inappreciable.

I trust that, in what is said above, I shall not be supposed to undervalue M. Bessel's analytical treatment of this intricate problem, especially as it conducts to results which, regarded as a first approximation, represent sufficiently well the results of experience.

system of itinerant pendulum observation, which consists in making each transferable pendulum a convertible one, by the simple addition of another knife-edge, and in doing away with extra apparatus of tail-pieces, sliders, &c., by the initial adjustments of the instrument. And I may here incidentally remark, that the general principles of reducing, as far as possible, the number of moveable parts in every instrument intended for standard determinations of whatever kind, is one which cannot be too strongly recommended, and has been successfully acted on by the present Astronomer Royal in more than one recent construction. Two pendula, a copper and an iron one, on Mr. Baily's principle, were furnished by the Society for this expedition, an account of which may be found in the *Notices* of the Society for June 13, 1828.

The adjustment and trial of these pendula previous to the sailing of the expedition, were performed by Mr. Baily at his own house, and, thus engaged in actual experiment, he at once became led on into a minute examination of all the possible sources of practical error in the experiments, and consequent uncertainty in the important results of which they had become the basis. It was in this stage of his experience that he became acquainted with Professor Bessel's results, which determined him (as it had already done Captain Sabine) to go into the whole subject of the new correction by experiments performed *in vacuo*. But not content with assuming any fixed proportionality between it and the buoyancy correction, he resolved so to vary the form, magnitude, and materials of the vibrating masses, as to make its true nature and amount an object of inductive experimental inquiry; thus, though adopting the language of Buat and Bessel, disengaging himself in effect from any theoretical view of the *modus operandi* or mechanical process by which the effect was produced.

The result of these inquiries was a very elaborate and masterly paper read to the Royal Society, on the 31st of May, 1832, containing the results of experiments in air and in vacuo, on upwards of eighty pendulums of various forms and materials, by which the new correction is clearly shewn to depend not only on the dimensions but on the form and situation of the vibrating body. Independent of the excellence of this paper as a specimen of delicate experimental inquiry and induction, in which, to use the expression of one best capable of appreciating and admiring them, his generalizing powers seem to have been held in abeyance till the right moment for their exercise arrived, it had the further merit of bringing into distinct notice a number of minute circumstances, chiefly relative to the mode of suspension (important, however, from their influence on results), which it is absolutely necessary to attend to in these delicate and difficult inquiries, if the pendulum be ever again resorted to as a means of verifying or fixing anew the standard of length.

The return of the Chanticleer in 1831, without its lamented commander, threw the whole task of arranging and digesting for publication Captain Foster's pendulum observations on Mr.

Baily—a labour of love, prompted by the warmest friendship, and which he executed in the spirit of one determined to erect a monument to the fame of that truly amiable and talented officer, of the most durable and precious materials. His Report on the subject to the Admiralty was presented by the Lords Commissioners to the Council of the Astronomical Society, and printed at the expense of Government as the seventh volume of our *Transactions*. In this Report the observations are given in full, and with the most scrupulous fidelity, and those at each of the numerous stations discussed with the utmost care. The final re-examination of the pendulums in London was also personally executed by Mr. Baily, and the whole series of stations combined into a general result, which gives for the ellipticity of the earth $\frac{1}{289 \cdot 48}$. Not content with this, he has here also collected into one synoptic view the results obtained at various stations all over the globe with the invariable pendulum, by observers of all nations, so as to place them in comparison with each other, and to deduce from them a general result. Of these, by far the most numerous and prominent, in every respect, are those of our own countrymen, Captains Foster and Sabine; and nothing can be more gratifying, in estimating our own national share in this sublime application of science, than to find these principal authorities, whose observations were made and reduced with the most absolute independence of each other, agreeing at all the stations where they admit of comparison, with a precision truly admirable. In fact, the greatest disagreement of each of their final results, from a mean of them both, amounts to a quantity less than half a vibration out of 86,400, or in a mean solar day.*

Standard of Length.—From the pendulum to the standard of length, or the fixation of the *scientific unit*, the transition is easy, and, in Mr. Baily's case, was unavoidable. For, being once satisfied by experience of the innumerable minute circumstances on which perfect precision in these inquiries depends, and finding the parliamentary enunciation of the relation between the conventional and natural standards nullified, as it were, under his eye, he felt himself irresistibly urged to inquire how far the conventional unit itself might be depended upon, and within what limits of error it might certainly be reproduced in copies. His first step in this direction was to obtain the most perfect possible representative of this unit, and (as the Astronomical Society was now identified with almost all his undertakings) justly considering the possession of such a standard by that body as a thing in itself desirable, and the instrument itself likely, if thoroughly well executed, to become in its hands of universal scientific reference, he procured himself, to be named by the Council, a Committee for superintending its execution, and comparing it with the most authentic standards at present existing in this country. Perhaps there is no subject of

* The stations of comparison are London, Maranham, Ascension, and Trinidad. Taking London for a term of departure, each station affords a ratio whose extremes (see *Report*, p. 86) differ only by 0·0000103, the half of which multiplied by 86,400 gives 0''·44446.

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inquiry more perplexing, or one whose investigation calls for more patience and perseverance, than the detection and exact estimation of those minute sources of error which influence these delicate measurements, which can only be satisfactorily performed by endless repetition and systematic variation of every circumstance by which error can possibly be introduced. Another and peculiar source of annoyance, and even vexation, consisted in the rough and careless usage to which those precious instruments, on which the conservation of our national units depends, had been subjected in too many instances, by which rude and ignorant hands had irrecoverably marred some of those refined productions of human workmanship, which ought not even to be approached but with precaution, or touched but with the utmost delicacy. Few things seem to have excited Mr. Baily's indignation more than the continual occurrence of evidence, only too palpable, of the small respect in which these standards appear to have been held by those under whose protection they had been placed, and of the violence which has been repeatedly suffered to be perpetrated on them.

I shall by no means go into any minute analysis of the admirable "Report" to the Council of this Society, which contains his account of the construction of our standard scale, its comparison with the parliamentary standard, and its most authentic existing representatives—and with the French metre, as we have it represented in this country by two platina metres, in the possession of the Royal Society; or the means taken to secure it from loss, by the formation of carefully compared copies, two of which have been sent abroad, and two retained in England. Suffice it to say, that the delicacy of the means employed, the minuteness of the precautions used, and the multiplicity of the comparisons, surpassed every thing of the kind which had ever before been done in this country. This Report, too, is valuable in another way. Under the modest title of *A Short History of the Standard Measures of this Country*, it presents a summary of the subject so complete as almost to obviate the necessity of referring elsewhere for *historical* information.*

The immediate result of this useful and most laborious undertaking has been to put this Society in possession of, perhaps, the most perfect standard measure and divided scale in existence, in which every division, even to the individual inches, has been micrometrically verified, and their errors ascertained and placed on record. It would almost seem, too, as if a prophetic spirit had actuated the undertaking, and urged it to its completion without any of those delays which so often and proverbially attend the

* Mr. Baily was assisted in the actual comparisons by several Fellows of the Society, among whom the late Lieut. Murphy was conspicuous, an observer whose temper and scientific habits peculiarly fitted him for co-operating with Mr. Baily, and whose name would probably have occurred more than once in this memoir but for his untimely death, which took place in the service of Astronomy in a distant region, and was probably the unfortunate consequence of over-exertion in its cause.

construction and optical examination of delicate instruments. For the comparison of the new scale with the imperial standard yard had hardly been completed six months, when the latter, together with the other original standard by Bird (that of 1758), as well as the imperial standard of weight, were destroyed in the conflagration of the Houses of Parliament in October 1834. Thus the operation in question has been the fortunate means of preserving, to the latest posterity, that unit which has pervaded all our science, almost from the first dawn of exact knowledge.

The scientific unit is indeed preserved; but the nation remained, and remains up to this moment, without a legal standard either of weight or measure. In the early part of 1838, however, in consequence (as I have been led to understand) of some communications on the subject between Mr. Baily, Mr. Bethune, and the Astronomer Royal, the latter was induced to draw the attention of Government to the subject, an occasion having arisen which rendered the mention in an official form unavoidable. And on the 11th of May of the same year a commission was appointed, consisting of seven* members (Mr. Baily being one), to report on the course most advisable to be pursued under these circumstances. To this duty, which involved the hearing of a vast deal of evidence and much personal attendance, Mr. Baily gave his unceasing attention; suggesting many valuable points, both practical and theoretical; and, on the Report of the Commission being agreed on, and the practical formation of new standards, in conformity with the view therein taken of the subject, being referred by Government to the same commissioners, Mr. Baily undertook, to the general satisfaction of the whole body, and at their particular request, the delicate and important task of reconstructing the standard of length—a task which, unhappily, he did not live to complete. On whomsoever may devolve the completion of this standard, it will be satisfactory to the Members of this Society to know that, among the evidence adduced for its restoration, the scale prepared for it by Mr. Baily necessarily forms a most important and prominent feature.

Density of the Earth.—The accurate determination of one fundamental quantity naturally leads to inquiry into others. To make our globe the basis of measurement for the dimensions of the planetary system and of the visible universe, its form and magnitude must first be accurately known. To make it afford a scale by which the masses and attractive forces of the sun and planets can be expressed in terms conveying a positive meaning, its density must be ascertained, as compared with that of substances which occur on its surface, with which our experience is familiar, and from which our notions of material existence are drawn. The fine experiment of Cavendish, confirmed as it was, in its general result, by the operations on Schehallien, had satisfactorily demonstrated the continuity of the Newtonian law of gravity, from such vast distances as astronomy is conversant with, through the inter-

* An eighth was subsequently added.

mediate steps of the diameters of the earth, and of a mountain, down to those minute intervals which intervene between the parts of a philosophical apparatus, and their agreement within as moderate limits as could have reasonably been expected, had even led to something like a probable estimate of the earth's density, which, however, could never be regarded as satisfactory, otherwise than as a first step towards more precise determinations. Mr. Baily's labours, therefore, on the pendulum were hardly brought to a conclusion when he was led to enter upon this subject, the immediate occasion of his doing so being an incidental suggestion at the council table by Mr. De Morgan, of the desirableness of repeating the experiment of Cavendish*—a suggestion immediately seconded both by the Astronomer Royal and by Mr. Baily. The experience of the latter had shewn him how indispensably necessary, in such inquiries, are extensive repetition and variation of circumstance. The Schehallien experiment, from its very nature, admitted of neither; and, on carefully examining Cavendish's record of his own experiment, he found abundant reason to perceive how much was left to be desired, in both these respects, even in that form of the inquiry.

In resolving on a repetition of this experiment, the difficulty of the undertaking itself, and his own preparation for it, must have been, and no doubt were, very seriously considered. However confident in his own resources and perseverance, it was no holiday task in which he was now about to engage. The pendulum experiments, with all their delicacy, could hardly be regarded as more than an elementary initiation into the extreme minuteness necessary for this inquiry. There are two branches of research in physical astronomy which task to the utmost the resources of art, the delicacy of manipulation, and the perseverance of the inquirer—the parallax of the fixed stars and the density of the earth. In both, an immense object has to be grasped by the smallest conceivable handle. But, of the two problems, the latter is probably that which throws the greatest burden on the inquirer, inasmuch as it is not merely a series of observations to be carried on under well-ascertained circumstances and known laws, but a course of experiments to be entered on for eliminating or controlling influences which war against success in every part of the process, and where every element, nay, even the elementary powers of heat, electricity, magnetism, the molecular movements of the air, the varying elasticity of fibres, and a host of ill-understood disturbing causes, set themselves in opposing array in their most recondite

* *Fiat justitia, ruat cœlum.* The original design of this beautiful experiment was Mitchell's, who actually constructed the identical apparatus which Cavendish used, but died before he could execute the experiment. The apparatus came, after his death, into the possession of the Rev. W. H. Wollaston, D.D., *who gave it* to Cavendish, who used it, indeed, to excellent purpose, but who assuredly neither devised the experiment, nor invented, nor constructed, nor even, so far as I can perceive, materially improved the apparatus. All this is distinctly stated by Cavendish himself, who is, therefore, noway to blame for any misconception which may prevail on the subject.

and unexpected forms of interference. Nor could it have been overlooked by him that it was necessary, not merely to do over again what Cavendish had done before him, a thing in itself not easy, but to do it much more thoroughly and effectually.

Mr. Baily, however, was not to be discouraged by such considerations. He saw that there existed a blank in our list of exact data which it was necessary to fill, and he felt himself in possession of those gifts of nature and position which enabled him to fill it. Accordingly, in 1835, on the occasion above alluded to, the Astronomical Society appointed a committee to consider the subject; and Mr. Baily having offered to perform the experiment, in 1837, the Government (at the instance of Mr. Airy) granted the liberal sum of 500*l.* to defray the cost of the experiment.

This great work was brought to a satisfactory conclusion in 1842, and a complete account, with a full detail of the experiments, printed in one volume, published in 1843, forming the fourteenth of the series of *Transactions* of this Society. The experiments were varied with balls of different materials, and with suspensions no less various, combined so as to form no less than 62 distinct series, embodying the results of 2153 experiments; and which, formed into groups according to the nature of the combination, afford 36 distinct results, taking those only in which the balls were used, the extremes of which are 5·847 and 5·507, and the most probable mean 5·660, none of them being so low as Cavendish's mean result, 5·448. The probable error of the whole (0·0032) shews that the mean specific gravity of this our planet is, in all human probability, quite as well determined as that of an ordinary hand-specimen in a mineralogical cabinet,—a marvellous result, which should teach us to despair of nothing which lies within the compass of number, weight, and measure. I ought not to omit mentioning, that, of all the five determinations of this element we possess, Mr. Baily's is the highest.*

Though it would be equally remote from my present purpose, and superfluous in presence of such an assembly, to enter minutely into a discussion of these experiments, there is one point in their conduct which I cannot pass over in silence. The experiments had been carried on for eighteen months, a vast number of preliminary trials had been made, and upwards of 1000 registered results obtained, when it became apparent that the coincidence of Cavendish's results, one with another, was rather to be attributed to the paucity of his trials than to any especial accuracy in his observations or felicity in his mode of operating. Even in the few experiments made by Cavendish, discordances had shewn themselves, of

* The five determinations alluded to are, in order of magnitude, as follows:—

Scheballien experiment from Playfair's data and calculations	Max...4·867 / Min...4·559	Mean ..4·713
Carlini, from pendulum on Mont Cenis, corrected by Giulio		4·950
Reich, Repetition of Cavendish's expt. (most probable combination)		5·438
Cavendish, computation corrected by Baily		5·448
Baily (most probable combination)		5·660

which no account could be given other than by reference to the movements of included air; but, on Mr. Baily's extensive scale of operation, the limits of disagreement obviously arising from this cause became so enormous as to render it hardly possible to draw any line for the reception and rejection of results. In fact, at one period he had almost begun to despair of bringing the matter to any positive conclusion. The happy suggestion of Mr. Forbes, *to gild* the torsion-box and leaden balls, at once dispelled all this vagueness and uncertainty, and reduced the results to a high degree of uniformity.* Most experimenters would have been content to reject the discordant results. Mr. Baily unhesitatingly sacrificed the whole, and began anew, without appearing to regard with an instant's regret the time and labour lost. The gold medal of this Society was awarded to him for this important memoir.

Revision of Catalogues of the Stars.—The contributions of Mr. Baily to this branch of sidereal astronomy are so numerous and so important, as alone would suffice to rank him among the greatest benefactors to the science, since, without being himself an observer, he has conferred, by his indefatigable industry and perseverance in collating authorities, rescuing original observations from oblivion, and rectifying printed errors, a vast and unhoped-for accession of value to the works of all those on whom he has commented. In fact, this, which may be termed the archæology of practical astronomy, formed his staple and standing work, which, though from time to time interrupted by other subjects, was always resumed; always with increasing interest, and always on a larger and more effective scale, up to the very year of his death. His object appears to have been, so far as is now practicable, to destroy the gap which separates us from the elder astronomers, and to multiply, or at least to preserve from further destruction, the links which connect us with them; to ascertain *all* that *has really been* recorded of the stars, and to make that totality of knowledge the common property of astronomers—a precious and a pious labour, of which we have no examples, except in that spirit of loyal reverence which prompted Ptolemy to secure from oblivion the observations of Hipparchus, and make them the foundation of all future astronomy; and in that which animated Bessel, when on the basis of Bradley's observations he may be said to have afforded the means of reconstructing the whole fabric of the science.

The catalogues which Mr. Baily has re-edited are those of Ptolemy, Ulugh Beigh, Tycho Brahe, Halley, Hevelius, Flamsteed, Lacaille, and Mayer; a mass of commentation, expurgation, and minute inquiry before which the most stout-hearted might quail, since there is not one of them in which each individual star has not been made the subject of a most scrupulous and searching examination, and in which errors that had escaped all prior detection,—

* This was not, however, the *only* precaution used. Mr. Baily carried out the suggestion, by swathing the torsion-box in flannel, and applying over this defence an exterior *gilded* case. Should the experiment ever again be repeated, it should be attempted *in vacuo*.

errors of reading, errors of entry, of copying, of calculation, of printing, out of number,—have not been detected and corrected. But for these labours, the catalogues of Ptolemy and Ulugh, indeed, must have remained sealed books to any but professed antiquaries; and although we can now hardly ever have occasion to appeal to these earliest authorities for any practical purpose, we cannot but look on the labour thus cheerfully bestowed in embalming and consecrating their venerable relics as the sure pledge that our own works, if really worthy, will not be suffered to perish by time and neglect.

But while we admire both the diligence and the scrupulous exactness, of which the notes appended to these catalogues bear ample evidence, we must not omit to mention, that there are two of them, those of Mayer and Flamsteed, in respect of which Mr. Baily's researches have been pushed far beyond the mere duties of comparison and comment, having been extended to the conservation and minute examination of the original records from which the catalogues were formed. In the case of Mayer, his influence with the late Board of Longitude secured the publication of the original observations of that eminent astronomer at Göttingen, which had never before seen the light.* In the case of Flamsteed, his labours were much more extensive, and require a more particular statement, inasmuch as not only Flamsteed's greatest work, the *British Catalogue*, found in him its restorer to that high rank, as an astronomical document, which it is justly entitled to hold, but the fame and character of its author their defender and rescuer from grievous misapprehension and misstatement.

In 1832 it happened, by a most singular coincidence, that Mr. Baily became aware of the existence, in the possession of his opposite neighbour in the same street, E. Giles, Esq., of the whole of Flamsteed's autograph letters to Abraham Sharp, and was permitted to peruse and copy them. Their perusal convinced him that Flamsteed's life, astronomical labours, and personal character, had never been fairly placed before the world, and induced him to examine with care the mass of his papers preserved (or rather neglected and mouldering) at Greenwich. His first care was to arrest the progress of their further decay. His next, to avail himself of the original entries of the observations, and of the manuscript records of the computations founded on them, to trace out the sources, and to rectify the numerous errors and inconsistencies of the *British Catalogue* as it then stood before the world, and to present it to the public under quite a new aspect—as a noble monument of its author's skill and devotion, and a work worthy of the age and country which produced it. Among the papers thus examined, however, were also found an almost complete autobiography of Flamsteed, and a voluminous correspondence illustrative of those points so painfully at issue between

* In 1826.

Flamsteed, Newton, and Halley, relative to the publication of the Catalogue and observations, and to other matters of a more personal nature, which had hitherto all along been stated in an infinitely more unfavourable light towards Flamsteed than that which appears from Mr. Baily's thorough and voluminous exposition of the whole affair, and the evidence of the almost innumerable letters which he has printed at length, truly and properly to belong to them. Indeed it seems impossible not to admit, on the evidence here produced, that great and grievous injustice was done, and hardship imposed, in these transactions, on Flamsteed, whose character stands forward, on the whole showing, as that of a most devoted and painstaking astronomer, working at extreme disadvantage, under most penurious arrangements on the part of government, making every sacrifice, both personal and pecuniary, and embroiled (as I cannot help considering, by the misrepresentations and misconduct of Halley) with the greatest man of his own or any other age, holding a position with respect to the Observatory, as Visitor, which, under mistaken impressions of the true bearings of the case, might cause severity to assume the guise of public duty.

The volume which contains this important work of Mr. Baily was commenced (as we have seen) in 1832, and published in 1835, a rapidity of execution truly astonishing, when we consider that the volume extends to nearly 800 pages quarto; that the notes to the Catalogue alone occupy no less than 144 of them closely printed, not a line of which but involves some question of identity, of nomenclature, of arithmetical inquiry, or of reference to other authorities; that the examination and selection of the letters and other biographical matter for publication was a matter of the utmost delicacy and responsibility; and that the preface, which contains Mr. Baily's own summary of Flamsteed's life, the introduction to the Catalogue and the Supplement, in further vindication of Flamsteed's character and justification of his own views of it,—are all of them works of a very elaborate nature, and of the highest interest.

Catalogues of Lacaille and Lalande.—But Mr. Baily's views were not confined to the mere correction of existing catalogues. The labour of the commentator and collator, which has filled and satisfied so many minds, was to him only a means to an end of real practical importance. His aim was to render readily available to every astronomer all recorded observations of the sidereal heavens which could be depended on. Two great masses of observation might be said to exist buried under their own weight, and affording matter of grief and reproach to astronomy, now to be exchanged for congratulation and triumph. These were Lacaille's observations at the Cape of nearly 10,000 stars, and those of D'Agelet and Michel Lefrançais Lalande at Paris, of nearly 50,000. Neither of these collections of observations had been more than partially reduced. Lacaille himself had performed this task for 1942 of his stars. A considerable number of the stars of the *Histoire*

Céleste (Lalande's observations) had also been reduced and catalogued by Bode. But the great mass of both remained unreduced and unarranged, though it is true that Lacaille had accompanied each page of his observations with a table of reductions, and that in 1825, Professor Schumacher had published and dedicated to this Society a volume of assistant tables, enabling any one, with little trouble, to reduce any single observation of the *Histoire Céleste*. Still they remained unreduced, and, therefore, useless, except on those rare occasions when, for special reasons, it might be necessary to search out and reduce any particular object.

Thus was a treasure of great value held in abeyance. This Mr. Baily perceived, and after some correspondence with the French Bureau des Longitudes, which, however, led to no result, he resolved to bring the subject before the British Association. That liberal and energetic body at once acceded to his views, and in 1838 appointed two committees, each with funds at their disposal, to execute the reductions and prepare the catalogues. The reduction and arrangement of Lacaille's stars was executed under the superintendence of Mr. Henderson, that of Lalande's under Mr. Baily, the arrangement of the work in both (if I mistake not) having been effected on a plan concerted and matured by the latter. Both works were reported as complete (the prefaces alone excepted) in 1843, and it only remained to provide for their printing. This also was done by the liberality of the British government, who assigned 1000*l.* for the purpose; and this work was especially placed under Mr. Baily's direction. These catalogues, unhappily, he did not live to see published. The printing, however, of each was found advanced at his decease as far as 8320 stars,* and is now continuing under the more immediate inspection and superintendence of Mr. Stratford.

Catalogue of the British Association.—I have yet to speak of another and a magnificent work undertaken and brought to a successful conclusion by Mr. Baily; a work which, perhaps, deserves to be considered as the greatest boon which could have been conferred on practical astronomy in its present state, and whose influence will be felt in all its ramifications, giving to them a coherence and a unity which it could hardly gain from any other source. I allude to the general standard catalogue of nearly 10,000 stars, which the British Association are about to publish, at the instance of Mr. Baily. The plan of this great and useful work is an extension of that of the Astronomical Society, of which I have already spoken. The stars (selected by Mr. Baily) form a universal system of zero-points, comprehending probably every star of the sixth and higher magnitudes in the whole heavens. All the coefficients for their reduction are tabulated, and the greatest pains bestowed upon their exact identification and synonymes in other

* The total number of stars in the two catalogues respectively, will amount to 9766, and 47,400.

catalogues; so that this, in all human probability, will become the catalogue of universal reference. It is preceded by a valuable preface from the pen of Mr. Baily, his last contribution to astronomical science.

A very important feature of this and the two catalogues last noticed is their nomenclature. The system adopted is the same in all; and *that*, a system not capriciously adopted or servilely copied, but founded on a most searching and careful revision of all existing catalogues, and of the charts of Bayer, Flamsteed, and Lacaille, rectifying the boundaries of constellations which had become strangely confused, correcting innumerable errors of naming, numbering, and lettering, and reducing, in short, to order and regularity, a subject which had become almost hopelessly entangled. The way is thus at length opened to a more rational distribution of the heavens into constellations, and that final step which must sooner or later be taken, of introducing a systematic nomenclature into sidereal astronomy, rendered easy, whensoever astronomers shall be prepared on other grounds to take it. The trouble and difficulty attending this part of the work exceeds what any one unused to such tasks can easily imagine.

There are two papers by Mr. Baily relating to sidereal astronomy, of which mention ought to be made here; viz., one "On the Proper Motions of the Stars," which was read before the Astronomical Society on the 9th December, 1831, in which a list of about 200 stars, whose proper motion appears sufficiently sensible to merit further inquiry, is discussed. In drawing up this list, he was much aided by a series of transit observations by Dr. Robinson, observed expressly with a view to this inquiry. But as no positive conclusion of a general nature is arrived at in this memoir, and as the subject is yet hardly ripe for a complete discussion, I shall dilate no further on it. The other paper to which I allude (which was read also to this Society on the 14th November, 1834,) states the result of an examination of Dr. Halley's MSS. at the Royal Observatory. The appointment of Astronomer Royal was held by Halley twenty-two years, and though for the two first of them the observatory was entirely deprived of instruments, and for the next four a five-feet transit only was available, it might, at least, have been expected that he should have used diligently the means he did possess, or, at all events, have recorded the observations he did make in a regular, methodical, and intelligible manner. From Mr. Baily's examination of these papers, however, this appears to have been very far indeed from the case; and that, with the exception of differences of right ascension between the moon and planets and neighbouring fixed stars, which alone he seems to have considered worthy of attention, little of interest could be expected to repay the trouble and expense of their reduction. Of these papers Mr. Baily, ever anxious for the preservation of records, and mindful of the dormant value which they so often possess, obtained from the Admiralty a transcript, which, being carefully

collated with, and corrected by, the original MSS., is now deposited in our library.

The mention of the Royal Observatory induces me to notice here a change which has been lately made in the constitution of that noble institution, by a revision of the royal warrant, defining the number and mode of appointment of the Visitors, and placing this Society on a similar and equal footing with the Royal Society in the discharge of that important duty. This change was made at Mr. Baily's suggestion, with the entire concurrence, however, of the then President of the Royal Society, as to its expediency, on the occasion of the demise of the crown by the death of George IV., which rendered a new warrant necessary. The new system has been found to work admirably well, and to have secured a perfect harmony of feeling between the Visitors and the eminent individual who now fills the post of Astronomer Royal, as well as entire confidence in the recommendations and suggestions of that body on the part of government. Aware, as all are now, of the fatal and soporific influence of routine in public institutions, they have only henceforward to guard against the opposite extreme; to which end, they cannot do better than take for their guide and example that admirable combination of energy, gentleness, and judgment, which distinguished Mr. Baily, no less on every public occasion than in his conduct as a Visitor, in which capacity, under both the old and the new system of visitation, he was an invariable attendant, being never absent during a period of twenty-eight years from any meeting but the last.

About the end of June, 1841, an accident happened to him which had very nearly proved fatal. Crossing Wellington Street for the purpose of taking some MSS. to a printer, a deafness, which had for some years been increasing on him, rendered him unaware of a rider recklessly urging his horse to furious speed, who either did not see him or was unable to pull up. In consequence a collision took place, and Mr. Baily received a stunning fall, accompanied with a severe scalp-wound. So violent, indeed, was the shock, that he lay for a whole week senseless, and for an equal period after his life was considered in imminent danger. His sound and excellent constitution, however, carried him through it, and no ill consequences remained. By the end of September he was enabled to resume the observations of the Cavendish experiment, which this unfortunate occurrence had interrupted, and a few weeks' residence in the country completed the cure.

On the 8th of July, 1842, he was gratified by the observation of a phenomenon which it had from his youth upwards been one of his most ardent wishes to witness, viz. a total eclipse of the sun. To this he looked forward, indeed, with a curiosity peculiarly intense; having, on the occasion of the annular eclipse of May 15, 1836, which he travelled to Scotland to observe, and which he succeeded in observing under very favourable circumstances at Jedburgh, noticed a very singular phenomenon attending the formation of the annulus. I mean the appearance of beads of light,

alternating finally with long, straight, dark threads, cutting across the narrow line of the sun's limb, which he described in a highly interesting paper read to this Society on the 9th December, 1836. On the occasion of the total eclipse he selected Pavia for his station, that town lying in the path of the centre of the shadow. There, by especial good fortune, he obtained an excellent view of it, and there he witnessed, not only a repetition of the phenomenon of the beads, but that much more astonishing and previously unheard-of one, of the flame-like, or conical rose-coloured protuberances, seen to project, as it were, from the hidden disk of the sun beyond the border of the moon. This truly wonderful appearance (which was corroborated by several other observers at different places, among others by Mr. Airy, at Turin,) was described by him, on his return from Italy, in a paper read to this Society on the 11th Nov. 1842; and it is not a little singular that the two most remarkable solar eclipses on record should thus have furnished the subjects of his first and last astronomical memoirs,—

"Servatur ad imum
Qualis ab incepto processerit."

On his return from this journey he resumed his astronomical labours on the catalogues, as we have seen, which he continued, as well as his usual unremitted attendance to the business and at the meetings of this Society, till the spring of the present year, when his health began to decline, and several weeks of serious illness, a thing utterly unknown to him at any former period of his life (except as a result of accident), gave intimation of a failing constitution. For the first time since the reorganisation of the visitation of the Royal Observatory he was unable to attend the annual meeting of the Visitors in June. He, however, rallied somewhat, so as to be able to be present at the commemoration at Oxford on July 2, on which occasion the honorary degree of Doctor of Civil Law was conferred on him by that university, as well as on Mr. Airy and Professor Struve. On his return from Oxford his health again rapidly declined, and all efforts of medical skill proving unavailing to relieve an internal complaint, which had at length declared itself, he expired, after a protracted, but happily not painful illness, during which he was fully sensible of his approaching end, in a state of the utmost calmness and composure, at half-past nine o'clock in the evening of the 30th of August, at the age of seventy years and four months.

In passing in review, as I have attempted to do, the scientific works of Mr. Baily, and noticing, as we cannot help doing, the gradual expansion of his views, aad the progressively increasing importance of the objects they embraced, we are naturally led to ask by what means he was enabled thus to live as it were two distinct lives, each so active and successful, yet so apparently incompatible with each other? how, in what is generally regarded as the decline of life, he could not only accomplish so much with such apparent ease to himself, but go on continually opening out wider

and wider plans of useful exertion in a manner which seems only to belong to the freshness of youth? The answer to such an inquiry is, no doubt, partly to be found in his uninterrupted enjoyment of health, which was so perfect that he has been heard to declare himself a stranger to every form of bodily ailment, and even to those inequalities of state which render most men at some hours of the day or night less fit for business or thought than at others. But though this is in itself a blessing of the most precious kind, and, if properly used, a vantage ground of power and success to any one favoured enough to possess it, it must be regarded in his case as subordinate to, though, no doubt, intimately connected with, a gift of a much higher order,—that of an equable and perfectly balanced intellectual and moral nature,—that greatest of gifts, which has been regarded, and justly, as the only one really worthy to be asked of Heaven in this life,—*mens sana, in corpore sano.* Few men, indeed, have ever enjoyed a state of being so habitually serene and composed, accompanied with so much power, and disposition to exert it. A calm, the reverse of apathy, a moderation having nothing in common with indifference, a *method* diametrically opposed to routine, pervaded every part of his sentiments and conduct. And hence it arose that every step which he took was measured and consequent—one fairly secured before another was put in progress. Such is ever the march of real power to durable conquest. Hence, too, it arose that a clear natural judgment, and that very uncommon gift, a sound common sense viewing all things through a medium unclouded by passion or prejudice, gave to his decisions a certainty from which few were ever found to dissent, and to his recommendations a weight which few thought it *right* to resist.

It is very difficult in speaking of Mr. Baily's character to convey a true impression through the medium of a language so exaggerative as that which men now habitually use. Its impressiveness was more felt on reflection than on the instant, for it consisted in the absence of all that was obtrusive or imposing, without the possibility of that absence being miscontrued into a deficiency,—like a sphere whose form is perfect simply because nothing is protuberant. Equal to every occasion which arose, either in public or private life, yet, when not called forth, or when others occupied the field, content to be unremarked; to speak of his conduct as unassuming would convey but a faint idea of the perfect simplicity with which he stood aside from unnecessary prominence or interference.

Hardly less inadequate would it be to say of his temper that, always equable and cheerful, it was a source of peace and happiness to himself and others. It was much more,—it was a bond of kindness and union to all around him, and infused an alacrity of spirit into every affair in which the co-operation of others was needed, which was more than a simple reflex of his own good humour. It rendered every relation between himself and others easy and natural, and brought out all the latent warmth of every

disposition. One would have been ashamed to evade a duty or refuse a burden when it was seen how lightly his share was borne, how readily he stepped out of his way to offer aid wherever he saw it needed, and how frankly every suggestion was received, and every aid from others accepted and acknowledged. This is the secret of all successful co-operation.

Order, method, and regularity, are the essence of business, and these qualities pervaded all proceedings in which he took a part, and, indeed, all his habits of life. In consequence, all details found their right place and due provision for their execution, in every matter in which he engaged. This was not so much the result of acquired habits, as a man of business, as the natural consequence of his practical views, and an emanation of that clear, collected spirit, of which even his ordinary handwriting was no uncertain index. Among hundreds of his letters which I possess, there is hardly an erasure or correction to be found, but every where, on whatever subject, or whatever the haste, the same clear, finished, copperplate characters.

Of his choice of life I have already spoken something. Fortune he regarded as a mean to an end, but that end he placed very high; and fortune, he well knew, though a mean to its attainment, was not the only or the chief mean. As a member of civilized society, to add something to civilization, to ennoble his country and improve himself, by enlarging the boundaries of knowledge, and to provide for his own dignity and happiness by a pursuit capable of conferring both,—these were the ends which he proposed and accomplished. In choosing the particular line which he did, it is impossible too highly to appreciate the self-knowledge and judgment which enabled him to see and adopt those objects best adapted to his powers, and on which they could be, on the whole, most availably and usefully employed. Both in his public and private capacity he was liberal and generous in the extreme, and both his purse and his influence were ever ready, whether to befriend merit, or to promote objects of public and, especially, of scientific utility.

To term Mr. Baily a man of brilliant genius or great invention, would in effect be doing him wrong. His talents *were* great, but rather solid and sober than brilliant, and such as seized their subject rather with a tenacious grasp than with a sudden pounce. His mind, though, perhaps, not excursive, was yet always in progress, and by industry, activity, and using to advantage every ray of light as it broke in upon his path, he often accomplished what is denied to the desultory efforts of more imaginative men. Whatever he knew he knew thoroughly, and enlarged his frontier by continually stepping across the boundary and making good a new and well-marked line between the cultivation within and the wilderness without. But the frame of his mind, if not colossal, was manly in the largest sense. Far-sighted, clear-judging, and active; true, sterling, and equally unbiassed by partiality and by fear; upright, undeviating, and candid, ardently attached to truth, and

deeming no sacrifice too great for its attainment;—these are qualities which throw what is called genius, when unaccompanied, or but partially accompanied, with them, quite into the shade.

In speaking of his conduct with respect to this Society, and the infinite obligations we owe to him, we must regard him in the first place as the individual to whom, more than to any other, we owe the titles of a parent and a protector, and our early consolidation into a compact, united, and efficient body. As Secretary *pro tempore*, the draft of our Rules and the first Address explanatory of our objects, circulated at the commencement of our existence, were entirely, or in great measure, prepared by him; and, governed by these rules with hardly any change, we have continued to flourish for twenty-four years, which is the best test of their adaptation to our purposes. As I have already stated, he acted as Secretary during the first three years of our existence, during which period the business of our meetings and of our council was brought into that systematic and orderly train of which the benefit has never since ceased to be felt. On retiring from this office he was elected Vice-President, and on the next biennial demise of the chair he became our President, an office which he afterwards filled for three subsequent periods of two years, including that of his lamented death. Altogether, during eight years as President and eleven as Vice-President, he filled the highest offices of our institution, and was never off the Council, nor was there any Committee on which he did not sit as one of its most active and efficient members.

With the exception of the Meeting of May 12, 1836, when he was in Scotland observing the annular eclipse, he was never absent from any Council, or from any Ordinary, General, or Committee Meeting, until finally prevented by illness. Nor during the whole period of the Society's existence was there any matter in which its interests were concerned in which he was not a mover, and, indeed, the principal mover and operator. Nor was this care of our interests and respectability confined to formal business or to matters of internal management. On every external occasion which offered he bore those interests in mind. He watched and seized the precise opportunity to procure for us from Government the commodious apartments we occupy. He obtained for us the respected and dignified position of Joint-Visitors of the Royal Observatory. He let no opportunity pass of enriching our library with attested copies of the most valuable astronomical documents, such as "Flamsteed's Letters" and "Halley's Recorded Observations." He husbanded and nursed our finances with the utmost judgment and economy, thereby rendering us rich and independent. He printed at his own cost the thirteenth volume of our *Transactions*, and procured to be defrayed by Government the expense of the seventh, and, by subscription among the members, without entrenching on the funds of the Society, that of the computation and printing of our Catalogue. He prepared all our Annual Reports, and his addresses from the chair will always be read with pleasure

and instruction. He also prepared all Committees' Reports, and translated for reading at our meetings numerous notices and communications in the German language: among others the memoir relating to the Berlin charts. In fine, he superintended every thing in every department. But it was the manner and delicate tact of this superintendence which gave it its value and rendered it efficient. In respect of this point I may, perhaps, be permitted to use the expressions of a distinguished member of our body, to whom we owe many and great obligations, and who has witnessed the working of its machinery from the beginning, an advantage of which for some years I have myself been deprived by non-residence in London and absence from England. "Of his management of our Society," says Mr. Sheepshanks, "it is difficult to speak so as to convey a correct idea. No assumption, no interference with other people, no martinet spirit (which seems almost natural to all good business men), but every thing carried on smoothly and correctly, and without bustle. He hit, better than any chairman I have ever seen, the mean between strictness and laxity, and, while he kept every thing going in its proper channel, he also kept every body in good humour. This natural tact was a great gift, but there was another quality which I never saw in any one but him, and that was his readiness to give precedence and room to every one who wished to do any thing useful, and his equal readiness to supply every deficiency and do the work of every body else. He was also the person who never was asleep and never forgot any thing, and who contrived by his good humour, hospitality, and good sense, to keep every thing in train." To much of this view, as a matter of general character, I have given my own independent expression, but I could not deny myself the satisfaction of corroborating my own judgment by that of one so well qualified, from intimate knowledge, to form opinions.

Mr. Baily, as I have already stated, was a member of the Royal, Geological, and Linnean Societies, to which I may also add the Royal Irish Academy and the Society of Civil Engineers. In the Royal Society his eminence as an astronomer and a man of general science made his presence valuable, and the universal respect in which he was held gave him much influence. He filled in that body the office of Vice-President for six years, of Treasurer for three, and was fifteen times elected on the Council. I have already mentioned two of the three papers he contributed to its *Transactions*. The third contains a minute account of the standard barometer of that society, fixed up in their apartments in the year 1837, in which he enters into every particular of its construction, mode of registry, and corrections. It was read on the 16th of November, 1837. He was also one of the earliest members of the Royal Geographical Society, and took a very active part in its establishment. He was also a member and one of the trustees of the British Association, at whose meetings he was an occasional attendant, and acted, as we have seen, on some important committees. In 1835, the University of Dublin conferred on him

the honorary title of Doctor of Civil Law, as, I have already stated, was also done by Oxford in 1844. Among the foreign Academies, which in honouring him honoured themselves, I find him to have been a correspondent of the Royal Institute of Sciences of Paris, and of the Royal Academies of Berlin, Naples, and Palermo, as well as the American Academy of Arts and Sciences at Boston.

His portrait by Phillips, presented by some Fellows of the Society, has long adorned, and, though for the present removed from its frame, will speedily again adorn our meeting-room. May his mantle descend on our future presidents, and his spirit long continue to preside over our councils and animate our exertions in the cause he had so much at heart!

On the conclusion of the reading of the preceding Memoir, the thanks of the Society were proposed by the Dean of Ely, and unanimously voted to Sir John Herschel.

It was then moved by Mr. De Morgan, seconded by Mr. Donkin, and unanimously resolved,

> "That the Society feels it impossible to express in adequate terms its obligations to its late President; and it desires to impress on the minds of all the Fellows, that such imitation of his example as their occupations will allow is the mode of testifying their gratitude and respect for his memory with which he would have been most pleased."

LIST OF MR. FRANCIS BAILY'S PUBLICATIONS.

Chronologically Arranged.

1. Tables for the Purchasing and Renewing of Leases for Terms of Years certain and for Lives, with Rules for determining the Value of the Reversion of Estates after any such Leases, and for the Solution of other useful Problems, adapted to general use; to which is added an Appendix. London, 1802. 8vo.
   Second Edition, 1807.
   Third Edition, 1812.
2. The Rights of the Stock-Brokers defended against the attacks of the City of London. London, 1806. 8vo.
3. The Doctrine of Interest and Annuities analytically investigated and explained, together with several useful Tables connected with the Subject. London, 1808. 4to.

4. An Account of the several Life-Assurance Companies established in London, containing a View of their respective merits and advantages. London, 1810. 8vo.
Second Edition, 1811.

5. The Doctrine of Life-Annuities and Assurances analytically investigated and practically explained, together with several useful Tables connected with the subject. London, 1810. 8vo.
(This work has been lately translated and published in France under the following title: —
" Théorie des Annuités viagères et des Assurances sur la Vié, suivie d'une Collection de Tables relative à ces matières, par Francis Baily. Traduit de l'Anglais par Alfred de Courcy, et publié par la Compagnie d'Assurances générales sur la Vie. Paris, 1836.")

6. On the Solar Eclipse which is said to have been predicted by Thales. Read before the Royal Society, March 14, 1811. *Phil. Trans.* 1811.

7. A Synopsis of the Principal Elements of Astronomy, deduced from M. Laplace's *Exposition du Systême du Monde.* London, 1812. 8vo.

8. A New Chart of History. Large Sheet. London, 1812.
Corrected to 1817, with the Third Edition of the following work.

9. Description and Use of a New Chart of History, exhibiting the most material Revolutions that have taken place in the principal Empires, Kingdoms, and States, from the earliest authentic Records to the commencement of the present Year. London, 1812, 8vo.
Second Edition, 1813.
Third Edition, 1817.

10. An Appendix to the Doctrine of Life-Annuities and Assurances, containing a Paper read before the Royal Society, on a New Method of Calculating the Value of Life Annuities. London 1813. 8vo.
(By this Appendix the Doctrine of Life-Annuities, &c. was divided into 2 vols.)

11. An Epitome of Universal History, Ancient and Modern, from the earliest authentic Records to the commencement of the present Year. London, 1813. 2 vols. 8vo.

12. Report of the Sub-Committee of the Stock-Exchange relative to the late Fraud. London, 1814. 8vo.
Second Report of the Sub-Committee of the Stock Exchange relative to the late Fraud. London, 1815. 8vo.

13. Report of the Committee of the Stock-Exchange appointed for the Distribution of the Money stopped on Account of the late Fraud. London, 1815. 8vo.

14. Memoir relative to the Annular Eclipse of the Sun, which will happen on September 7, 1820. London, 1818. 8vo. with a map.

(Not published for sale.)

15. On the *Nautical Almanac*. *Phil. Mag.* for April, 1819. Vol. LIII. p. 217.

16. Memoir on a New and Certain Method of Ascertaining the Figure of the Earth by means of Occultations of the Fixed Stars. By A. Cagnoli, with Notes and an Appendix. London, 1819. 8vo.

(Not published for sale.)

17. Address Explanatory of the Views and Objects of the Astronomical Society. London, 1820. 8vo.
(Also nearly the whole of the Society's Annual Reports till the year 1844, inclusive.)

18. On a Method of Fixing a Transit Instrument exactly in the Meridian. Read June 9, 1820. *Mem. Ast. Soc.* Vol. I. p. 59.

19. On the Apparent Place of the Pole Star at the time of its upper culmination for the years 1820, 1821, and 1822. *Phil. Mag.* 1820. Vol. LV. p. 401.

20. Tables by the Board of Longitude. *Phil. Mag.* 1820. Vol. LVI. p. 288.

21. On the Solar Eclipse which took place on Sept. 20, 1820. Read Dec. 8, 1820. *Mem. Ast. Soc.* Vol. I. p. 135.

22. Astronomical Tables and Remarks for the Year 1822. With a Map. London, 1822. 8vo.

(Not published for sale.)

23. Remarks on the present defective state of the *Nautical Almanac*. London, 1822. 8vo.

24. On a New Method of determining the Latitude of a Place by Observations of the Pole Star. *Phil. Mag.* 1822. Vol. LIX. p. 445.

25. Astronomical Information. *Phil. Mag.* 1822. Vol. LX. p. 388.

26. On some New Tables of Aberration and Nutation. *Phil. Mag.* 1822. Vol. LX. p. 279.

27. On some New Tables for determining the Time by means of Altitudes taken near the Prime Vertical. Read January 10, 1823. *Mem. Ast. Soc.* Vol. I. p. 315.

28. Mr. Pond and M. Bessel. *Phil. Mag.* 1823. Vol. LXII. p. 389.

29. Supplementary Table for computing the Precession and Nutation of the Fixed Stars. *Phil. Mag.* 1823. Vol. LXI. p. 217.

30. On the New Tables of Aberration, Nutation, and Precession. *Phil. Mag.* 1823. Vol. LXI. p. 366.

31. Astronomical Information. Mr. Pond and M. Bessel. *Phil. Mag.* 1823. Vol. LXI. p. 469.

32. On M. Inghirami's List of Occultations of the Fixed Stars. *Phil. Mag.* 1823. Vol. LXII. p. 161.

33. Astronomical Information. *Phil. Mag.* 1823. Vol. LXII. pp. 391 and 466.

34. Mr. Pond and M. Bessel. *Phil. Mag.* 1823. Vol. LXII. pp. 390 and 467.

35. On the Mercurial Compensation-Pendulum. Read May 9 and June 13, 1823. *Mem. Ast. Soc.* Vol. I. pp. 381–420, with a Plate.

36. On the ensuing Opposition of Mars. *Phil. Mag.* 1824. Vol. LXIII. p. 50.

37. On the Circular Micrometer. *Phil. Mag.* 1824. Vol. LXIII. p. 167.

38. On Mr. Babbage's New Machine for Calculating and Printing Mathematical and Astronomical Tables. *Phil. Mag.* May 1824. Vol. LXIII. p. 335; and *Ast. Nach.* No. 46.

39. On the Occultation of the *Georgium Sidus*. *Phil. Mag.* 1824. Vol. LXIII. p. 458.

40. Astronomical Discovery (Bessel). *Phil. Mag.* 1824. Vol. LXIV. p. 67.

41. New Lunar Tables by M. Damoiseau. *Phil. Mag.* 1824. Vol. LXIV. p. 68.

42. On the Method of determining the Difference of Meridians by the Culmination of the Moon and Stars; with an Appendix and a List of Stars applicable to the purpose for the Year 1825, Read April 9 and May 14, 1824. *Mem. Ast. Soc.* Vol. II. p. 1.

43. A Statement of some circumstances connected with the mode of contracting the Columbian Loan in 1824. London, 1825. 8vo.

44. Astronomical Information. *Phil. Mag.* 1825. Vol. LXV. p. 466.

45. Errors in Piazzi's Catalogue of Stars. *Phil. Mag.* 1825. Vol. LXVI. p. 261.

46. Notice respecting the opposition of *Mars*. *Phil. Mag.* 1825. Vol. LXVI. p. 465.

47. An Address delivered at a Special General Meeting of the Astronomical Society of London, on April 14, 1826, on presenting the Gold Medals to J. F. W. Herschel, Esq., J. South, Esq., and Professor Struve, *Mem. Ast. Soc.* Vol. II. p. 541.

48. Astronomical Tables and Formulæ, together with a variety of Problems explanatory of their use and application. To which are prefixed the Elements of the Solar System. London, 1827. 8vo.

49. A List of Moon-culminating Stars for 1827. *Phil. Mag.* Vol. I. (second series), p. 47.

50. Astronomical Collections, No. 1, containing a Catalogue of Zodiacal Stars. London, March 1827. 8vo.
(Not published for sale.)

51. New Tables for facilitating the Computation of Precession, Aberration, and Nutation of 2881 principal Fixed Stars; together with a Catalogue of the same reduced to Jan. 1, 1830. To which is prefixed an Introduction explanatory of their construction and application. London, 1827.
Appendix to Vol. II. *Mem. Ast. Soc.*

52. Further list of Errors in Piazzi's Catalogue of Stars. *Phil. Mag.* 1827. Vol. I. p. 19.

53. List of Moon-culminating Stars for 1827. *Phil. Mag.* for 1827. Vol. I. (new series) p. 47.

54. On some new Auxiliary Tables for determining the Apparent Places of the Greenwich Stars. *Phil. Mag.* for 1827. Vol. I. p. 81.

55. On the Royal Observatory at Palermo. *Phil. Mag.* 1827. Vol. II. p. 81.

56. On the Right Ascension of γ *Cassiopeiæ*. *Phil. Mag.* 1828. Vol. III. p. 64.

57. New Astronomical Ephemeris. *Phil. Mag.* 1828. Vol. IV. p. 141.

58. On a new Micrometer, principally intended for the construction of a more complete Map of the Heavens. By M. Steinheil. *Phil. Mag.* 1828. Vol. IV. p. 173.

59. Further Remarks on the present defective state of the *Nautical Almanac*; to which is added an Account of the new Astronomical Ephemeris published at Berlin. London, Jan. 1829. 8vo.

(Extracted from the Appendix to "Astronomical Tables and Formulæ.")

60. A Letter to the Editor of "The Times," and inserted in that paper April 17, 1829.

61. On the Discordances in the Results of the Methods for Determining the Length of the Simple Pendulum. *Phil. Mag.* 1829. Vol. V. p. 97.

62. Appendix to Lieut. H. Foster's Paper on the Longitude of Port Bowen, by the method of Moon-culminating Stars. London, 1829. *Mem. Ast. Soc.* Vol. III. p. 43.

63. A Catalogue of the Positions (in 1690) of 564 Stars observed by Flamsteed, but not inserted in his British Catalogue; together with some remarks on Flamsteed's Observations. Read May 8, 1829. *Mem. Ast. Soc.* Vol. IV. pp. 129–164.

64. On the System of Prize Chronometers at Greenwich. *Phil. Mag.* 1829. Vol. VI. p. 424.

65. On Mr. Pond's recent Catalogue of the Places of 720 principal Stars, compared with the Places of the same Stars in the Catalogue of this Society; with Remarks on the Differences between the two Catalogues. Read March 12, 1830. *Mem. Ast. Soc.* Vol. IV. pp. 255–290.

66. Mayer's Catalogue of Stars, corrected and enlarged; together with a comparison of the Places of the greater part of them, with those given by Bradley, and a reference to every Observation of every Star. Read June 11, 1830. *Mem. Ast. Soc.* Vol. IV. pp. 391–445.

67. Report of the Committee of the Astronomical Society of London relative to the Improvement of the *Nautical Almanac.* Adopted by the Council, November 19, 1830: approved by the Right Honourable Lords Commissioners of the Admiralty, and ordered by them to be carried into effect. *Mem. Ast. Soc.* Vol. IV. p. 447.

68. On the New *Nautical Almanac.* *Phil. Mag.* 1831. Vol. IX. p. 23.

69. La Caille's Catalogue of 398 principal Stars, together with a comparison of the Places of such as are visible in this Latitude with those given by Bradley, and a reference to every Observation of every Star. Read April 8 and May 13, 1831. *Mem. Ast. Soc.* Vol. V. pp. 93–124.

70. On the Proper Motion of the Fixed Stars: with a List of those which are known, or supposed, to be materially affected by such a motion. Read Dec. 9, 1831. *Mem. Ast. Soc.* Vol. V. pp. 147–170.

71. On the Visitation of Greenwich Observatory, with a Copy of the New Warrant. *Phil. Mag.* 1831. Vol. IX. p. 27.

72. On the Computation of the Moon's Motion in Right Ascension. *Phil. Mag.* 1831. Vol. IX. p. 241.

73. On the Correction of a Pendulum for the Reduction to a Vacuum: together with Remarks on some Anomalies observed in Pendulum Experiments. Read May 31, 1831. *Phil. Trans.* 1832. pp. 399–492.

74. An Account of Experiments with an Invariable Pendulum, during a Russian Scientific Voyage by Captain Luetke. *Phil. Mag.* 1832. Vol. I. p. 420.

75. An Address delivered at the Annual General Meeting of the Royal Astronomical Society, on Feb 8, 1833, on presenting the Honorary Medal to Professor Airy. *Mem. Ast. Soc.* Vol. VI. pp. 247–256.

76. Report on the Pendulum Experiments made by the late Capt. Henry Foster, R.N., in his Scientific Voyage in the years 1828–1831, with a view to determine the Figure of the Earth.
    Printed at the public expense, and forming the seventh volume of the *Memoirs of the Royal Astronomical Society.* 1834. 4to.

77. Some Account of the Astronomical Observations made by Dr. Edmund Halley, at the Royal Observatory at Greenwich. Read Nov. 14, 1834. *Mem. Ast. Soc.* Vol. VIII. pp. 169–190.

78. An Address delivered at the Annual General Meeting of the Royal Astronomical Society, Feb. 13, 1835, on presenting the Honorary Medal to Lieutenant Johnson. *Mem. Ast. Soc.* Vol. VIII. p. 298.

79. An Account of the Rev. John Flamsteed, the first Astronomer Royal, compiled from his own Manuscripts and other authentic Documents, never before published. To which is added, his British Catalogue of Stars, corrected and enlarged. London, 1835. 4to.
    (Printed at the public expense.)

80. Report on the New Standard Scale of this Society. Presented December 11, 1835. *Mem. Ast. Soc.* Vol. IX. p. 35.

81. On a Remarkable Phenomenon that occurs in Total and Annular Eclipses of the Sun. Read Dec. 9, 1836. *Mem. Ast. Soc.* Vol. X. p. 1.

82. Supplement to the Account of the Rev. John Flamsteed. London, 1837. 4to.
    (Printed at his own expense for private circulation only.)

83. An Address to Astronomical Observers relative to the Improvement and Extension of the *Astronomical Society's Catalogue of* 2881 *Stars*. London, 1837. 4to.
(For private circulation only.)

84. On the Non-existence of the star 42 *Virginis*.
Monthly Notices of the Roy. Ast. Soc. June 9, 1837.

85. On the Repetition of the Cavendish Experiment.
Monthly Notices of the Roy. Ast. Soc. Dec. 8, 1837.

86. Description of a New Barometer, recently fixed up in the Apartments of the Royal Society; with Remarks on the mode hitherto pursued at various periods, and an Account of that which is now adopted for correcting the observed Height of the Mercury in the Society's Barometers. *Phil. Trans.* 1837. p. 431.

87. An Address delivered at the Annual General Meeting of the Royal Astronomical Society, Feb. 8, 1830, on presenting the Honorary Medal to the Hon. John Wrottesley. *Mem. Ast. Soc.* Vol. XI. p. 306.

88. Experiments with the Torsion-Rod for Determining the Mean Density of the Earth. *Mem. Ast. Soc.* Vol. XIV.
(Printed partly at the Government expense.)

89. The Catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, Hevelius, deduced from the best Authorities; with various Notes and Corrections, and a Preface to each Catalogue. To which is added the synonym of each Star in the Catalogues of Flamsteed or La Caille, as far as the same can be ascertained. Forming Vol. XIII. of *Mem. Ast. Soc.*
(Printed at his own expense.)

90. Some Remarks on the Total Eclipse of the Sun, on July 8, 1842. *Mem. Ast. Soc.* Vol. XV. p. 1.

LONDON:
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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *December* 13, 1844. No. 11.

The Rev. R. Sheepshanks, Vice-President, in the Chair.

Signor Gaetano Cacciatore, Director of the Observatory at Palermo, was balloted for, and duly elected an Associate of the Society.

John Russell Hind, Esq. observer to Mr. Bishop, at the Regent's Park Observatory, was balloted for, and duly elected a Fellow of the Society.

The following communications were read:—

I. Observations of the Moon and Moon-culminating Stars made at Port Essington, on the North Coast of Australia, in longitude $8^h\ 48^m\ 38^s$ east, and latitude $11^\circ\ 22'$ south. By Owen Stanley, Esq. Commander R.N.

The observations give the times of transit of the moon and moon-culminating stars, together with the rates of the clock, and they are corrected for errors of level and azimuth. They extend from June 20 to September 22, of the year 1839. No description of the transit instrument, with which they were made, is given.

II. Announcement of the discovery of Mauvais' Second Comet, in a Letter from M. Mauvais, dated July 9, 1844, addressed to Mr. Baily.

The comet was discovered on the night of Sunday, July 7, and was observed again on July 8. Information of its discovery was immediately forwarded to M. Schumacher. Its positions are given for the times of observation, and the daily motion is deduced from them. The place of the comparison-star is also given.

III. Circular Letter from Professor Encke, dated Berlin, 1844, July 10, announcing the independent discovery of the Comet, known as Mauvais' Second Comet, on the night of July 9, by M. D'arrest, a Student of the University at Berlin.

M. D'arrest gave information of his discovery of the comet to Professor Encke, on the night of July 9, and furnished a position of it, from a good series of observations; he gave also its daily motion deduced approximately from a motion of 50′ in right ascension, and 20′ in declination.

IV. Astronomical Observations made at Hudson Observatory, United States, in longitude $5^h\ 25^m\ 39^s{\cdot}5$ west, and latitude $41^\circ\ 14'\ 42''{\cdot}6$ north. By Elias Loomis, Esq. Communicated by Lieutenant-Colonel Sabine.

Hudson Observatory is furnished with:—1. An equatorial telescope of 66 inches focal length, and about 4 inches aperture, made by Simms in 1837. The circles are 12 inches in diameter, reading by verniers; the one to single seconds of time, the other to ten seconds of arc. 2. A transit circle having a telescope of 30 inches focal length, and nearly 3 inches aperture, made also by Simms. The circle is 18 inches in diameter, graduated to five minutes, with three reading microscopes, each measuring single seconds. 3. A clock with a mercurial pendulum, by Molineux.

A particular description of the building, with the instruments and the mode of using them, is contained in the *Transactions of the American Philosophical Society*, New Series, Vol. VII. pp. 43-47. The latitude of the Observatory has been deduced, from 63 culminations of *Polaris*, and 4 of *β Ursæ Minoris*, to be $41^\circ\ 14'\ 42''{\cdot}6$; and the longitude, from 72 corresponding moon-culminations at Greenwich, and others at Cambridge, Oxford, and Edinburgh (150 in all), to be $5^h\ 25^m\ 39^s{\cdot}5$. The observations from which these results are derived are contained in the *Transactions of the American Philosophical Society.*

The observations contained in this paper were principally made with a circular micrometer attached to the equatorial: they consist of—

1. Observations of Encke's Comet on nine days, from March 28 to April 11, 1842.

2. Observations of the Great Comet of 1843 on six days, from March 11 to April 6, 1843.

3. Observations of Mauvais' First Comet on twenty-five days, from July 30 to October 1, 1843.

4. Observations of Faye's Comet on three days, January 23, February 10 and 11, 1844.

All the observations are corrected for the effects of refraction, parallax, and aberration; and, in the case of Encke's comet, the errors of Professor Encke's *Ephemeris* are exhibited. In every case the differences of the right ascension of the comet and the stars of comparison are given, and the number of repetitions of the observations from which each result is deduced. Tables are added, containing the right ascensions and declinations of the stars of comparison.

V. Two Communications from the late Professor Henderson, containing Elements and an Ephemeris of Mauvais' Second Comet.

VI. Observations of Mauvais' Second Comet, made at Starfield, by W. Lassell, Esq.

The instrument used was the 9-feet equatorial Newtonian telescope of 9 inches aperture, and 112 inches focal length, having applied to it a parallel-wire micrometer, carrying a power of 96. When practicable, distances, both of right ascension and declination of the comet, were taken from the nearest star of sufficient magnitude to bear a slight illumination, and the approximate place of this star was obtained by comparison with the nearest star contained in the Astronomical Society's Catalogue.

In measuring these distances the author deemed it essential that the star of comparison should be so nearly of the same declination as the comet, that both should pass well through the field of the micrometer, while the telescope was clamped. He thus obtained the difference of declination in revolutions and parts of the micrometer, while the difference of the times of transit over the declination wires gave the difference of right ascension.

The following are the resulting places of the comet, subject to correction for parallax, and for whatever error may be found to exist in the determination of the places of the stars of comparison :—*

Day.	Sidereal Time.	Right Ascension.	North Declination.
1844.	h m s	h m s	° ′ ″
July 22	19 36 24·9	15 8 29·67	
	19 38 50·8	......	37 11 33·1
26	19 26 27·75	14 52 12·7	
	19 23 45·25	......	34 9 16·35
29	19 20 48·8	14 41 23·1	31 48 8·9
Aug. 3	19 13 35·1	14 25 51·08	
	19 15 54·5	......	27 53 22·3
10	19 26 46·2	14 8 17·2	
	19 28 55	......	22 30 36·7
14	19 8 11·7	13 59 5·75	
	19 4 41·5	......	19 33 45·4
27	19 27 45·8	13 39 51·9	10 45 2·8
29	19 9 56	13 37 8·94	9 28 55
31	19 32 37·15	13 35 1·3	8 17 30·66
Sept. 2	18 53 31·4	13 32 43·96	7 5 15·8
5	18 53 11·7	13 29 28·8	5 19 57 9
7	19 0 42·9	13 27 21·7	4 11 42

* The observations themselves are in the possession of the Society.—Sec.

On the last three days no stars were favourably situated for direct comparison; and the places of the comet for those days were obtained by applying the same instrumental corrections to its observed places, which were found requisite to be applied to the observed places of the nearest known stars; with further corrections also for difference of refraction.

Starfield Observatory, Dec. 7, 1844.

VII. Observations of Mauvais' Second Comet and De Vico's Comet, and of an Occultation of a fixed Star by the Moon, made by C. Rumker, Esq. at Hamburg. Communicated by Dr. Lee.

Results of the Observations of Mauvais' Second Comet.

Day.	Mean Time at Hamburg.	Apparent Right Ascension of Comet.	Apparent North Declination of Comet.	No. of Obs.
1844.	h m s	° ′ ″	° ′ ″	
July 12	11 33 20	239 58 32·4	43 52 11	3
16	10 34 15	234 27 38·5	41 28 18	8
17	10 49 14	233 8 50·8	40 48 15	21
20	12 49 45	229 22 17·8	38 39 27	3
22	10 53 52	227 11 30·1	37 14 33	12
23	10 34 2	226 7 55·2	36 30 15	14
24	10 34 6	225 5 22·5	35 44 37	13
25	10 18 40	224 5 20·3	34 59 7	2
Aug. 1	10 42 25	217 57 13·4	29 28 52	10
3	10 33 25	216 28 50·0	27 54 49	4
5	10 16 19	215 6 58·6	26 21 38	20
7	10 18 52	213 50 23·7	24 48 39	10
8	9 18 45	213 15 38·6	24 4 22	2
9	10 1 55	212 39 30·0	23 17 22	9
10	9 31 53	212 6 34·0	22 33 7	9
11	9 30 53	211 33 59·5	21 48 14	9
13	9 45 10	210 31 54·3	20 18 58	9
15	9 43 49	208 34 26·6	18 51 36	5
21	9 41 54	207 3 22·0	14 40 40	8
29	8 37 17	204 20 53·2	9 32 21	9
30	8 37 17	204 2 58·6	8 55 19	8
31	8 29 28	203 45 6·2	8 18 37	10

Results of the Observations of De Vico's Comet.

Day.	Mean Time at Hamburg.	Apparent Right Ascension of Comet.	Apparent South Declination of Comet.
1844.	h m s	° ′ ″	° ′ ″
Sept. 12	13 8 49	9 18 24	†14 23 47
	13 13 39	9 18 31	14 23 26
13	10 10 3	9 51 31	13 57 31
20	9 49 56	13 36 46	10 32 58
21	10 23 48	14 4 36	10 3 34
	12 52 37	*14 6 56	......
22	10 44 42	14 31 0	9 34 54
	12 50 26	14 33 7	† 9 32 22
24	11 44 1	15 20 39	8 38 10
	12 45 47	15 21 36	† 8 36 38
28	12 35 38	16 45 27	† 6 48 46
30	9 3 14	17 19 54	6 1 16

From the observations made at Berlin, on September 5 and September 8, and the observation at Hamburg of September 13, M. Funk, assistant to M. Rumker, has computed the following elements :—

Perihelion Passage, Sept. 2, $10^h\ 19^m\ 49^s$, Greenwich Mean Solar Time.

Longitude of perihelion	342° 56′ 30″
Longitude of ascending node............	62 8 44
Inclination	4 5 48
Logarithm of perihelion distance	0·1062216

Motion direct.

Observation of the Occultation of ι *Tauri* by the Moon on September 4, 1844.

	h m s
Hamburg Mean Solar Time of Immersion	13 39 8·5
,, ,, ,, Emersion	14 36 15·3

VIII. Observations of De Vico's Comet, made at Aylesbury by Thomas Dell, Esq. Communicated by Dr. Lee.

The following observations of the comet discovered by M. De Vico, were made at Aylesbury, with a 42-inch refractor mounted equatorially. The eye-piece, which gave a power of 28 to the instrument, was furnished with an annular micrometer, by means of which the places of the comet were obtained by comparison with the places of known fixed stars.

* Observation made with the transit instrument.
† Observations made with the meridian circle.

The stars with which the comet was compared were taken from the Astronomical Society's Catalogue, and computed for each day by the logs. A, B, C, D. The differences in right ascension and declination were computed by the formulæ given by Dr. Pearson in his *Practical Astronomy*, Vol. I. art. "Annular Micrometer."

The long interval which occurs between the observations of September 19 and October 5, was caused by the extreme brilliancy of the moon overpowering the feeble light of the comet during the greater part of that time.

Day.	Sidereal Time.	Apparent Right Ascen. of Comet.	Apparent Declination of Comet.	Star of Comparison.	No. of Obs.
1844.	h m s	h m s	° ′ ″		
Sept. 18	0 34 32	0 50 22·62	−11 35 3·30	ϕ^1 Ceti	3
19	0 31 21	0 52 19·49	11 5 57·15	ϕ^1 Ceti	3
Oct. 5	2 23 41	1 14 13·22	4 8 49·33	13 Ceti	1
7	22 9 7	1 16 6·47	3 22 44·83	34 Ceti	2
10	2 27 53	1 18 38·01	2 17 39·71	38 Ceti	4
11	0 18 35	1 19 26·27	1 55 50·87	38 Ceti	3
13	1 21 18	1 20 54·88	1 14 54·70	42 Ceti	1
14	1 42 12	1 21 35·37	− 0 55 12·14	42 Ceti	3
18	0 28 30	1 24 11·63	+ 0 21 18·06	26 Ceti	2

IX. Elliptical Elements of De Vico's Comet, with an Ephemeris. By J. R. Hind, Esq.

"These elliptical elements of the new comet discovered at Rome on August 22, have been computed from an observation made at Cambridge on September 15, one at Greenwich on October 3, and a third at Mr. Bishop's Observatory, in the Regent's Park, on October 22. The last position was obtained by comparison of the comet with a star in Professor Santini's Catalogue, employing a wire micrometer. The observations were corrected for aberration and parallax, and the resulting elements are as follows:—

Epoch, 1844, Sept. 15·55896, Mean Time at Greenwich.

Mean Anomaly = 2° 22′ 25″·53.

$$\left.\begin{array}{ll} \pi \ldots & 342^\circ\ 32'\ 40''\cdot 1 \\ \Omega \ldots & 63\ 52\ 24\cdot 1 \end{array}\right\} \text{Apparent equinox, October 0.}$$

$$i \ldots \quad 2\ 54\ 27\cdot 14$$

$$\phi = \sin^{-1} e \ldots \quad 37\ 59\ 59\cdot 65$$

Log. semi-axis major .... 0·4893706

Log. mean daily sidereal motion in seconds 2·8159507

Period of sidereal revolution .... 1980 days.

Motion direct.

"An ephemeris has been computed from the above elements,

which may serve for the speedy reduction of the observations during the next six weeks, and likewise assist in finding the comet.

Berlin Mean Noon	True Right Ascension of Comet.	True Declination of Comet.	Log. Dist. from Earth.	Time for Aberration.
1844.	h m s	° ′ ″		m s
Nov. 8	1 34 4·5	+ 5 47 49	9·66758	−3 51·17
10	1 35 15·5	6 13 13	9·68354	3 59·83
12	1 36 30·0	6 37 57	9·69946	4 8·78
14	1 37 47·9	7 2 3	9·71532	4 18·03
16	1 39 9·3	7 25 34	9·73110	4 27·58
18	1 40 34·4	7 48 33	9·74680	4 37·43
20	1 42 3·4	8 11 1	9·76240	4 47·58
22	1 43 36·1	8 33 1	9·77788	4 58·02
24	1 45 12·5	8 54 35	9·79323	5 8·74
26	1 46 52·6	9 15 45	9·80845	5 19·75
28	1 48 36·5	9 36 33	9·82354	5 31·05
30	1 50 23·9	9 56 59	9·83848	5 42·64
Dec. 2	1 52 15·0	10 17 6	9·85328	5 54·52
4	1 54 9·5	10 36 54	9·86793	6 6·68
6	1 56 7·5	10 56 25	9·88242	6 19·12
8	1 58 9·2	11 15 41	9·89675	6 31·84
10	2 0 14·3	11 34 42	9·91092	6 44·84
12	2 2 22·6	11 53 28	9·92493	6 58·11
14	2 4 34·4	12 12 1	9·93877	7 11·65
16	2 6 49·4	12 30 23	9·95243	7 25·44
18	2 9 7·6	12 48 32	9·96591	7 39·48
20	2 11 28·7	+13 6 28	9·97922	−7 53·78

"The following constants are adapted to this ellipse, and may be used for further calculation,

$$x = [0\cdot4813600] \sin (E + 76^\circ\ 3'\ 34'') - 1\cdot810161$$
$$y = [0\cdot3543699] \sin (E + 339\ 39\ 41) + 0\cdot483894$$
$$z = [0\cdot0292555] \sin (E + 331\ 32\ 24) + 0\cdot313834$$

where E is the comet's eccentric anomaly.

"It is to be remarked, that this comet is in some parts of the orbit liable to considerable perturbations from *Jupiter's* influence."

J. R. HIND.

Mr. Bishop's Observatory, Regent's Park, November 7, 1844.

X. Observations of De Vico's Comet, made at Ashurst by R. Snow, Esq.

The observations extend from September 24 to October 7. The

right ascensions and declinations of the comet are given as deduced from the instrumental readings without any correction. In some instances, when the comet was immediately compared with a star, the position of the star is similarly given. On October 2, the instrumental right ascension and declination of *θ Ceti* are also given; but, in general, the corrected positions of the comet cannot be determined till the stars of comparison have been observed on the meridian, and their positions furnished.

XI. Observations of Altitude and Azimuth of the Great Comet of 1843, made at St. Helena. By G. Brand, Esq.

The observations were made with an altitude and azimuth instrument by Gilbert, and they extend from March 6 to March 23. On March 6, the comet was compared with the moon only; on every other day, the altitudes and azimuths of some of the fundamental stars were observed. The length of the tail is stated to be, on March 6, 42° 55′; on March 7, 37° 23′; and, on March 17, 32° 47′. It is stated also, that on March 8, the colour of the tail had changed, and become more like the rays of the moon: on the 15th it appeared to be much brighter.

XII. Extract from the Translation of a Letter from Professor Bessel, dated Konigsberg, 10th of August, 1844. On the Variations of the Proper Motions of *Procyon* and *Sirius*. Communicated by Sir J. F. W. Herschel.

The subject which I wish to communicate to you, seems to me so important for the whole of practical astronomy, that I think it worthy of having your attention directed to it. I find, namely, that existing observations entitle us without hesitation to affirm that the proper motions, of *Procyon* in declination, and of *Sirius* in right ascension, are not constant; but, on the contrary, that they have, since the year 1755, been very sensibly altered. If this be so, the observations of the place of a star at two epochs are no longer sufficient to express its place for any indefinite time; but, for this purpose, it is necessary to investigate the law of the change. It follows also from this, that we are yet very far off from the correctness we imagined ourselves to have arrived at in the fundamental determinations of astronomy; and, that a new problem presents itself, whose solution will cost much labour and a long period of time, viz. the problem of determining the special motions of a star. For, even if a change of the motion can, up to the present time, be proved only in *two* cases, yet will all other cases be rendered thereby liable to *suspicion;* and it will be equally difficult, by observations, to free other proper motions from the suspicion of change, and to get such a knowledge of the change as to admit of its amount being calculated.

The earliest suspicion of the want of constancy of the proper motion was derived about the year 1834, from the corrections of the clock-time, which, at this observatory, were registered with every observed culmination of a fixed star and its reduction to the

meridian. At that time it began to be remarked in a striking manner, that negative clock-corrections derived from *Sirius* were greater, and positive corrections less, than those resulting from the other fundamental stars.

As the right ascensions of *Sirius*, which are given for the beginning of each year in the *Tabulæ Regiomontanæ*, are obtained by comparison of the right ascension in 1755 with that in 1825, their agreement with the observations of the latter years was complete; but, as early as the year 1835, fifty observations shewed, when compared with the three fundamental stars following *Sirius*, viz. *β* and *α Orionis* and *α Canis Minoris*, that $0^s \cdot 188$ must be added to the *Tabulæ Regiomontanæ*, to make the agreement again perfect. This disagreement has been since still increasing. In the year 1843 I found it, from fifty observations by Dr. Busch with the old instrument, $= + 0^s \cdot 318$, and, from forty, which I made myself with the new meridian circle of Repsold, I found it $= + 0^s \cdot 324$.

A second suspicion of the variability of the proper motions of the stars was awakened in me in the year 1840, by the declination of *Procyon*, since a new determination of all the elements of reduction of its declination, and of the declinations of the other fundamental stars, gave the observed declination of *Procyon* more northerly than that of the *Tabulæ Regiomontanæ*, (the tabular result being obtained by comparison of the observations of 1755 with those of 1820), by $1'' \cdot 64$.* This difference has also increased, since, by observations made with the instrument of Repsold, I find it for $1844 = + 3'' \cdot 18$.

What I have brought forward concerning *Sirius* and *Procyon* depends on determinations, whose certainty I esteem as great as can be attained by the present apparatus for observing. At the same time it does not cease to be necessary to subject the important result here given to the strictest scrutiny, by means of all the existing determinations of other observatories, before it can be received as the indisputable result of observation. I would communicate the result which this investigation has produced; but I should go beyond the limits of a letter, should I here give place to the criticism to which some of the numbers must be subjected, before they can be received as valid. Since this is, nevertheless, not the less necessary, I must refer to a paper which will very soon appear on this subject in the *Astronomische Nachrichten.* It is plain that we can obtain for the declination of *Procyon* comparative results from the different catalogues, only by eliminating the constant errors, which, without doubt, affect all the observations up to the present time, and frequently to the amount of several seconds. This I found to be the case by subtracting from the difference between every determination of *Procyon* and the *Tabulæ Regiomontanæ*, the mean of the differences of the results for eight stars, *α Ceti*, *α Orionis*, *β Virginis*, *α Serpentis*, *γ*, *α*, *β Aquilæ*, and *α Aquarii*, the mean of whose declinations is the same as that of *Procyon*, within a very

* *Astr. Nachr.* No. 422.

few minutes. By this means it will be gathered that the following collection of results does not depend upon the absolute declination of this star and the absolute right ascension of *Sirius*, but upon the relative declination and right ascension of each respectively, as founded on the comparison with the above-mentioned eight and three stars.

1. *Relative Declination of Procyon.*

		″	
Fundamenta Astron.	1755	0·00	
Maskelyne	1770	+1·54	For the most part but few observations of the stars; probably not altogether free from errors of observation.
Piazzi	1800	+1·99	New edition of the Catalogue. The results of separate years, which are supplied in the old edition (Supplement), and the determination from the later observations in Libro vi. à Spec. R. di Palermo, give a yet somewhat greater difference.
Bessel	1820	0·00	
Pond I.	1822	−0·03	Mr. Pond's own catalogue for this year.
Pond II.	1822	+0·16	M. Olufsen's computations, *Astr. Nachr.* No. 73.
Struve	1824	−0·15	
Argelander	1830	+0·03	
Airy	1830	+0·47	This is the mean of seven very beautifully accordant results derived from the observations from 1829 to 1835. It agrees tolerably well with the Catalogue of 1112 Stars; but deserves the preference, since the latter in the event of a variable proper motion cannot be correctly reduced to 1830.
Pond	1832	+0·84	
Henderson	1833	+0·89	
Konigsberg Obs.	1838	+1·59	*Astr. Nachr.* No. 422.
Ditto, with the Repsold circle	1844	+2·62	

2. *Relative Right Ascension of Sirius.*

		s	
Fundamenta Astron.	1755	0·000	
Maskelyne	1767	−0·079	Derived from a new reduction of the observations; this result differs by −0·288 from that contained in the Catalogue for 1770; the reason of which I do not know.
Piazzi	1800	+0·033	
Maskelyne	1806	+0·016	Derived from a new computation of the observations of 1803, and agreeing nearly with the Catalogue for 1805.
Bessel	1815	−0·036	
Pond	1819	−0·083	Derived from a new computation of the observations, but agreeing with Mr. Pond's own result.
Bessel	1825	0·000	
Struve	1825	−0·006	
Argelander	1828	−0·003	
Airy	1830	+0·049	This is the mean of seven results derived from the observations from 1829 to 1835; it is, for reasons mentioned above, preferred to the result derived from the Catalogue of 1112 Stars.
Pond	1832	+0·084	
Konigsberg Obs.	1835	+0·188	
Ditto, with both instruments	1843	+0·321	

These tables shew that the determinations for 1820 and 1825 are fully corroborated by means of nearly contemporaneous

observations at other observatories, as indeed the care which has been bestowed on all these observations would lead us to expect. They leave, besides, no doubt of the continual increase of the difference from the *Tabulæ Regiomontanæ*, from 1820 to the present time. This continual increase can be explained on the supposition of an unchanged proper motion only by attributing errors to the determinations for 1755, 1820, and 1825, respectively, of sufficient magnitude to make the *relative* declination of *Procyon* for 1755 to appear to be 7″ in error, and the *relative* right ascension of *Sirius* for 1755 more than a second of time. That so great errors cannot exist is proved by the different checks in the *Fundamenta Astronomiæ*; but Lacaille's and Tobias Mayer's cotemporary results leave no doubt on this point, although they cannot determine it within 2″ in declination and 1-4th of a second in right ascension. I regard also as a result rendered certain by the observations, that the supposition of an *unchanged* proper motion in the case of the relative declination of *Procyon*, and in the case of the relative proper motion of *Sirius* in right ascension, is shewn to be false.

The law of the change of each of the two motions is not yet known with sufficient exactness by the observations given. If Piazzi's determination of the relative declination of *Procyon* is correct (as I believe it to be),* then has the difference, between 1755 and 1820, reached a positive maximum. In the case of the right ascension of *Sirius*, I have sought to obtain a more approximate knowledge of its change, through the following up of the results of the observations with the transit instrument which Pond (vol. for 1811-12) published, and by a new reduction of Maskelyne's observations. This has given a positive maximum of the difference from the *Tabulæ Regiomontanæ* of about $0^{s}\cdot 3$, between 1790 and 1800; but since the pivots of the axis were unluckily proved to have been injured, and were corrected in 1803, which correction produced a significant effect on the subsequent observations of right ascension of *Sirius*, it cannot be *certainly* maintained that the maximum was produced in reality by the motion of *Sirius*, and not, at least in part, through the defect of the instrument. In the meanwhile it follows, from what has been advanced at present, and from the tables, that a period, not very different from that of a half century, would serve in both cases for a sufficient explanation of the observations. I think it, however, expedient in the present state of the subject, to wait for a further developement of the nature of the change, from the observations of the next half century, before pronouncing a judgment thereupon. This has, besides, no real value for the objects of astronomy before the nature of the motion of *all* the stars of the fundamental list are known.

I have investigated the conditions which must be fulfilled, that a sensible change of the proper motion, like that observed, may be capable of explanation by means of a force of gravitation. If the star that exhibits it be represented by S; an attracting mass by

* That of Maskelyne for 1770 has little weight.

m_n, and the corresponding star by S_n; the sun by O; the distance SS_n by r_n; OS_n by r_n'; OS by ϱ; the angle at the star S by s_n; the angle made by the plane OSS_n with the plane of motion of the star S by u_n, we easily find the expression of the second differential co-efficient of the apparent motion of the star, with respect to the time,

$$= \frac{m_n}{r_n r_n} \cdot \frac{1}{\varrho}\left(1 - \frac{r_n^3}{r_n'^3}\right) \sin s_n \cos u_n,$$

and the half of this is the expression for the real motion in a unit of time, answering to a uniform motion with the velocity at the commencement of that unit of time. Take now a century as the unit of time; the sun's mass as the unit of mass; the distance divided by $\sin 1'' = 206265$ times the mean distance of the earth from the sun as the unit of distance; then this expression becomes

$$= 0{\cdot}00000464 \cdot \frac{m_n}{r_n r_n} \cdot \frac{1}{\varrho}\left(1 - \frac{r_n^3}{r_n'^3}\right) \sin s_n \cos u_n.$$

Since the motion resulting from the foregoing observations is from ten to fifteen millions of times greater than this, it is necessary, for the explanation of it, by means of the attraction of *one* mass,

1. That either m_n be very great; or,
2. That r_n be small, that is, the attracting mass very near to the disturbed star; or,
3. That r_n' be very small, that is, the attracting mass very near the sun. The smallness of ϱ, the distance of the star in motion, from the sun, does not produce this effect, since it can be regarded as a factor of $1 - \frac{r_n}{r_n'}$. But, what *one* mass cannot effect,
4. The joint action of millions of existing stars might produce.

On the supposition that the hypothesis (1) is the true one, but is not connected with (2) or (3), the change of motion which the observations since 1755 have shewn, must have existed during a long space of time with a similar amount and direction; for the relative positions of O, S, S_n, change during this time, by the smallness of the existing motions of the sidereal system, so little, that it does not enter at all into the consideration. The change of motion must also increase proportionally to the square of the time, and much greater values are obtained than are consistent with the numbers of Hipparchus: I find, for example, that this increase of the present observed change of the motion of *Sirius*, would alter its right ascension, in 2000 years, by more than three degrees. Independently of this contradiction of the most ancient observations, there is also very little probability that we should be living precisely at that time when a great proper motion of a fixed star had become changed into a motion in the opposite direction, and again becoming great.

But it would be yet far less likely that this circumstance should take place in two cases independent of each other. One is justified, then, both by observations and probability, to fall back upon the explanation (1), with the exclusion of (2) and (3).

Against the explanation (4) the same objections sufficiently hold.

If (3) were the right supposition, a mass existing so near the sun would produce great irregularity in the motions of the planets, which we do not observe to be the case.

There remains then the explanation (2) alone. Stars, whose motions, since 1755, have shewn remarkable changes, must (if the change cannot be proved to be independent of gravitation) be parts of smaller systems. If we were to regard *Sirius* and *Procyon* as double stars, the change of their motions would not surprise us; we should acknowledge them as necessary, and have only to investigate their amount by observation. But light is no real property of mass. The existence of numberless visible stars can prove nothing against the existence of numberless invisible ones. There have been also stars which seemed to possess the peculiarity of a bright body passing over, and which have again lost it; for example, the star of *Tycho*. The phenomena then of the varying motions of the stars, which are so important for the results of plane astronomy, seem also to possess interest in relation to our knowledge of the physical constitution of the universe.

(Signed) F. W. Bessel.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *January* 10, 1845. No. 12.

The Rev. R. Sheepshanks, Vice-President, in the Chair.

Professor Haquin Selander, Astronomer Royal at Stockholm, and the Baron Fabian Jacob Wrede, Director of the Academy of Sciences at Stockholm, were balloted for, and duly elected Associates of the Society.

John George Cockburn Curtis, Esq. of the Hydrographic Office, Admiralty, and Arthur Kett Barclay, Esq. of Norbury, Croydon, were balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. On the Flexure of a uniform Bar supported by a number of equal Pressures applied at equi-distant points, and on the Positions proper for the Applications of these Pressures, in order to prevent any sensible Alteration of the Length of the Bar by small Flexure. By G. B. Airy, Esq. Astronomer Royal.

The author observes that this communication is, in reality, merely an auxiliary in a long series of troublesome researches, and ought, in strictness, to accompany the account of those researches, wherever it shall be ultimately placed. He thought, however, that it would be agreeable to the Society, both because it treats of mathematical formulæ and physical experiments which are familiar to many members of the Society, and also because its results bear in some degree upon the appreciation of one of our possessions, which is, perhaps, more creditable to us than any other, viz. the Standard Scale of the Society.

In the use of Standards of Length, two points have gradually attracted more and more of attention — the application of supports in such a manner as to produce no irregularities of flexure, and the application of such supports as will permit the standard freely to yield to the expansive or contractive effects of temperature. Since these points were insisted on by Captain Kater and Mr. Baily, the principle adopted in the support of standards has been to sustain

them upon two rollers in a definite position. In whatever way a bar be supported, flexure must be tolerated; and the only points aimed at are, that it shall produce no sensible effects on the measured length, or that it shall always be the same.

Several bars in use are notched at the ends to the centre of their thickness, and the defining points are in the axis of the bar; and in these cases there is good reason to believe that the effects of any flexure are nearly insensible. In the Standard Scale of this Society, the divisions are upon the upper surface, and the interval between these is not unaffected by flexure. In this case there may be some fear of the bar acquiring a permanent flexure, but this may be checked by an examination of the divisions which Mr. Baily caused to be cut upon the other sides of the cylinder. Still it is necessary to be assured that, as far as the best theories of flexure enable us to judge, the upper surface is not sensibly lengthened or shortened.

Very simple considerations will convince us that bars supported on two rollers, placed at the distance of one-fourth of the length from each end, must have the length of their upper surface necessarily elongated. The ill effects of this elongation are nearly obviated in the geodetic standards, which are notched to the centre; and may be considered as discoverable in the Society's scale by the use of the divisions on the opposite side. But it was the wish of the members of the Committee of Superintendence of the National Standards, not only to place the divisions in or near to the axis of the bar, by which means the *effect* of flexure would be obviated, but also to make the actual flexure as small as possible by a proper choice of the points of support. It was also recognised as a desirable principle that the bar should be subjected to as little strain as possible, and, therefore, that it should be supported at numerous points.

Great facility is given to the arrangements for supporting a bar with definite pressures applied at special points, by the use of levers. Thus, if any portion of the bar rest upon two rollers which are placed at the ends of a lever, and if the fulcrum of a lever (whether moveable or not) be in its centre, the pressures upwards produced by these rollers will necessary be equal. If there be another such lever, and if the fulcra of this and the former be upon the extremities of a third lever, and if its fulcrum be at its centre, then the pressures upwards produced by the four rollers will be equal. By this arrangement of rollers and levers one half of the bar may be supported. If another similar system be applied to support the other half of the bar, the pressures produced by its four rollers will also be equal among themselves; and if the bar be laid symmetrically upon them, all the individual pressures will be equal In this manner, by altering the lengths of the arms of the levers, any number of pressures, bearing any proportion to each other, and applied at any points whatever, may be produced. And the levers may be so arranged as to occupy a very small space.

Mr. Baily decided on the application of eight rollers for the

support of the national standard; and the author undertook to investigate the positions of the rollers which, supposing the pressures and the intervals equal, would so sustain the bar that its surface should not be sensibly lengthened. In the progress of the investigation he was struck with an analogy in the results for two, for four, and for eight rollers, which seemed to indicate that the process could easily be generalised; and, on trying it in the general form, it proved to be exactly what he expected. The author goes on to describe the nature of the investigation, which is as follows:—

It is first assumed, that the flexure is so small that the mere curvature of a neutral line will not produce a sensible alteration in its length; that the extension of a surface is proportional to the momentum of the bending force; and that, when the momenta are equal, the extension produced by a bending force downwards, and the contraction produced by a bending force upwards, are equal. The method of applying these assumptions is to estimate the curvature at any point, by conceiving the section of the bar at that point, and the whole of the bar on one side of that point, to be held perfectly firm; and then to use the algebraic sum of the momentum of the weight of the remaining part of the bar downwards, and of the momentum of the supporting pressures under that part of the bar upwards, as the representation of the molecular extension in the neighbourhood of that section.

From this statement it will easily be seen that the curvature of the bar is expressed by a discontinuous function. In shifting our ideal section gradually from one support to another, the momentum produced by the weight of the portion of the bar increases continuously; as also does the momentum of the pressures of the supports; and, therefore, through that portion of the bar the change of curvature is continuous. But on passing a new support, although the change in the momentum of the weight of the portion of the bar and the change in the momentum of the pressures of the former supports are both continuous, yet there is now suddenly introduced a new pressure; and this interrupts the continuity of the change. From this consideration it appears that, between two supports, the whole effect of molecular extension and contraction, as produced by curvature, is to be found by integration; but, that, when this integration is performed, the whole of the various parts can only be combined by summation.

These remarks are common to every method of supporting a bar. But when the supporting pressures are all equal, and their points of application are equally distant, the summation to which allusion is made can, for all the intervals between the supports, be effected by the processes of the calculus of differences. For the parts exterior to the terminal supports, these processes will not apply.

The author then proceeds with the mathematical investigation of the general expression for the extension of surface of the portion of the bar lying between any two of its contiguous supports.

If W be the weight of the bar, and b the length between the nth and the $n+1$th support, a the whole length of the bar, and m the whole number of supports (placed at equal distances), he proves that the extension of surface of that portion of the bar lying between the nth and $n+1$th support will be represented by

$$\frac{W\,b}{6\,a}\left\{3 \,.\, n \,.\, \overline{n-1} \,.\, b^2 + b^2 + 6 \,.\, \overline{n-\tfrac{1}{2}} \,.\, b\,c + 3\,c^2\right\}$$
$$- \frac{b^2\,W}{2\,m} \,.\, n \,.\, \overline{n-1} - \frac{b^2\,W}{2\,m} \,.\, n.$$

The sum of all the extensions is then taken from $n = 1$ to $n = m$, which gives the whole extension between the 1st and the mth, or last support.

If, also, c be the length of bar projecting beyond each of the terminal supports, and e the distance from each end of the bar at which the defining points are marked, then the additional extensions at both ends of the bar of the length $c - e$, will, by integration, be found to be represented by $\frac{c^3 - e^3}{6\,a}$ W. Adding this to the former expression, and remembering that

$$c = \frac{a}{2} - \overline{m-1} \,.\, \frac{b}{2},$$

the expression for the whole extension of the bar between the defining points reduces itself to the very simple form

$$\frac{W}{24\,a}\left\{a^3 - 8\,e^3 - (m^2 - 1)\;a\;b^2.\right\}$$

And if this be made $= 0$

$$b = \frac{a}{\sqrt{m^2 - 1}}\;\sqrt{1 - \frac{8\,e^3}{a^3}}.$$

II. An Account of some Ancient Astronomical Tables in the Library of the Reverend Charles Turnor, A.M., F.R.S., &c. By Mr. Richard Harris, Assistant-Secretary of the Society.

One of the volumes containing these tables is without date or title, but from its containing a calendar for the year 1349, there can be little doubt of that portion, at least, having been written in the early part of the fourteenth century. The other volume contains a calendar for the year 1347, and bears the title *Tabulæ Mediorum Motuum omnium Planetarum.* From an inscription on the fly-leaf it appears that it once belonged to the fraternity of Tolerants, or Grey Friars, at Babewell, near Bury St. Edmund's. The tables, which form the principal portion of the volume, were computed for the meridian of Oxford, and bear the general title "Almanak editum Oxoniæ:" they appear to have been chiefly formed from the famous Alphonsine Tables; the requisite correction being applied throughout for the difference of meridians, and some trifling alterations made in the method of arrangement to suit the

convenience of computers. In addition to solar, lunar, and planetary tables, this part of the volume contains a list of remarkable places, with their latitudes and longitudes.

Motives of convenience appear to have induced the writer or possessor of the *Oxford Almanac* to bind with it a calendar and astronomical ephemeris, in which the places of the sun, moon, and five planets, are given in regular order for every day of the year, together with the correction for their motions in hours, minutes, seconds, &c., besides the usual lists of saints' days, and other matters relative to the Church. This ephemeris was computed for 1347, but was intended for permanent use, tables being added which contain the necessary corrections for determining the places of the heavenly bodies on the corresponding days for several years.

This calendar is followed by tables of the retrogression of the five planets by Johannes de Lineriis, a writer of whom little is known, though he has been characterised by an old biographer as one of the four most celebrated astronomers that flourished between the times of Alphonso and Purbach.

The greater part of the other volume is occupied with a transcript of the Toledo Tables, usually attributed to Arzachel: among these are tables of the places of the sun, moon, and planets, and of the agreement of the Christian with the Arabic, Persian, and other eras. There is also a table of trepidation or libration, computed on the assumption that the point of intersection of the ecliptic and equinoctial moves in a small circle described with a radius of $4^\circ\ 18'\ 43''$; the limits of the libration in longitude being $10^\circ\ 45'$ east and west from the mean place, and its period about 4182 lunar years.

This part of the volume also contains a geographical table, tables of sines, of shadows or co-tangents, and of declinations, computed for an obliquity of $23^\circ\ 33'\ 30''$. A catalogue of thirty-five fixed stars is likewise given, which is curious and interesting from the circumstance of its being apparently derived from a copy of Ptolemy's great catalogue, which has either perished or escaped the notice of commentators.

This collection of tables is followed by a calendar for the year 1349, tables of planetary motions, lunar tables *cum canonibus*, and a few examples of the method of computing the places of the heavenly bodies from the data furnished in the earlier parts of the same volume.

In the recently published *Cycle of Celestial Objects*, Captain Smyth gives a short account of these MSS., and the high value he attached to them led the author of this paper to make a full examination of the tables, and the theories on which they are founded, the details of which were read to the meeting.

The following communication was not received in time to be read at the meeting, but it is thought desirable that the publication of it should not be delayed :—

III. Observations of the Second Comet of Mauvais, made at the Royal Observatory, Cape of Good Hope, under the direction of T. Maclear, Esq. Communicated by the Astronomer-Royal.

Day of Observation.	Star of Comparison.	Cape Mean Time for R.A.	Diff. R.A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Diff. of Decl. of Comet and Star.	No. of Obs.
1844.		h m s	m s		h m s	′ ″	
Oct. 27	* (1)	15 55 8·3	+3 9·56	2	15 55 8·3	−13 44·8	2
28	* (2)	15 51 11·5	−0 40·64	12	16 5 37·1	−18 19·9	3
29	* (3)	15 55 41·4	+0 34·28	18	15 48 5·2	− 8 50·1	5
					16 3 51·0	− 8 27·2	5
30	* (3)	16 3 53·4	−0 42·56	10	15 47 2·5	+28 39·7	8
					16 15 15·1	+29 26·0	4
31	* (4)	15 53 15·2	−2 20·31	7	15 50 28·6	−11 39·5	10
Nov. 1	A.S.C. 1457	15 26 30·4	+2 9·44	6	15 24 28·2	−23 22·6	5
	* (6)	16 5 36·7	−0 53·59	7	16 5 36·7	− 5 15·6	7
2	A.S.C. 1457	15 24 17·5	+0 47·77	5	15 10 35·3	+15 52·4	5
		15 54 26·5	+0 46·33	6	15 42 51·4	+16 43·9	5
	* (7)	16 13 33·5	−2 23·63	5	16 13 33·5	−17 19·0	5
3	* (8)	15 17 11·4	−4 6·96	5	15 17 11·4	+ 9 42·7	5
	* (9)	16 3 11·9	−0 29·21	16	16 3 11·9	−19 25·4	16
4	* (10)	15 31 37·0	+0 32·50	14	15 31 37·0	− 2 10·9	14
8	* (11)	15 45 27·1	−1 47·85	16	15 45 27·1	− 0 11·5	16
9	* (12)	15 3 12·8	+0 47·80	7	15 3 12·5	−28 4·9	7
10	* (12)	14 59 4·5	−0 56·22	10	14 59 4·5	+17 53·4	10
		15 45 49·6	−0 59·90	12	15 45 49·6	+19 23·8	12

The sign + denotes the comet's right ascension and declination to be greater than the star's; the sign — the contrary.

The above observations, with the exception of those on October 28 and 29, were made with a spider-line position micrometer applied to Dollond's achromatic telescope of 46 inches focal length; the power employed was 43. On October 28 and 29 a different micrometer was used, the comet not being visible in an illuminated field. This micrometer was made by removing the cross-wires from the micrometer eye-piece of the mural circle microscope and substituting three pieces of watch-spring, each about 12″ in width, permanently fixed parallel to each other, and at about 20ˢ apart; and another piece at right angles to the former, moveable by means of the micrometer screw. The differences in right ascension were determined from the transits over the three parallel bars; and the differences in declination measured by means of the moveable one: both objects being bisected by the edges of the bar.

The observations are not corrected for the effects of refraction and parallax.

The comet was first seen on October 27: it was then discovered without difficulty soon after it rose. It appeared as a faint nebulous patch of light, nearly circular, about 1′ in diameter, and with a condensation of light near its centre. On November 3 it was a fine nebulous mass of light; the outline seemed to be of a parabolic figure, with a condensation of light in its focus. A faint trace of a tail was seen, about 4′ in length: its direction being *apparently* towards the sun. On November 8, a distinct nucleus, about as bright as a star of the ninth magnitude, was visible in the brightest part of the head. The length of the tail was about 9′, its light being very faint.

The comet is now (Nov. 10) visible to the naked eye.

The following Table contains the approximate places of the stars of comparison, determined by means of the setting circles of the telescope :—

Star.	Mag.	Approximate Right Ascension.	Approximate Declination.	Remarks.
		h m s	° ′	
✱ (1)	9	12 34 50	−23 2	
✱ (2)	8	12 37 37	23 42	
✱ (3)	7	12 34 57	24 9	
✱ (4)	7	12 35 10	25 29	
✱ (6)	8	12 32 30	25 59	
✱ (7)	7	12 32 38	26 52	
✱ (8)	7	12 32 54	27 4	This is the same as No. 6728 of the Madras Catalogue.
✱ (9)	8	12 29 15	27 33	
✱ (10)	8·9	12 26 34	28 0	
✱ (11)	7	12 22 44	30 45	
✱ (12)	6	12 18 42	−31 56	This is the same as No. 6608 of the Madras Catalogue.

A star of the 8·9 magnitude follows ✱ (10) about 30^s, and is north of it about 8′; another star of the 9th magnitude follows ✱ (10) about 1^m 50^s, and is about 4′ to the south of it.

The observations were made by Mr. Mann.

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *February* 14, 1845. No. 13.

Report of the Council of the Society to the Twenty-fifth Annual General Meeting, held this day.

The Report of your Council has generally commenced with congratulation on the progress of astronomy during the past year; but there is a melancholy difference between this annual meeting and all which have preceded it: though astronomy has flourished, the most useful guardian of its interests in this country, and the best friend of our Society, has been removed by death. It will be long before those who conduct our affairs cease to associate the name of Francis Baily with even their smallest details; and every one of them, on the present occasion, remembers that our Annual Reports, up to the present time, were drawn up under his superintendence, and for the most part by his own pen.

As soon as the loss sustained on the 30th of August was known to the Council, a meeting was held (Sept. 20), at which it was determined to call a special general meeting at the conclusion of the ordinary meeting next after the event, not only to enable the Society to make its acknowledgment of Mr. Baily's services public, immediate, and expressive, but also to secure such a biographical record as the historians of astronomy would require at the hands of his surviving colleagues. The composition of a memoir was undertaken by Sir John Herschel, at the request of the Council; and of the manner in which this duty was performed those who had not the good fortune to be present on the 8th of November, when it was read, have ere this formed their opinion from its publication as No. 10 of the current volume of the *Monthly Notices.* The *inane munus* of the eulogist, gratifying as it might have been to the feelings of a friend, was not all that the occasion demanded: careful research, accurate description, well-considered praise, were due here, if ever they could be due any where. To have omitted diligent investigation of his real claims — to have given a negligent account of his great services — to have appropriated to him, by vague expressions of admiration, any part of what was due to another — would have been an insult to his memory; for it would have shewn that his example had been forgotten before his ashes were cold. Your Council, therefore, rejoice that they were able to

command the various qualifications requisite for this important task; and they feel that this meeting will join them in thanking Sir John Herschel, not only for those parts of his memoir which it must have been as pleasant for him to read as for you to hear, but for the elaborate contribution which he has made to the history of astronomy, and the care which he has taken to do justice to others, thus giving at once to the reputation of our lamented President that stability which the eulogist too often leaves to the historian.

In thus acknowledging the recent service of one of our fellows who, by readily undertaking both responsibility and labour, shewed himself a true disciple of the school in which Mr. Baily won his fame, your Council cannot refrain from reminding themselves and you, that, on the continued imitation of the illustrious example which survives the grave, the existence of this Society depends. In the history of a public body, the period is always critical which immediately follows the removal of a member whose zeal and industry were constantly employed in every department: but the first epoch of the kind must excite an anxious feeling; for it is then to be settled whether the association is permanent, or whether it is only the accident of a time. If your Council do not view the present crisis with alarm, it is because they know that the feeling just mentioned prevails throughout the Society, and because they have good reason to hope that, in this as in many other cases, a danger perceived is half avoided.

At the meeting of the Council above-mentioned Mr. Airy was elected President until the present annual meeting in the room of Mr. Baily; and Mr. Christie was elected a vice-president in the room of Mr. Airy.

The Report of the Auditors, which is subjoined, will shew the state of the finances of the Society:

RECEIPTS.

	£.	s.	d.
Balance of last year's account	128	8	0
1 year's dividend on £900 Consols	26	4	4
9 months' ditto on £2143 14s. 10d. New $3\frac{1}{2}$ per Cents	55	3	6
On account of arrears of contributions	111	6	0
66 annual contributions (1844–45)	138	12	0
4 ditto (1845–46)	8	8	0
1 ditto, in part (1846–47)	1	12	0
4 compositions	84	0	0
8 admission fees	16	16	0
5 first year's contributions	9	9	0
Sale of Memoirs	71	2	6
	£651	1	4

EXPENDITURE.

Purchase of £20 7s. 9d. New $3\frac{1}{2}$ per Cents	21	0	0
Magnay and Co. for paper	92	10	6
Harrison and Co. for stationery	12	8	0
J. Basire, for engraving Plates I.–IV. Vol. XV.	54	19	9
Carried forward	£180	18	3

EXPENDITURE (*continued*).

	£.	s.	d.
Brought forward	180	18	3
Moyes and Barclay, for printing Monthly Notices, &c. for Session 1844–45	36	4	0
Cubitt and Co. for repair of rooms on the basement story	43	6	0
C. Rivington, for law expenses	15	8	0
R. Harris, for 1 year's salary as assistant-secretary	83	6	8
R. Harris, for commission on collecting £357 5*s*. 6*d*.	17	17	3
Charges on books, and carriage of parcels	6	17	5
Postage of letters	15	14	8
Porter's and charwoman's work, &c.	10	18	8
Tea, sugar, cakes, &c. for the evening meetings	13	13	0
Coals, candles, &c.	12	18	6
Sundry disbursements by the Treasurer	6	7	0
Taxes: Income tax 1 9 2; Land tax 3 2 6; Window duty 5 4 9; Sewer's rate 0 12 9; Church and Rector's rate 1 14 0	12	3	2
Balance in the hands of the Treasurer (Jan. 22, 1845)	195	8	9
	£651	1	4

The assets and present property of the Society are as follow:

			£.	s.	d.
Balance in the hands of the Treasurer			195	8	9
3 contributions of 6 years' standing		£37 16 0			
3 —— of 5	ditto	31 10 0			
5 —— of 4	ditto	42 0 0			
2 —— of 3	ditto	12 12 0			
14 —— of 2	ditto	58 16 0			
24 —— of 1	ditto	50 8 0			
			233	2	0

£900 3 per Cent Consols.
£2143 14*s*. 10*d*. New 3½ per Cent Annuities.
1 Gold Medal unappropriated.
Unsold Memoirs of the Society.
Various astronomical instruments, books, prints, &c.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be best seen from the following abstract, continued from the Report of last year, viz.:

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1844	100	122	77	5	304	33	337
Since elected	3	5	...	...	8	4	12
Deceased	−2	−2	−1	...	−5	...	−5
Removals	+1	...	−1	...	...	...	...
February 1845	102	125	75	5	307	37	344

With respect to the instruments belonging to the Society, it may be proper to place on record the present state of them, viz.:

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* portable quadruple sextant,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Beaufoy* clock,
The *Herschelian* 7-feet reflector,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,

are in the apartments of the Society.

The brass quadrant, said to have been *Lacaille's*,

is in the apartments of the Royal Society.

The Standard Scale

is under the care of the Astronomer Royal, and, with the consent of the Council, is in actual use for the formation of a new standard measure, under the direction of the Standard Commission.

The remainder of the instruments are lent, during the pleasure of the Society, to the several parties undermentioned, viz.:

The *Fuller* theodolite, to the Lords of the Admiralty.
The other *Beaufoy* clock, } to the Royal Society.
The two invariable pendulums, }
The *Lee* circle, to Lord Wrottesley.
The *Wollaston* telescope, to Professor Schumacher.

In the last Annual Report, reference was made to the apartments on the basement story, which formed part of the original grant to the Society, but of which the Government still found it necessary to retain possession. Your Council have the pleasure of stating that these apartments were given up to the Society in the month of May last, and have since undergone complete repair. The advantage of such an accession of room is very great, particularly in that it enables the Society to receive, for safe custody, deposits for which it might be difficult to find a better guardian. Shortly after the apartments were obtained, the Society received (May 24) from the Government the cases containing standard copies of the United States' weights and measures, which had been sent from that country.

The fifteenth volume of the *Memoirs* of the Society is now in the press; and as twenty sheets are printed, it is confidently hoped that it will be ready for publication in a few months.

The gold medal has been this year awarded by your Council to Captain W. H. Smyth, R.N. for the zeal which dictated the construction of the Bedford Observatory, the persevering skill and industry with which his observations on double stars were conducted, and the final catalogue of 850 celestial objects which forms part of his recently published work, entitled *A Cycle of Celestial Objects.* The President will, as usual, at the close of

other proceedings, undertake the description of the grounds on which the Council justify their award and the presentation of the medal.

It will be seen that the portrait of Mr. Baily, by Phillips, which was presented to the Society some years ago, is not in its place. It is now in the possession of the Rev. R. Sheepshanks, who has obtained the loan of it from the Council, for the purpose of having an engraving taken from it at his own cost. We have often been indebted to Mr. Sheepshanks for services which entitle him to the gratitude of every astronomer; but the present obligation is one which this Society will feel to be peculiarly its own.

We have also to notice a present received in November last, from F. Archer, Esq. of a well-executed cast (by himself) from Chantrey's bust of our honorary member, Mrs. Somerville. And Mrs. Somerville herself has presented the Society with an equatoreal which belonged to Smeaton (described in No. 9 of the current volume of *Monthly Notices*), and which was left to her by Smeaton's daughter, Mrs. Dixon. An elaborate and expressive model of the lunar spot *Maurolicus*, and the parts adjacent, executed and presented to the Society by J. Nasmyth, Esq. has attracted much attention.

To her Majesty's Government we have to express our obligations for printing in our *Memoirs*, at the public expense, the late Professor Henderson's right ascensions of principal fixed stars, deduced from observations made at the Cape of Good Hope in 1832 and 1833. It has been a fixed rule with the Council not to print in the *Memoirs* the official transactions of public observatories; and they are happy to say that administrations of all parties, and also the Directors of the East India Company, have always recognised the reasonableness of this rule, the instant the grounds on which it was made were brought before them.

The Council have received from Mr. Baily's executors the manuscript details of the Cavendish experiment.

In the month of March last, Dr. Lee requested that the Society would accept the perpetual advowson of the Vicarage of Stone, near Hartwell. The grant is now enrolled according to the statute. This is the second gift of an advowson from Dr. Lee, to whom the Society is under many other obligations, and who has shewn an incessant interest in its welfare, not only by munificent contributions to its funds, its library, and its collection of instruments, but by active personal services extending over many years.

Among the losses by death, your Council have to regret Captain Basil Hall, Professor Henderson, and Mr. J. Frodsham.

Captain Basil Hall was the second son of the late Sir James Hall, Baronet, of Dunglass, President of the Royal Society of Edinburgh, who is so well known by the experiments which he instituted to corroborate the Huttonian Theory of the Earth.

Basil Hall was born on the last day of the year 1788; and, having very early manifested a predilection for sea life, he entered the Royal Navy in May, 1802, and served his noviciate on the

Halifax Station. On the 10th of June, 1808, he received his first commission as junior lieutenant of the Endymion, in which fine frigate he saw much service on the coast of Spain. In 1812 he joined the Illustrious, of seventy-four guns, bearing the flag of Vice-Admiral Sir Samuel Hood, the naval commander-in-chief in the East Indies; where he so exerted himself, that within two years he was promoted to the rank of commander, and appointed to the Victor, sloop-of-war. This ship was ordered home and paid off, shortly after the peace.

During all these services Basil Hall had manifested an active zeal for attaining a proficiency in nautical astronomy, and the use of its requisite instruments,—a zeal at that time the more meritorious, since it was necessarily pursued among the incessant duties consequent upon a state of actual warfare. To this useful pursuit he added a considerable ardour for geological researches, a taste, which was probably induced by his father's investigations. With such acquirements he was, therefore, strongly recommended to the Admiralty for employment; and he had not been long on half-pay before he was commissioned to the Lyra, a ten-gun brig, expressly fitted to accompany the Alceste frigate on the expedition which carried out Lord Amherst's embassy to China in 1816. His narrative of that memorable voyage, together with his improvements of our knowledge of the Yellow Sea and the coast of Corea, are well known, since his interesting description has passed through several large editions. This expedition was followed by his promotion to post rank in November, 1817.

Having returned from a tour of the continent of Europe, Captain Hall was appointed to the command of the Conway, of twenty-six guns; and on the 10th of August, 1820, he sailed from England for the South American Station. At this time our Society was in its infancy; but Captain Hall, who was always anxious to further the ends of science by making such observations as would be generally available, sent a communication to the Council, expressing his readiness to attend to any instructions on subjects wherein he might promote our views in the South Seas. The reply of the Council will be found embodied in the Report to the first Annual General Meeting, 9th of February, 1821. The Captain's proceedings were published on his return, with a scientific Appendix, by the late Commander Henry Foster, then a midshipman of the Conway.

After paying off this ship Captain Hall engaged himself in various scientific and literary pursuits; for he was of a temperament and inclination which would not admit of idleness. In 1825 he married Margaret, the youngest daughter of Sir John Hunter, Consul-General of Spain; and two years afterwards he made a tour of North America, with his family, the results of which are before the public. From the time of his return to the close of 1841 he was engaged in repeated and extensive journeys in France, Germany, Egypt, and other parts. At length he returned to England, labouring under a melancholy affliction, which, if not

induced, was probably exasperated by his constant mental exertion. After being thus lost to his friends and the world for nearly two lingering years, he expired on the 11th of last September, in the fifty-fifth year of his age. He was buried at Kingston, near Portsea.

Captain Hall was both an accomplished and a very useful officer. If he was sometimes hasty, his warmth of temper was neither ungenerous nor enduring; for he was open to conviction, and ready to make acknowledgment whenever inadvertently in error. His general literary works are well known and admired; but though our business is with those productions relating to science, we may at once pronounce that his talent of describing well what he felt with acuteness, and his accurate power of examining and embodying nautical traits, render his features of sea-life a distinctive picture of the epoch in which he lived and served. His style is natural and easy, and his force and individuality of character are admirable. Under every shadow which stern criticism may muster up, his works must be acknowledged to have done honour to the profession to which he belonged; while the well-directed application of his talents to the service of morality must ever be recognised. Still his most earnest aspirations were for science: "one of the calmest and most unmixed of human pleasures I am acquainted with," said he, "is practical astronomy;" and his reason was, that it inspired unceasing admiration. The writings, which more peculiarly claim our notice are:—

1. Various Hydrographical Remarks on the Indian and China Seas.
2. Memoir on the Navigation of the South American Coast.
3. On the Geology of the Table Mountain.
4. On the Care and Use of Chronometers at Sea.
5. Experiments made with an Invariable Pendulum in South America.
6. A Series of Observations on a Comet seen at Valparaiso in 1821.
7. A Sketch of the Professional and Scientific Objects which might be aimed at in a Voyage of Research.
8. A Method of laying down Ships' Tracks on Sea Charts.
9. A Discussion of the Trade Winds.

At none of its former anniversary meetings has this Society had cause to deplore heavier losses than those it has sustained in the past year. The death of its president, Mr. Baily, was followed, at a short interval, by that of Professor Henderson, of Edinburgh; an astronomer of first-rate merit, and one who, for many years, has been conspicuously distinguished among us by the frequency and importance of his contributions to our publications. His services to the cause for which we are associated have been, indeed, of no ordinary kind; and, although prematurely terminated, have entitled him to a high place among the most deserving of our members. It becomes, therefore, a duty we owe to his memory to recapitulate

in this Report his principal claims to our gratitude, and to place on record a few particulars of his personal history; as well to testify our respect for his eminent merits as to gratify, however imperfectly, the desire which will be felt to know something of the life and character of one whose labours will henceforth form part of the annals of astronomy.

Thomas Henderson was born at Dundee on the 28th of December, 1798. His father was a tradesman in respectable circumstances, who died early in life, leaving to the care of his widow a family consisting of two sons and three daughters. The eldest son, John, was bred to the legal profession; and, after practising for some time as a writer in Dundee, went to Edinburgh, where he studied for the bar, and was rapidly rising to distinction as an advocate, when he died suddenly, at the age of thirty-eight, of aneurism of the heart. Thomas, the other son, and the youngest of the family, was destined also for the profession of the law, and had the advantage of receiving the best education which could be obtained in a town which has long been distinguished in Scotland for the excellence of its public schools. After the usual preliminary instruction, he was sent, at the age of nine, to the grammar-school, where he pursued the usual course of classical study during four years, and was distinguished by his diligence and quickness of apprehension, being generally the dux of his class. In 1811, he proceeded to the Academy, where he continued two years longer. The Dundee Academy was at that time under the very able rectorship of Mr. Duncan, now Professor of Mathematics in the University of St. Andrew's. The course of instruction included elementary mathematics, natural philosophy, and chemistry; and young Henderson passed through the complete course with the same distinction which had marked his progress at the grammar-school. Professor Duncan, in a letter to the writer of this notice, bears the following testimony to the merits of his former pupil. "The two Hendersons," he says, "were the best scholars I had in the whole period of my incumbency. You are aware, I suppose, that John became distinguished at the bar, and was only prevented, by an early death, from rising to great eminence. Thomas, the future astronomer, was remarkable for every thing that was good, — the diligence and success with which he prepared his lessons, the exactness with which he performed the exercises, the propriety and modesty of his demeanour."

At the age of fifteen he was placed in the office of Mr. Small, a writer (or solicitor) in Dundee, with whom his brother had entered into partnership. In this situation he remained six years; and it appears to have been during this period that he began to devote his leisure hours to the study of astronomy, though the particular circumstance, or accident (if, indeed, there were any other cause than the promptings of a naturally active and inquisitive mind), which first gave this direction to his inquiries is not known. During his attendance at the Academy, and even at an earlier date, he had evinced a remarkable predilection for works relating

to geography and chronology; and his taste for these studies was often gratified at the expense of his health, for he was naturally of a weakly constitution, and subject to some disorder of the eyes, which at times rendered him nearly blind. From these subjects to astronomy the transition is easy and natural; and, although he received no aid from the lessons of a master, and had no encouragement from example, yet, in a town which could boast of being the birth-place of Ivory, and in which Dr. Small, the expositor of Kepler, had so long resided, it may be supposed there would prevail, among the better informed classes of the inhabitants, some general sentiment of respect for proficiency in astronomy and mathematics, which might not be without its influence on a mind possessing a peculiar aptitude for such studies. However this may be, the fact deserves to be recorded, as an instance of what may be done under circumstances apparently the most adverse, that it was while employed as an attorney's clerk in a provincial town, that he laid the foundations of that extensive acquaintance with astronomy for which he became afterwards so distinguished.

Having gone through a six years' apprenticeship, Mr. Henderson, at the age of twenty-one, repaired to Edinburgh for the purpose of completing his legal education and obtaining professional employment. He first obtained a situation in the law office of a Writer to the Signet, where his intelligence and abilities were remarked by Mr. (now Sir James) Gibson Craig, who became his steady patron and friend, and by whose recommendation he was appointed secretary, or Advocate's clerk, to the celebrated John Clerk, afterwards one of the judges of the Supreme Court of Scotland, under the title of Lord Eldin. On Lord Eldin's retirement from the bench, he was for some time private secretary to the Earl of Lauderdale; an office which he relinquished for the more profitable appointment of secretary to the Lord Advocate (Jeffrey). In these successive employments, he passed the twelve years from 1819 to 1831; and it may be remarked that, although of a subordinate nature, they were such as would only be intrusted to a person of acknowledged abilities, and one whose character for fidelity, discretion, and general intelligence, was already established. It is probable that they allowed him considerable intervals of leisure, but in no other respect were they favourable for scientific pursuits; and, indeed, it may be regarded as one of the most remarkable features in his history, that, while engaged in the discharge of multifarious and active duties, in a line of life so foreign to astronomy and physical science of any kind, he should not only retain his tastes for the liberal studies he had commenced at the Dundee Academy, but find leisure to make so many new and important acquisitions.

Soon after he became resident in Edinburgh his astronomical acquirements procured him introductions to Professors Leslie and Wallace, Captain Basil Hall, and other distinguished persons, by whom his talents were quickly appreciated, and who afforded him every encouragement to persevere in his scientific pursuits. At that time the small observatory on the Calton Hill, belonging to

the Astronomical Institution of Edinburgh, was placed under the charge of Professor Wallace, who, finding in Mr. Henderson a person to whose hands the instruments could be safely intrusted, allowed him free access to them, and thereby gave him an opportunity of acquiring a practical knowledge of a subject which he had already become familiar with from study and books. The instruments, it is true, were not of first-rate excellence,—a clock and transit of 30 inches focal length, with an altitude and azimuth instrument by Troughton, formed the equipment of the observatory,—but to a young astronomer, who had no other access to astronomical apparatus, such an opportunity was invaluable; and there is little doubt that this circumstance had considerable influence on his future history.

The opportunity of making observations, however, did not in any degree withdraw him from the less inviting parts of the science. From the outset of his career he perceived that, in order to be an astronomer, something more is necessary than mere expertness in the use of instruments. He accordingly continued to direct his main attention to the reduction of observations; and, at an early period, acquired a great knowledge of methods, and great facility in calculating eclipses, occultations, cometary orbits, and, generally, in all the computations and reductions which are subservient to practical astronomy.

Mr. Henderson first brought himself into notice as an astronomer in 1824, by communicating in that year to Dr. Young, then Secretary to the Board of Longitude, a method of computing an observed occultation of a fixed star by the moon, of which that accomplished philosopher thought so highly, that he caused it to be published, under the title of an improvement on his own method, in the *Nautical Almanac* for 1827 and the four following years; accompanied in some of the last of those years by a second method also proposed by Mr. Henderson. These methods were also published in the *London Quarterly Journal of Science;* and he received for them the thanks of the Board of Longitude. About the same time, or shortly after, he began to contribute to the *Quarterly Journal of Science* various useful papers and notices; among which may be mentioned, in particular, elements for computing the eclipses of the sun, and lunar occultations of the planets and satellites for the years 1826, 1827, and 1828; and lists of the principal lunar occultations for the years 1826, 1827, 1828, and 1829. These lists, it is presumed, were the cause of several valuable observations of the phenomena being made, which, but for them, would probably have been neglected.

In 1827 he communicated a paper to the Royal Society of London, "On the Difference of Meridians of the Royal Observatories of London and Paris," which is published in the *Philosophical Transactions* for that year, and which furnishes a remarkable instance of the value of that habit of scrutinising calculation for which he was particularly distinguished. In the copy of the observations officially furnished from the Royal Observatory to

Sir John Herschel, with a view to his operations in 1825 for determining the difference of longitude between Greenwich and Paris by means of fire signals, there was an error of a second in one of the numbers, which had the effect of causing some irregularity in the results of the different days' work; but as the discrepancies were small, they had been ascribed to errors of observation. Mr. Henderson, remarking the irregularity, was led to recalculate the original data, and thereby detected the error; and not content with this, he submitted the entire process to a new calculation. His result differed immaterially from that which had been previously obtained; but the correction of the error, by rendering the single results more consistent, gave a greatly increased confidence to the general conclusion; and, as was said of it by Sir John Herschel himself, "had the effect of raising a result liable to much doubt, from the discordance of the individual days' observations, to the rank of a standard scientific *datum*, and thus conferring on a national operation all the importance it ought to possess."

Mr. Henderson's connexion with this Society began by his undertaking, upon the request of the Council, to compute an ephemeris of the occultations of *Aldebaran* by the moon, in the year 1829, for ten different observatories in Europe. In this undertaking he was associated with Mr. Maclear, and the ephemeris, purporting to be their joint production, was read at the December meeting in 1828, and published in No. 15 of the *Monthly Notices*. His first contribution to the *Memoirs* (published in Vol. IV.) contained observations of transits of the moon, and stars nearly in the same parallel of declination, over the meridian, made at the Calton Hill Observatory in 1828, from which and corresponding observations made at Greenwich he computed the difference of meridians. This paper deserves notice, as shewing that he had already adopted the practice of estimating and allowing for the weights of the results and determining their probable errors, according to the methods in use among the German astronomers, but of which the examples were not, as yet, frequent in this country. The method of determining differences of longitude by means of observations of moon-culminating stars, then recently proposed by Nicolai, had been strongly recommended by Mr. Baily in a paper published in Vol. II. of the *Memoirs*, where all the requisite rules and formulæ were given for the computation. Mr. Henderson entered into these views with his accustomed energy, and not only embraced every opportunity of putting the method in practice by computing corresponding observations, but was at much pains to promote such observations by preparing lists of moon-culminating stars for the use of observers. A list of this kind was prepared by him at the request of the Council for the use of the Arctic expedition under the command of Captain (Sir John) Ross in 1830.

Another subject on which his talents for computation were frequently exercised in furtherance of the views of the Council, was the calculation of the lunar occultations of fixed stars and planets. Observations of these phenomena, interesting both to

practical and physical astronomy, were at that time much encouraged by the Society on account of their use in determining longitudes; and, as such phenomena will seldom be observed unless they have been predicted, it was desirable to ascertain the times of their occurrence by a previous calculation. In this case also Mr. Henderson rendered most efficient aid, by contributing, for several years, monthly lists of the principal lunar occultations computed for the meridian of Greenwich. The promptitude and accuracy with which these calculations were made induced the Council to request him to communicate his methods to the Society; and he accordingly drew up a set of "Practical Rules for the Approximate Prediction of Occultations," which are published in Vol. IV. of the *Memoirs*. These services were duly acknowledged in our *Annual Reports;* and at the anniversary meeting in 1830, the thanks of the Society were voted to him "for the very valuable assistance he had rendered to the cause of astronomy in his various computations presented to the Society."

The amount of Mr. Henderson's contributions to astronomy from 1825 to 1830, consisting of observations, tables, remarks, methods, and calculations of various kinds, published in the *Quarterly Journal of Science*, the *Nautical Almanac*, and the *Notices* and *Memoirs* of this Society, would have done credit to a professed computer; but, in order rightly to appreciate his zeal, it must be remembered that he was all this while occupied with professional duties of a kind which would be found by most persons to be sufficiently engrossing. His disinterestedness was no less remarkable than his scientific ardour; for, though in the receipt of very moderate emoluments, he declined all remuneration for his calculations; nor would this feature of his character be fully appreciated unless it were told at the same time that a considerable part of his income was appropriated by him to the support of his mother and sisters.

Mr. Henderson's official duties, while connected with the Earl of Lauderdale and the Lord Advocate, brought him for some months in each year to London, upon which occasions he became personally acquainted with the principal astronomers of the metropolis, and had an opportunity, particularly at the observatory of Sir James South, which was freely thrown open to him, of seeing and handling instruments of the first class. His various useful contributions to astronomy had already acquired for him a considerable reputation; and the high opinion which had been formed of his talents was increased by observation of the worth and unaffected simplicity of his character, and the range and extraordinary accuracy of his information on all astronomical subjects. Accordingly, a prospect of attaching him to an office by which his services would be secured for the exclusive benefit of astronomy, afforded satisfaction to all those who took an active interest in the progress of our science.

The death of Dr. Robert Blair, in December 1828, having caused a vacancy in the professorship of Practical Astronomy in

the University of Edinburgh, Mr. Henderson's qualifications for that office were represented to the Government (the patrons of the appointment) by Dr. Thomas Young, and energetically urged by some other astronomers, particularly by Captain Basil Hall. From the correspondence which passed with the Secretary of State, it appears that the Government upon that occasion postponed the filling up of the vacancy in order that opportunity might be given to consider upon what footing the office, which had hitherto been a sinecure, could be placed with the greatest prospect of advantage to science. In the summer of 1829, another astronomical appointment became vacant by the lamented death of Dr. Young. This very eminent person had received valuable assistance from Mr. Henderson in the computations required for the *Nautical Almanac*, and had formed the most favourable opinion of his talents. About a fortnight before his death he placed a memorandum in the hands of the late Professor Rigaud of Oxford, to be made use of in case of his decease; and when the anticipated event took place, the memorandum was found to contain a request that it might be stated to the Board of Admiralty that he knew no person more competent to be his successor in the superintendence of the *Almanac* than Mr. Henderson. Professor Rigaud lost no time in making known in the proper quarter the recommendation of his deceased friend, which he also supported with the weight of his own influence; but, in consequence of other contemplated arrangements it was unsuccessful, and the superintendence of the *Nautical Almanac* was, upon that occasion, committed to Mr. Pond, then Astronomer Royal. Mr. Pond was also well aware of the efficiency and value of Mr. Henderson's aid, and immediately preferred a request to him to continue the same computations which he had been accustomed to supply to Dr. Young; making offer at the same time of remuneration, and sufficient employment to occupy a great portion of his time. But, although Mr. Henderson, shortly after, supplied Mr. Pond with some calculations on which he had previously been engaged, this offer appears to have been declined, and for two years longer he continued to follow his professional occupation.

His character as an astronomer, however, was now fully established; and accordingly, on the death of Mr. Fallows, in 1831, he was regarded as one of the persons best qualified to undertake the direction and management of the observatory established by Government, and then recently completed, at the Cape of Good Hope. Through the intervention of Captain Beaufort his qualifications were a second time brought under the notice of the Lords Commissioners of the Admiralty, and on this occasion successfully; but the idea of leaving his country was distasteful to him; and he accepted the office with some reluctance, and only in deference to the advice of his friends. The warrant of his appointment is dated in October 1831, and a few months after he embarked for the colony.

Mr. Henderson arrived at the Cape in April 1832, and forthwith commenced his observations. The principal instruments were

a 10-foot transit by Dollond, and a 6-foot mural circle by Jones; and his only assistant was Lieutenant Meadows, who had been sent out in the previous year. Few examples are upon record of more zealous and successful exertion than that which is furnished by him during his residence of thirteen months at the Cape. The results of his own personal exertions during that short interval comprehend the determination of the latitude and longitude of his station; the positions of stars near the South Pole for determining the polar positions of his instruments; the amount of refraction near the horizon; observations of the moon and stars for determining the moon's horizontal parallax; of *Mars* for determining the parallax of that planet, and thence that of the sun; of eclipses of *Jupiter's* satellites; occultations of fixed stars by the moon; a transit of *Mercury;* places of Encke's and Biela's comets; and, finally, between 5000 and 6000 observations of declination. So large a mass of work would have been sufficiently creditable under the most advantageous circumstances; but when we call to mind that he had to contend with all the difficulties incidental to a new and imperfectly organised establishment, where no assistance was to be obtained from artists, and with a notoriously unmanageable instrument, it will be easily admitted that it would be difficult to overrate the zeal, perseverance, and skill, with which he laboured to discharge the duties which had been intrusted to him. But the merit of accumulating so large and valuable a mass of observations, great as it must be allowed to be, is completely thrown into the shade by comparison with that which is due to the persevering industry with which he laboured in their reduction, and in deducing from them the results we shall presently have occasion to mention.

When Mr. Henderson accepted the appointment to the Cape Observatory, it was no doubt anticipated (probably even by himself) that his residence there would be of some considerable duration. These anticipations were not realised, for in May 1833 he resigned the office, and shortly after returned to Europe. The reasons which induced him to take this step were fully stated by him in his letter of resignation addressed to the Secretary of the Admiralty. After briefly alluding to the exertions necessary to carry their lordships' instructions into effect, he went on to say, that, not only did the state of his health render him unable much longer to support the requisite labour, but that the observatory, considered as a place of residence, laboured under so many disadvantages, and required a mode of life so different from that to which he had been accustomed, that he found it impracticable to remain longer; and being thus unable to perform the duties, he felt it to be incumbent on him instantly to resign the situation. But with that regard for the interests of science, and that kindness of disposition so eminently characteristic of him, he added, that on his return home he would proceed, with the sanction of their lordships, to the task of reducing the observations and of extracting from them the results they were intended to afford; expressed his sense of the efficient aid he had received from his assistant, Lieutenant Meadows; modestly prof-

fered such assistance as his experience might enable him to give to his successor; and concluded by recommending the state of the observatory to their lordships' consideration.

Had his state of health permitted him to continue his observations satisfactorily, there can be little doubt that the inconveniences of which he complained would in due time have been removed, or at least have become supportable; but, he had been made aware before he left England, that his physicians already apprehended the germs of that disease which eventually proved fatal to him, and under the depressing influence of this knowledge, aggravated by separation from all his friends, and his family to whom he was tenderly attached — and by his complete isolation — his spirits gave way, and he became apprehensive that he would be unable to maintain the observatory, from which so much was expected, in a state of the requisite efficiency. Impressed with this idea, he took the resolution to give in his resignation, in order that it might have the contingent benefit of another appointment; and thus an act which, hastily judged, might appear to be an abandonment of his post, was the simple and natural result of a high, it may be an exaggerated, sense of public duty. The sacrifice involved in his resignation was of no small importance to him in a pecuniary point of view; for all he had to fall back upon was a pension of 100*l.* a-year to which he had become entitled upon the retirement of Lord Eldin. But no one was ever less influenced by considerations of personal or pecuniary advantage, and under any circumstances he would have disdained the emoluments of office without the most punctilious discharge of its duties.

On his return to this country in 1833, Mr. Henderson took up his abode in Edinburgh, and being now without official engagements, he began the task of reducing the rich store of observations he had brought with him from the Cape. The first result of this self-imposed labour was the determination of an important astronomical element — the sun's parallax — from a comparison of observations of the declinations of *Mars* near opposition, made at Greenwich, Cambridge, and Altona, with the corresponding observations at the Cape. Previous to his departure for that station he had expressed a wish that a selection should be made of such stars as could conveniently be observed with *Mars* at the opposition in November 1832, with a view to the determination of the parallax, and that a list of them should be circulated among different astronomers in various parts of the world, for the purpose of obtaining corresponding observations. Accordingly, Mr. Sheepshanks having furnished the apparent places of *Mars* during the requisite period, and Mr. Baily having selected the stars to be observed, the council caused the list to be printed and circulated with directions as to the mode in which the observations should be made. In consequence of these preparations four sets of corresponding observations were obtained, from each of which Mr. Henderson deduced a value of the parallax. The mean of the whole gave a parallax of $9''\cdot028$; a result which is known from the more certain method of the transits

of *Venus* to be somewhat too large, as was the case also in Lacaille's attempt at the Cape, in 1751, to determine the solar parallax by the same method. The determination, he remarks, is chiefly valuable as shewing the probable accuracy of the method, and the limits within which this important datum in physical astronomy may be determined independently of the rare phenomena of the transits of *Venus*.

Another paper of a more elaborate kind followed soon after, containing an investigation of the anomalies of the 6-foot mural circle in the Cape Observatory. When this instrument was first set up, there were found to be considerable discrepancies in the reading of the different microscopes, a circumstance which occasioned great perplexity to Mr. Fallows; and, although that astronomer ascertained that the mean of the six readings might be depended upon, he did not succeed in arriving at any satisfactory conclusion respecting the cause and the laws of the irregularities. Aware of these anomalies, which, indeed, were confirmed by his first observations, Mr. Henderson, a few weeks after his arrival at the Cape, undertook a laborious examination of the state of the instrument by means of a series of readings of each of the six microscopes at every tenth degree of the limb; and in April and May, 1833, shortly before his departure from the Cape, he repeated the experiment on a more extended scale, by examining the division corresponding to every fifth degree of the circle, and also the divisions immediately before and after it. On the results of this last experiment, which, however, were found to be identical with those of the former one, he grounded the investigation which fonns the subject of the paper;—an investigation which exhibits in a very advantageous point of view his sagacity, patience, and laborious accuracy, and is an admirable model of a carefully conducted experimental inquiry. The result was, that, in order to explain the observed anomalies it was necessary to suppose the figure of the instrument to be an oval of small eccentricity, that the pivots of the axis were not exactly circular, and that the whole instrument frequently changed its position upon the pier from the defective bearing of one of the pivots. But the most important conclusion deduced from the investigation,—as it involved no less important a question than the trustworthiness of the *whole* of his Cape observations,—was that the mean of the readings of the six equidistant microscopes was affected only to a very small extent (if affected at all) by these imperfections, and that the probable error of the instrument is not greater than the errors of the best instruments of similar construction hitherto made. From a similar investigation, founded on a less complete examination of the instrument by Mr. Fallows, Mr. Sheepshanks and Mr. Airy, in a paper which is printed in Vol. V. of the *Memoirs*, had previously arrived at a similar conclusion.*

* It may not be without interest to state that, when this instrument was sent back to England some years afterwards, and examined at the Royal Observatory, it was found that, owing probably to some oversight in the construction, the

While thus busily employed with the reduction of his own observations, on the results of which, he was well aware, his reputation as an astronomer would essentially depend, Mr. Henderson's assistance was still, as it had been in the early part of his career, freely extended to others whenever an opportunity occurred of promoting the cause of astronomy. Thus, at the request of Mr. Baily, he undertook the reduction of Captain Foster's observations of the comet of 1830, made at Ascension Island, the results of which, together with an ephemeris for facilitating the calculation of observations of the comet made in the southern hemisphere, are published in Vol. VIII. of the *Memoirs*. Fortunately, however (even though thus turned to account), the interval of leisure was of no long duration; and he was soon called upon for the discharge of more active duties. In 1834 an agreement was concluded between the government and the members of the Astronomical Institution of Edinburgh, whereby the latter gave up to the University the use of their observatory on the Calton Hill, which the former undertook to convert into a public establishment, by furnishing it with suitable instruments, and making provision for an observer and assistant. It was then resolved to fill up the office of Professor of Practical Astronomy, which had remained vacant since 1828, and to combine with it the direction and superintendence of the observatory; and the Secretary of State did this Society the honour to request that a deputation from the Council would confer and advise with him respecting the person whom it might be proper to appoint to the situation. In consequence of this request, a deputation waited upon Lord Melbourne, and, in the strongest terms, recommended Mr. Henderson, whose appointment accordingly followed immediately. The royal commission, nominating him Professor of Practical Astronomy and His Majesty's Astronomer for Scotland, which was dated the 18th of August of that year, required him to take upon himself the care and custody of the instruments within the observatory, and "to apply himself with diligence and zeal to making astronomical observations at the said observatory, for the extension and improvement of astronomy, geography, and navigation, and other branches of science connected therewith."

Mr. Henderson was now placed in a situation suited in every respect to his tastes, habits, and pursuits; and, as he was still young, those who were best acquainted with the extent of his knowledge, his industrious habits, and his facility and accuracy in all practical matters, formed the highest expectations of the value of his future services to astronomy. Nor were their expectations disappointed. The annals of the Edinburgh Observatory, his *Catalogue of Southern Stars*, his investigations of annual parallax, and other deductions from his Cape observations, besides various contributions relative to subjects of a less important, but always of an interesting nature, amply justify the recommendation of our

large steel collar carried by the conical axis was quite loose. It is well, perhaps, the discovery was not made sooner, or the instrument would probably have been condemned, and the observations been lost.

Council; while their very excellence increases our present regrets that a career so auspiciously begun has been so prematurely brought to a close.

As soon as Mr. Henderson had got his observatory into working order, and had established a regular routine of duty, he resumed the reduction of his Cape observations,—an occupation which engrossed the greater part of his leisure time during the remainder of his life, and of which the fruits are a series of papers communicated to the Society and published in our *Memoirs*, all more or less interesting, and some of them of the first order of excellence.

The next result of these reductions, and, indeed, one of the most intrinsically important of the whole, was a Catalogue of the declinations of 172 principal fixed stars, chiefly in the southern hemisphere, which was read to the Society at the April meeting in 1837. Owing to various causes of delay, the reduction of the right ascensions of the same stars was only completed, and the results given to the Society, in the course of the last year. They have, however, already been printed, and will appear in our forthcoming volume. Although the number of stars contained in this Catalogue is not large, it acquires importance and value both on account of the still relatively defective state of our knowledge of the absolute positions of the southern stars, and from the circumstance that it is the first which has been deduced from observations made in the southern hemisphere with instruments equal to those of the best European observatories. Indeed, if we except Mr. Johnson's excellent Catalogue of Stars observed at St. Helena, it must be regarded as the first in which the places of stars not visible in our latitudes have been determined with the precision and certainty now aimed at in astronomy. It was principally with a view to the determination of the positions of the principal southern stars, for the aid of navigation, that the establishment of the Cape Observatory had been urged upon and undertaken by the government, and this catalogue forms one of the most important instalments astronomy has yet received from it.

Although the determination of the absolute places of the stars formed the principal business of the observatory, Mr. Henderson was too zealous an astronomer to omit taking advantage of the circumstances in which he was placed to investigate various important points which can only be determined by a comparison of observations made at places remote from each other, or which acquire a special interest from the position of the observer. A point of this latter kind is the amount of refraction in the southern hemisphere. In an interesting paper on this subject, printed in Vol. X. of the *Memoirs*, he has given the results of a series of observations made at the Cape, of stars having a greater zenith distance than 85°, both north and south, and compared the refractions thence deduced with the tables of Bessel and Ivory. As tending to throw light on the important subject of horizontal refractions, these results are valuable with reference to general physics as well as to practical astronomy.

Another important determination, also prompted by the locality, was that of the moon's horizontal parallax. It is well known that the determination of this element was one of the principal objects of Lacaille's voyage to the Cape; his purpose being to obtain observations of the moon's declination corresponding to others made at different European observatories. A similar investigation was proposed for Mr. Henderson, or rather several stars had been marked in the *Berlin Ephemeris* and *Nautical Almanac* as favourably situated for having their declinations observed along with the moon's in both hemispheres, by which means the moon's apparent declinations are obtained free from the effects of errors in the assumed declinations of the stars. But the number of corresponding observations of this kind which could be made, being found to be too small to permit the element to be determined from them with sufficient accuracy, Mr. Henderson had recourse, whenever there was a deficiency of moon-culminating stars, to such of the principal stars as were observed, and could be used for comparison. The investigation, like all others which he undertook, is conducted in the most careful manner; and the result, although, in consequence of the mode of proceeding adopted, it is dependent on the accuracy of the determination of the relative declinations of the principal stars, is probably the best determination of the constant we are in possession of.

It would almost seem as if, in these investigations, Mr. Henderson had it in view to repeat the labours of Lacaille in his memorable residence at the Cape about the middle of the last century. In another and more important feature he may be considered as an imitator of that great astronomer. Lacaille had remarked that, in the *then* advanced state of astronomy no one could any longer be believed on his mere word; and that, in order to employ with confidence an observed position, it was necessary to have all the details of the observation and all the elements of reduction. No astronomer was ever more careful of the observance of this important maxim than Mr. Henderson. From the reading of the circle to the final result of the investigation, every thing necessary for the complete understanding of the process or verification of the work is carefully set forth and explained; and, accordingly, all his investigations and results are characterised by an air of truthfulness which it would be in vain to look for where a different practice is followed.

But of all the results which he deduced from this persevering examination of his Cape observations, the one which will be considered as the most interesting is the annual parallax of the double star α *Centauri*, amounting to about a second of space. This binary system was recognised to be double about a century ago; and, on comparing the observations of Lacaille with those of the present time, it is found to have an annual proper motion of about $3''\cdot6$. Being situated within about 30° of the South Pole, it is always above the horizon at the Cape, and, consequently, favourably placed for being accurately observed at all times. The

intrinsic brightness of the two component stars is also remarkable; and from this, and their large proper motion—circumstances indicative of proximity to our system—Mr. Henderson was led to suspect they might have a sensible parallax. He was not, however, aware of their large proper motion till he was about to leave the Cape; and the observations from which the result was first deduced, were accordingly not made with especial reference to the question of parallax, but for the purpose of determining their mean positions. The first indications of parallax were detected upon a comparison of their declinations with those of such of the standard stars as were observed on the same day throughout the year; but he deferred the announcement of the discovery until he had completed the reduction of the right ascensions, and obtained, by this means, a further test of the accuracy of his conclusion. A similar investigation of these latter observations confirmed the previous deduction; and, in a paper read to the Society at the January meeting of 1839, he announced a parallax of the double star amounting to about a second of space. With his habitual caution and accuracy, he applied every means of testing the result the observations afforded, deducing the parallax separately from the observations of right ascension, from the direct observations of declination, and from the reflected observations of declination, and this in respect of each of the two stars; and though the resulting parallax differed somewhat in the different cases, the general agreement was satisfactory, and the amount much too large to be ascribable to probable errors of observation. But notwithstanding the corroboration which the results of these different modes of deduction afforded each other, if the question of the parallax of this binary star had been allowed to rest in the state in which it was placed in Mr. Henderson's first paper, astronomers would probably have agreed in thinking his investigation only went the length of establishing a strong probability in favour of its existence. Mr. Main, after an elaborate comparison of the result with the individual observations from which it was deduced, observes, "For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its discovery, and the warmest thanks of astronomers, as an extender of the knowledge we possess of our connexion with the sidereal system."—*Memoirs*, vol. xii. p. 36. The result, however, was far too curious to be allowed long to remain without an attempt being made to confirm it. No sooner was Mr. Henderson's paper communicated to Mr. Maclear, than that energetic astronomer undertook a series of observations of the double altitudes of the two stars with the mural circle, with an express view to the question. They extend over seventeen months, from March 26, 1839, to August 12, 1840; and, as the mural circle at the Cape was changed shortly after their commencement, they have the further advantage of having been made, for the

greater part, with a different instrument. These observations being transmitted to Mr. Henderson, he immediately undertook to reduce them; and, in an elaborate paper inserted in Vol. XII. of the *Memoirs*, he deduced the parallax for each of the two stars both from the direct and reflected observations. The results entirely confirmed his former deductions, but, as was to be expected from the greater number of observations, they were more consistent. The mean gave, as before, a parallax of about 1″; whence it is inferred that this system, the nearest, perhaps, of the stellar bodies, is separated from us by an interval exceeding 200,000 times the distance of the sun from the earth.

Looking to the history of this interesting subject — to the consistent results given by the Dublin circle, which have been disproved by other instruments — and considering that the present determination has as yet been only partially confirmed by the observations of right ascension, the cautious astronomer will, perhaps, be inclined to maintain some reserve until the star shall have been examined with an instrument affording the means of exact micrometrical measurement, or, at least, until a longer series of meridian observations shall have been made; for, as has been remarked by Sir John Herschel, " It is only on a very long series of observations of absolute places, affected, as they are, by instrumental error and uncertainty of refraction, that any conclusion of this kind can rest with security." But whatever the ultimate decision of astronomers may be with respect to the parallax in question, there will be but one opinion as to the merit of Mr. Henderson's investigations, and the interest which attaches to the subject: and, in the meantime, it will be remembered that this is the first determination of the parallax of a star which has been confirmed by a different observer using a different instrument; and that if future observations shall continue to give similar results, which now seems a reasonable anticipation, to Mr. Henderson will indisputably belong the enviable distinction of having been the first who succeeded in an inquiry so often, but fruitlessly, attempted by astronomers, namely, the discovery of a fixed star whose distance from our system is capable of measurement and expression. For these researches he was proposed to the Council at the meeting in November last, as deserving the Society's gold medal for the present year, but his untimely death prevented the question from being entertained.

Another attempt made by him to determine the annual parallax was less successful. *Sirius*, the brightest star in the heavens, might be supposed to be one of the least remote; and some astronomers—Cassini, Lacaille, and Piazzi—had assigned it a parallax of several seconds. The Cape observations being well adapted for the investigation of the question, Mr. Henderson undertook the examination of those made by himself, and also a series by Mr. Maclear, in order to ascertain if they indicated any sensible parallax; but, in this case, the result amounted only to about a quarter of a second,—a quantity not exceeding, perhaps, the probable error of the determination.

Mr. Henderson's labours on the subject of parallax did not terminate even here. In a letter to Mr. Main, which was read at the December meeting of 1842, he gave the results of an investigation of the parallaxes, as they appear from his Cape observations, of twenty stars which had been observed sufficiently often to reduce the errors of observation within reasonable limits. They comprehend the greater number of the stars situated in the same region of the heavens as α *Centauri*, which have been indicated by Sir John Herschel as deserving of investigation for parallax, and which, on that account, are at present objects of particular attention at the Cape Observatory. In a few instances considerable parallaxes appear; in the greater number of cases the results are so small as to afford little hope of a measurable parallax being found, and in some they are negative. The mean of the twenty parallaxes is $+ 0''\cdot 29$, a result which, on the whole, may be considered as affording encouragement to continue the research.

It is matter of much regret that the reduction of the entire series of Mr. Henderson's Cape observations—the rich store from which the above interesting results have been extracted—has not been completed by him; and that the observations must consequently be deprived of the advantage of his final revision. From the examination of his papers it appears that the number of positions determined both with the mural circle and transit instrument, from April 10, 1832, to May 24, 1833, is between 5000 and 6000. The transit observations (made chiefly by Lieut. Meadows) are found to be carefully written out, with the column headed "correction of instrument" filled up for the whole period; and that headed "correction of clock" from the commencement to October 31, 1832. The circle observations, which were all made by himself, are in a similar state of forwardness; and it may be inferred from the appearance of the papers that if he had lived a few months longer, and the work had proceeded at the ordinary rate, the whole would have been ready for the press.

A statement of the results deduced by Mr. Henderson from his Cape observations could not be properly concluded without an expression of admiration for that disinterested zeal which led him to undertake and execute so great an amount of extra-official labour. The observations, it will be remembered, were made at a public establishment, and their reduction was matter of public concern and importance; and after his resignation of the appointment, the public had no claim on his services more than on those of any other individual. But, as already remarked, considerations of this kind never, in any degree, influenced his conduct; and, during ten long years, he gratuitously devoted to this purpose all the time he could spare from his official duties. In the same circumstances almost any other person would have claimed, and undoubtedly have obtained, both assistance and remuneration.

In addition to the results now mentioned, he rendered still another important service to the astronomy of the southern heavens, in superintending the reduction of the stars observed at the Cape

by Lacaille. This work, which he undertook at the instance of Mr. Baily for the British Association, was announced as completed in our Annual Report for 1843; but it has not yet been published.

Mr. Henderson's labours in the Edinburgh Observatory are well known to astronomers from the five volumes of observations which have been published for 1834-1839. A sixth volume is understood to be left nearly ready for publication; and the observations for the remaining years will, no doubt, still be rendered available to science. The published volumes are prefaced by an Introduction, containing a minute and most lucid description of the instruments and methods used in the observations and reductions, with every detail and explanation which can contribute to give authenticity and value to the work. His first year's observations having been referred to this Society by the Home Secretary, a committee of the Council reported them to be of first-rate excellence, and recommended their publication as matter of scientific importance. This report has been ratified by all astronomers; and, so far as they have yet been published, the Edinburgh observations not only do credit to the astronomer and his assistant, Mr. Wallace, but have conferred on the Observatory a high reputation among the similar institutions of Europe.

Although the periodical publication of the Edinburgh observations rendered it, in a great measure, unnecessary for him to have recourse to other channels of publicity, he continued, from time to time, to communicate to this Society various observations of phenomena, either of an occasional nature or having an immediate interest, and notices likely to be useful to practical astronomers. Among these may be mentioned observations of the planets near their oppositions and inferior conjunctions, of moon-culminating stars, of the annular eclipse of the sun on May 15, 1836; and, more especially, elements of cometary orbits. Indeed, if he had no other claim on our regard, his care in disseminating the earliest information respecting the orbits of newly discovered comets would alone have entitled him to the applause due to a useful labourer in the cause of astronomy. Various observations and notices of the kind now referred to were also communicated by him to Professor Schumacher, and published in the *Astronomische Nachrichten*, whereby they obtained immediate circulation over the Continent.

His attention at Edinburgh was chiefly directed to planetary observations, and the formation of an extensive catalogue of zodiacal stars. He had proceeded far in the latter work; and in one of his last letters, written to an intimate friend, he spoke of being engaged in fixing the positions of some of those stars in Argelander's recent catalogue, which, though visible to the naked eye, had escaped the notice of all former astronomers.

In recounting Mr. Henderson's labours in the Edinburgh Observatory, we should fail to do justice to his zeal if we omitted all allusion to other avocations which occasionally made large demands on his time. His office of professor, though he gave no lectures in his own

department, entailed on him various duties. During one whole session (1835-1836) he delivered the mathematical lectures for Professor Wallace, then incapacitated by illness; and in the last year he undertook, for some time, a similar duty for Professor Forbes, in the class of Natural Philosophy. In short, he was one whose general talents and habits of business, coupled with extreme benevolence and great good sense, rendered him a most useful coadjutor; and, accordingly, the largest share of any public business, with which he happened to be connected, was sure to fall into his hands.

Having none of the accessory advantages of birth, fortune, or early introduction, Mr. Henderson had to rely on his own energy and talents alone in conquering his way to fame; and he was only beginning to enjoy his well-earned reputation, both in his own country and abroad. He was admitted a Fellow of this Society in 1832, and of the Royal Society in 1840, and he had been a member of the Royal Society of Edinburgh since 1834. It does not appear that he was a member of any foreign society, but he was in frequent correspondence with Schumacher, Bessel, Encke, and other distinguished astronomers on the continent, by whom his talents and opinion on astronomical subjects were held in high estimation.

In 1836 he married Miss Adie, eldest daughter of the well-known optician and ingenious inventor of the sympiesometer. The death of this lady in 1842, a few weeks after the birth of their only child, produced an effect on his sensitive temperament from which he never completely recovered. In the summer of that year he was gratified by an event which afforded him at the time the liveliest pleasure, and ever after formed a bright spot in his memory. This was the visit to Edinburgh of Professor Bessel, whom he had always been accustomed to regard as his master in science; and for whose character and writings he entertained an unbounded admiration. In company with the great astronomer, and his countryman and colleague, the celebrated mathematician Jacobi, he made a short excursion to the Highlands; and his friends well remember the delight with which he used to recount the incidents of that journey, and relate anecdotes of his illustrious companions.

Although his constitution was never robust, and he was occasionally subject to low spirits, during the influence of which he would express misgivings as to his hold on life, his health did not undergo any visible change till the autumn of 1844, when he was suddenly seized with an illness of so alarming a kind, that, happening at the time to be on a visit to a friend, some days elapsed before he could be removed to his own house. From this attack he partially recovered, and hopes were entertained that he would soon be enabled to resume his usual duties; but a relapse having occurred, he expired suddenly on the 23d of November, a few weeks before he would have completed his forty-sixth year. The disease was then ascertained to be hypertrophy of the heart; and there

can be little doubt that, in the state of health induced by this organic disorder, the fatigue of the nightly observations, and of climbing the steep hill on the summit of which the observatory is built, had been extremely prejudicial to him, and contributed to accelerate its fatal termination.

The character of Mr. Henderson as an astronomer stands high, and his name will go down to posterity as an accurate observer, an industrious computer, a skilful manipulator, and an improver of methods in that department to which he devoted himself. Endowed by nature with perceptive powers of great acuteness, and accustomed by his early professional training to examine and sift the evidence of every fact or statement presented to his mind, and to keep it before him until he had obtained a clear conception of it in all its bearings, every acquisition he made was perfect; and all his knowledge was stored up in a memory of unusually retentive powers, in so orderly a manner as to be always available at the moment it was wanted. A sharp eye, and habits of order, regularity, and attention, enabled him to become an excellent observer; but the services for which he will be remembered consist not so much in numerous and accurate observations, as in the use which he made of them and the manner in which he worked out their results. At the outset of his career he was led (probably by the commendation of them in our *Memoirs*) to study attentively the methods of the German astronomers, particularly those of Bessel and Struve, upon whose model he formed his practice, and from which he never departed. All his memoirs and investigations are characterised by the excellences of those illustrious masters. Every observation is scrupulously discussed, and its results drawn out in the most concise and serviceable form. His processes are fully explained; his formulæ of reduction carefully chosen; no labour is evaded; and no circumstance which can affect the accuracy of the final result is passed unnoticed. Nor is the manner in which his results are set forth and communicated inferior to the skill displayed in deducing them. His descriptions of instruments, methods, and details of practice, are stamped by a simplicity, neatness, and precision, which shew at once the correctness of his taste and his mastery over his subject. His introductions to the Edinburgh observations may be cited as admirable specimens of astronomical writing. Nothing, indeed, is more remarkable in all his compositions than good taste, and the entire absence of every trace of exaggeration of his own merits or affectation of singularity. Discerning clearly his object, he pursues it in a direct undeviating course; never stepping aside for the sake of display, yet skilfully availing himself of every means of reaching it, which science had placed at his disposal. To represent him as profoundly skilled in the higher departments of physical astronomy would be to make a pretension, from which the modesty of his nature would have recoiled; but he had a sufficiently accurate knowledge of the analytical processes by which the phenomena of the universe are deduced from, or connected with, the theory of gravitation, to appreciate

correctly their bearings on the practical branches, to which he directed his attention. To a very considerable knowledge of mathematics he added great powers of calculation—powers which he never abused; for though less liable than most men to make mistakes, he, in no case, allowed himself to dispense with the most scrupulous revision of his work. One of his most distinguishing qualities was sound judgment. He never attempted any thing to which his powers were not fully equal; and, as a consequence of this, whatever he did he did well.

Few men have been more conversant with the modern history of astronomy, especially that branch of it with which he was more immediately connected. It may be said, without any exaggeration, that he knew, and could name the author of every invention or contrivance, whether mathematical or mechanical, bearing on his professional duties.

In his private character, and the relations of domestic life, Mr. Henderson was distinguished by great warmth of affection and amiability of disposition. Naturally modest and retiring he shrunk from the most distant contact with obtrusiveness and ostentation; but he knew well enough his position, and was by no means deficient in spirit to defend it. His disposition was cheerful and social; and when released from official duties he took great pleasure in the society of his friends, to whom he was endeared by the worth of his character, the unaffected simplicity of his manners, his kind-hearted benevolence, and enthusiastic admiration of whatever is noble and excellent. His reputation for astronomical knowledge, and his readiness to assist in its promotion, involved him in an extensive correspondence; but, however occupied he might be, when reference was made to him on any point connected with his professional pursuits, he was always to be depended upon for a prompt answer, either containing (as was most frequently the case) a full exposition of the matter in hand, or a frank acknowledgment that he had no information to give. He was disinterested and generous to a degree hardly consistent with his circumstances. Notwithstanding his slender appointments he had already collected an extensive and valuable astronomical library; but the proceeds of its sale were the principal part of the heritage he left for his orphan daughter.

The loss of so excellent a member, at an age when his knowledge and faculties had only attained to maturity, and when past triumphs justified the brightest anticipations for the future, cannot be regarded by the Society with feelings of ordinary regret. Let us hope there will never be wanting among us examples of similar and equally successful devotion.

The following is a list of Mr. Henderson's Astronomical Papers.

I. In the *Memoirs of the Royal Astronomical Society*:—

Observed Transits of the Moon and Stars nearly in the same Parallel of Declination over the Meridian. Vol. IV. p. 189.

Note on the difference of Meridians between the Observatories of Greenwich and Edinburgh. Vol. IV. p. 191.

Practical Rules for the Approximate Prediction of Occultations. Vol. IV. p. 587.

On the Latitude and Longitude of the Observatory at the Cape of Good Hope. Vol. VI. p. 125.

Positions of several Stars near the South Pole, deduced from Observations made at the Observatory at the Cape of Good Hope. Vol. VI. p. 133.

Observations of the Periodic Comet of 6·7 years, made at the Observatory, Cape of Good Hope, between Nov. 18, 1832, and January 3, 1833. Vol. VI. p. 159.

Observations of the Moon and Stars made with the Mural Circle at the Observatory, Cape of Good Hope, for the determination of the Moon's Parallax. Vol. VI. p. 205.

Observations of *Mars* and Stars made with the Mural Circle at the Observatory, Cape of Good Hope, for the determination of the Parallax of *Mars*. Vol. VI. p. 207.

Letter to Professor Airy, on the Sun's Parallax, as deduced from various Observations made at Greenwich, Cambridge, and the Cape of Good Hope. Vol. VIII. p. 95.

Supplement to a Paper, entitled "On the Latitude and Longitude of the Observatory at the Cape of Good Hope." Vol. VIII. p. 137.

On the Mural Circle of the Observatory at the Cape of Good Hope. Vol. VIII. p. 141.

Observations of the Comet of 1830, made at Ascension Island, by the late Captain Foster. Reduced by Mr. Henderson. Vol. VIII. p. 191.

Observations of *Vesta* at the Opposition in 1834, made at Edinburgh. Vol. VIII. p. 232.

Observations of *Jupiter* at the Opposition in 1834, made at Edinburgh. Vol. VIII. p. 234.

Observations of *Venus* at the Inferior Conjunction, in December, 1834, made at Edinburgh. Vol. VIII. p. 236.

Observations of *Mars* at the Opposition in January, 1835, made at Edinburgh. Vol. VIII. p. 238.

Places of the Comet of Biela, deduced from Observations at Slough and the Cape of Good Hope. Vol. VIII. p. 240.

Places of Encke's Comet, from Observations made at the Cape of Good Hope. Vol. VIII. p. 243.

Notes on Mossotti's Observations of the Comet of Encke. Vol. VIII. p. 250.

Notes on Mr. Dunlop's Observations of Two Comets in 1833 and 1834. Vol. VIII. 264.

Results of the Lunar Observations made at Edinburgh in the years 1834 and 1835. Vol. IX. p. 272.

On the Declinations of the Principal Fixed Stars, deduced from Observations made at the Observatory, Cape of Good Hope, in the years 1832 and 1833. Vol. X. p. 49.

Refractions of Stars near the Horizon, observed at the Cape of Good Hope. Vol. X. p. 271.
The Constant Quantity of the Moon's Equatorial Horizontal Parallax, deduced from Observations made at Greenwich, Cambridge, and the Cape of Good Hope, in 1832 and 1833. Vol. X. p. 283.
On the Parallax of *α Centauri*. Vol. XI. p. 61.
On the Parallax of *Sirius*. Vol. XI. p. 239.
The Parallax of *α Centauri*, deduced from Mr. Maclear's Observations at the Cape of Good Hope, in the years 1839 and 1840. Vol. XII. p. 329.
The Right Ascensions of the Principal Fixed Stars, deduced from Observations made at the Observatory, Cape of Good Hope, in the years 1832 and 1833. Vol. XV. p. 129.

II. In the *Monthly Notices* of the Royal Astronomical Society :—

Occultations of *Aldebaran* by the Moon, in the year 1829, computed for ten different Observatories in Europe, by T. Henderson, Esq. and T. Maclear, Esq. Vol. I. p. 89.
A List of the Occultations of *Aldebaran* by the Moon, in the year 1830, computed for ten different Observatories in Europe, by T. Henderson, Esq. and T. Maclear, Esq. Vol. I. p. 133.
Various Observations made at the Royal Observatory, Edinburgh. Vol. III. p. 129.
Opposition of *Uranus*, August 1835, observed at Edinburgh. Vol. III. p. 143.
Oppositions of *Jupiter* in January 1836, of *Juno* in January 1836, and *Vesta* in March 1836, observed at Edinburgh. Vol. III. p. 200.
On the Annular Eclipse of the Sun, on May 15, 1836. Vol. IV. p. 165.
Apparent Positions of Galle's first Comet. Vol. V. p. 9.
Elements of Galle's first Comet. Vol. V. p. 16.
On the Parallaxes of certain Southern Stars. Vol. V. p. 223.
Elements of the Comet of 1668. Vol. V. p. 266.
Elements and Ephemeris of the Great Comet of 1843. Vol. V. p. 266.
Letter to the Secretary on the Great Comet of 1843. Vol. V. p. 267.
Approximate Elements of the Comet of Faye. Vol. VI. p. 15.
On the Orbit of the Comet of Faye. Vol. VI. p. 18.
Elements and Ephemeris of the Comet of Faye. Vol. VI. p. 57.

III. In the *Philosophical Transactions of the Royal Society of London* :—

On the Difference of Meridians of the Royal Observatories of Greenwich and Paris. Vol. for 1827, p. 286.
Observations of the Comet of Encke made in 1832. Vol. for 1833, p. 549.

IV. In the *Philosophical Magazine*:—

On Mr. Burns' Method of finding the Latitude by Double Altitudes Vol. LXVI. p. 283.

Some Remarks on Captain Sabine's Pendulum Experiments. Vol. II. (New Series), p. 126.

V. In the *London Quarterly Journal of Science*:—

Rules for Computing an observed Occultation of a Fixed Star by the Moon. No. XXXVI. p. 343. Also in New Series, No. II. p. 434. Annexed also to the *Nautical Almanacs* for 1827–1831.

Remarks on the Determination of the Longitude from Observations of the Moon's Right Ascension. No. XXXVII. p. 109.

Discordance of the Lunar Observations made at Greenwich and Paris. No. XXXVII. p. 116.

Corrections of the Catalogue of Zodiacal Stars. No. XXXVIII. p. 239.

A Method of Computing the Sun's Horizontal Parallax from Observations of the Transits of *Venus*. No. XXXIX. p. 94.

Remarks on the Discordances observed between the Lunar Observations at Greenwich and Paris. No. XXXIX. p. 96.

Remarks on M. Mendoza Rios' Method of computing the True from the Apparent Lunar Distances. No. XL. p. 315.

In the *Quarterly Journal*, New Series:—

Remarks on the Solar Tables. No. II. p. 438.

Answer to some Objections stated against the Method for Computing an Observed Occultation. No. VIII. p. 411.

VI. In *Schumacher's Astronomische Nachrichten*:—

Note on the Occultation of Mercury by the Moon on Aug. 9, 1831. No. 190, col. 437.

Astronomical Observations made at Edinburgh in 1831. No. 242, col. 25.

Note on Mossotti's Observations of Encke's Comet. No. 243, col. 39.

Account of Observations made at the Cape of Good Hope in 1832 and 1833. No. 257, col. 293.

On the Longitude of the Cape Observatory, and the Sun's Mean Equatoreal Horizontal Parallax. No. 262, col. 403.

Elements of Two Comets observed at Paramatta in 1833 and 1834. No. 271, col. 117.

Mean Declinations of 172 Principal Fixed Stars, deduced from Observations made at the Cape of Good Hope. No. 318, col. 81.

Refractions of Stars near the Horizon, observed at the Cape of Good Hope. No. 319, col. 103.

On the Moon's Equatoreal Horizontal Parallax. No. 338, col. 25.

On the Comet of 1668. No. 476, col. 333.
Elliptic Elements of Faye's Comet. No. 495, col. 235.

Computations of the principal Lunar Occultations of the Fixed Stars, during the years 1826, 1827, 1828, and 1829, made for the Royal Observatory at Greenwich.

Elements for Computing the Eclipses of the Sun, and Lunar Occultations of the Planets and their Satellites, for the years 1826, 1827, and 1828. In various Numbers of the *Quarterly Journal.*

Computations of the principal Lunar Occultations of the Fixed Stars and Planets, for the years 1830, 1831, and 1832. In *Monthly Notices of the Royal Astronomical Society*, and in the *Nautical Almanac.*

Ephemeris of the Comet of 6·7 years, in *Supplement to Nautical Almanac for* 1832.

On γ *Virginis.* In Smyth's *Cycle of Celestial Objects*, vol. i. p. 483.

Mr. John Frodsham was originally placed under an eminent workman, who was in the employment of Messrs. Troughton and Simms, to learn the manufacture of astronomical and mathematical instruments. From this employment he was withdrawn by his father, of the firm of Parkinson and Frodsham, chronometer-makers, a vacancy having, by an unhappy accident, suddenly occurred in his establishment, which could only be filled up by a person of acknowledged ability, and in whom entire confidence could be reposed. Mr. Frodsham devoted himself immediately with great energy and success to the business of chronometer-making, and shewed a singular dexterity in some of the most delicate adjustments, particularly in isochronizing the vibrations of the balance for unequal arcs.

He pursued the business of his profession with great zeal, and, having the advantage of a previous liberal education, his friends looked forward to his future efforts with considerable confidence. His death, which occurred in July last, was occasioned by a fall from his gig, from which he was thrown with considerable violence on his return from a drive, in the neighbourhood of London. The members of this Society will, doubtless, sympathise with his father, Mr. W. J. Frodsham, under this severe bereavement. He has to grieve not only over the sudden removal of a beloved son, but of a friend and a partner, who shared with him the most serious and important of his business anxieties, and on whose advice and assistance he might with confidence have calculated during the remaining years of his life.

The Royal Observatory must occupy a very prominent place in this year's Report.

The reduction of the Greenwich planetary observations from

1750 to 1830 was suggested by the British Association at the Cambridge meeting. On the motion of the Board of Visitors of the Royal Observatory, Her Majesty's government undertook to defray the expense of printing, and committed the work to the charge of the Astronomer Royal. All is now done and printed, in 671 large quarto pages, except the introduction. There are five sections: I. contains the investigation of clock errors and rates, and computation of mean time, all by stars; II. Investigation (by stars) of index errors of quadrants and circles, and zenith points of circles (the *Tabulæ Regiomontanæ* are the basis of these two sections); III. Geocentric places of the planets inferred from the original observations, and corrected by the elements obtained in I. and II.; IV. Computation of the tabular geocentric places of the planets, each from the best existing theory applying to that planet (the four small planets excepted); V. A comparative view of the observed and tabular geocentric places, and an exhibition of the equations which this gives for the heliocentric errors of each planet.

The reduction of the Greenwich lunar observations from 1750 to 1830 was also suggested by the British Association, and has been carried on, under the superintendence of the Astronomer Royal, at the expense of the government. The reductions being now very nearly completed, the Board of Visitors has recommended to the government to print them with considerable detail, and with actual correction of the elements of the tables. This last is rendered practicable by the deduced results having been uniformly compared with those of Plana's lunar theory (with some emendations). It is understood that an adequate sum is to be inserted in next year's estimates.

It will be perceived that the Royal Observatory is making up its ledger; and future astronomers, who will nearly as soon publish unmade as unreduced observations, will be surprised at the uniform credit which it has maintained during the long period in which it has never investigated the state of its own accounts. The truth is, that it has always led the world; and it is not fair to demand of the highest why it is not yet more high. We may now confidently expect new lunar tables, and considerable emendations of the planetary ones. The astronomical world will not fail to bear in mind what it owes to the present Astronomer Royal, who, when at the head of the Cambridge Observatory, first presented a complete volume of reduced observations as part of the regular business of the institution. If, as may reasonably be assumed, the impulse given to astronomy in England by the young exertions of this Society was one of the causes of the foundation of an observatory at Cambridge, we form no trivial wish when we hope that the consequences of the future may equal in importance those of the past.

It is probably known to most of the Fellows that the government, on the representation of the Astronomer Royal to his Board of Visitors, has sanctioned the erection of an altitude and azimuth instrument for extra-meridional observations of the moon. For

more than a century all fundamental observations of stars and planets have been made in the meridian; and, if sufficient frequency of observation could be insured, there is no reason to suppose that any other species of observation could be required. In the case of the moon, however, there is a necessary loss of the meridian observation during a part of the month, and occasional occurrence of clouds at the time of otherwise visible transit; to which it must be added, that theoretical knowledge of our satellite is so advanced, as to make it difficult to carry it further by such sets of observations as can be procured with meridional instruments. The Astronomer Royal has therefore determined to commence the task of following the moon through her daily course to an extent which will supply, or more than supply, the failure of meridian opportunities; and your Council feels a sanguine presentiment that this abandonment of the meridian will be an epoch in the history of astronomy. The altitude and azimuth instrument is constructed on the same general principles as the ordnance zenith sector described by Mr. Airy at the meeting of May 13, 1842. It consists of very few parts, and these cast, and therefore rather massive; and so important has it been considered to unite the small parts, that the microscopes are cast in the same piece with the rest, and are bored afterwards. The instrument turns on pivots above and below. The revolving azimuthal frame consists only of four casts—namely, the lower end, with a pivot and four microscopes; the two sides, of which one has four microscopes; and the top, with its pivot. The moving vertical circle consists of two parts only; one having one pivot, the graduated circle, and the two ends of the telescope, the other having the other pivot. The extreme firmness which is required in the construction of a *theodolite* (as it may very properly be called), which shall give results comparable to those of the *fixed instruments*, is, of course, accompanied by considerable loss of manual adjustment, and consequent increase of arithmetical reduction. Those who have any idea of the very serious labour involved in the contemplated class of observations, as compared with that required in meridional work, will feel that the present Astronomer Royal has dictated to his successors the motto by which Ptolemy described Hipparchus, φιλοπονος και φιλαληθης; and they will also feel that he has adopted it for his own.

Your Council cannot but feel it to be a matter of congratulation that the principal public observers of Great Britain have, to no small extent, begun to bear in mind that different observatories, situated at no great distance from each other, should aim at diversified plans of action, and independent objects of investigation. While all have recorded the places of the bodies of the solar system, and have lent the assurance of number of observations, and consequent power of detection, to the *data* on which future planetary tables will be constructed, each one has tried, or is trying, to make the greatest progress of some one branch of astronomy peculiarly its own work. Thus, while Greenwich has constantly

devoted its peculiar attention, pursuant to the will of its founder, to the moon, the large equatoreal at Cambridge has suggested the researches of the Plumian Professor; the Radcliffe Observer at Oxford has devoted his special attention to the circumpolar stars, and will soon be furnished with a new mode of action by the possession of the splendid heliometer which is in preparation; and our lamented colleague at Edinburgh has left the materials for a catalogue of zodiacal stars. Differences of locality, of instruments, of mode of government, of taste and reading in the directors of different observatories, will originate differences of plan: it may be permitted to a body so closely connected with the common pursuit as your Council, and so deeply interested in the astronomical welfare of each and all of these institutions, to hope that these differences of plan will one day arise out of a matured system of co-operation, in which foreign observatories will be combined with our own. Much advantage has arisen in this country from the division of labour, which has thrown the observation of double stars and nebulæ upon the amateur astronomer; and more will always be gained, the larger the amount of strength and the wider the range of researches which are thus judiciously subdivided.

The benefit of co-operation has been lately seen in the junction of the observatories of Pulkowa, Altona, and Greenwich, for the determination of their differences of longitude. Our associate, Mr. Struve, had connected Pulkowa and Altona; and, having strongly represented to his government the propriety of taking Greenwich as the zero point for all longitudes, it was resolved to connect the two last-named observatories. A portable transit was erected during the last summer in a temporary observatory on the grounds of the Royal Observatory, and forty-two box chronometers were carried backwards and forwards by the steam-boats, eight times each way. At first Mr. Otto Struve observed at Greenwich, and Mr. Döllen at Altona, for two voyages: the observers were then reversed for four voyages, and again resumed their old stations for two more; by which arrangement it was hoped to eliminate both personal equation and its gradual changes. The clock in the temporary observatory was regularly compared with the Greenwich transit clock, so that the ordinary observations of the Observatory will contribute to the result: to this end all necessary observations for personal equation were made. The result is not yet completely calculated.

While the preceding operation was in progress, another of a similar kind was undertaken by the Astronomer Royal and Mr. Sheepshanks, for the connexion of Valentia in Ireland (the western point of Europe) and Greenwich; to which was added, the incidental determination of the longitude of Liverpool Observatory and Kingstown Harbour. Valentia is nearly in the latitude of Greenwich, and it will probably be the extremity of an arc of parallel extending across the south of Russia. Mr. Sheepshanks (and afterwards Mr. Hind) was stationed with a portable transit instrument at Kingstown, and thirty pocket chronometers were carried

backwards and forwards, eight times each way. The mode of carriage was as follows:—

The chronometers being in two well-padded cases, each containing fifteen, these cases were inserted in boxes which were screwed to the railway carriages and steam-boats (the owners of which gave every possible facility): these boxes were not disturbed throughout the whole operation, each one forming, in fact, a part of a carriage or steam-boat; all had similar locks, and each person employed was furnished with a key. An assistant took the chronometers from Greenwich to the Euston Square station; Mr. Hartnup received them at Liverpool and transferred them to the steam-boat; and Mr. Sheepshanks or Mr. Hind received them at Kingstown. In a similar manner, when a portable transit was erected at a station at Valentia, under the charge of Lieut. Gossett, R.E., the chronometers were carried backwards and forwards, ten times each way, in boxes fixed upon the mail coaches as far as Tralee, and afterwards in an express car, furnished by Mr. Bianconi.

Several parts of the disputed boundary between the United States and British North America are defined astronomically; and, to prepare for the proper execution of this part two officers of Engineers, Captain Robinson and Lieutenant Pipon, employed under the British Commissioner, Lieut.-Colonel Estcourt, were for some time stationed at the Royal Observatory. There was one point which was not necessarily astronomical, the drawing a straight line of between 60 and 70 miles to connect two defined points. As it appeared almost impossible to effect this by survey, from the difficult character of the country, Mr. Airy recommended that the azimuths at the two ends should be computed from observed latitudes and difference of longitudes, and that two parties should cut through the woods in the assigned directions, one from each terminal station. The two parties thus cutting, independently of each other, drew lines which met within 300 feet.

We have yet to acknowledge one more obligation to the Astronomer Royal, namely, the recent publication of a "Catalogue of the Places of 1439 Stars reduced to the 1st of January, 1840." This work contains the mean places of stars deduced from all the observations made at Greenwich in the years 1836 to 1841, inclusively. The place of the equinox is that resulting from observations during the same period. The year which corresponds to the mean of the observations of each element is also given; and the annual precession for 1840, with the proper motion for those stars in which account of proper motion has been taken. This necessary information with respect to the mean date was first given, we believe in the Cambridge Catalogue; and we trust that every future catalogue will also contain it. It is otherwise impossible to investigate proper motion, or the changes in proper motion, with the nicety which the present state of practical astronomy authorises us to apply to these delicate researches. The nomenclature is taken from Baily's *Flamsteed*, the magnitudes from Argelander's *Uranometria;* and there are columns of references for those stars which occur in

the Catalogues of Hevelius, Bradley, Mayer, Piazzi, the Astronomical Society, Groombridge, Pond, Argelander, Cambridge (first), Johnson, and Taylor. This brief notice needs no further comment here; for it would only be a waste of time to add one word describing the excellence of the instruments employed, the finish and perfection of the reductions, the care of the editor, or the utility of the final results. We congratulate all cultivators of astronomy on this noble addition to its *Fundamenta*, the influence of which will be instantly felt in every working observatory, and directly or indirectly throughout every department of the science.

In the Cambridge Observatory Professor Challis has confined himself in a great degree, so far as the meridian instruments are concerned, to the planets and those double stars which have been observed with the Northumberland equatoreal. A *first series* of observations of double stars is in preparation, and a *Second Cambridge Catalogue*, in continuation of the one which was inserted by Mr. Airy in our *Transactions*. The observations of the various recent comets with the equatoreal above mentioned will be valuable additions to the several yearly volumes.

Your Council have great satisfaction in directing the attention of the Society to the Observatory recently established at Liverpool by the corporation. In accordance with the advice of the Astronomer Royal, the astronomical portion consists of a transit-room and a dome for a large equatoreal. An adjoining apartment is appropriated to the chronometers which are brought there for trial or rating, and to the meteorological instruments; the rest of the building forms a comfortable house for the observer. A transit of five feet focal length and four inches aperture by Simms, a sidereal clock and a mean time clock by Molyneux, and a standard barometer (Newman's construction) by Adie of Liverpool, have been for some months in use, to the perfect satisfaction of Mr. Hartnup, the director. The telescope of the transit is a particularly fine one, and the mounting, which was directed by the Astronomer Royal, is the strongest and stiffest perhaps in existence. With the two clocks (which are within hearing of each other, and which are regularly compared whenever time is got or chronometer errors ascertained) it forms a perfectly efficient apparatus for getting and keeping the time, and is adequate to the most delicate determinations of right ascension. When the equatoreal is completed (the object-glass will have an aperture of eight inches, the mounting is to be under Mr. Airy's superintendence), we may expect most valuable assistance from the Liverpool Observatory in the extra-meridian branch of practical astronomy. But the principal and most interesting object of this establishment is, that of *giving true time to the great port of Liverpool*; an object which is of high national importance, and which has hitherto been almost unaccountably neglected. The observatory is admirably situated for this purpose, on the brink of the Mersey, at the entrance to the Waterloo Dock; the horizon is good, and infinitely better than could have been hoped for in the heart of a busy manufacturing

town. A ball similar to that at Greenwich is let fall every day, except Sunday, precisely at one P.M. Greenwich time, and the whole arrangement is so complete, and the longitude so well known, that the dropping of the balls at the two observatories may be considered to be simultaneous. It is evident that an observatory furnishing exact time will be of the greatest utility to all makers of *good* chronometers, and a hinderance to the venders of those which are indifferent. The ship-owners may, too, if they please, enjoy an advantage hitherto belonging solely to the Admiralty, that of having their chronometers tried and rated by a competent and disinterested party, previous to purchase; an advantage which will be thought almost inappreciable by persons fully aware of the dependence of modern navigation on the goodness of timekeepers; and, also, what indifferent watches are now disposed of to the unwary by ignorant and unscrupulous dealers. Mr. Hartnup's aid was most efficient in the measurement of the Valentia arc of longitude, and we trust that is only the first of many services to be rendered to science by this zealous and intelligent observer.

The magnificent telescope erected by the Earl of Rosse, which has attracted so much of public attention, is nearly in working order; nothing being now incomplete except some of the gallery machinery. It may reasonably be predicted that the energy which has called this instrument into existence will not quail before the more easy and pleasant task of using it. There is nothing on which it is so difficult to speculate as the probable results of an increase of optical power.

Passing to another extreme, your Council desire to notice the remarkable catalogue published by our associate, Mr. Argelander, of stars observed with the naked eye. The author is the Bayer of our generation, and his appreciation of magnitudes will probably come into universal use. He has reopened the road in which the astronomer may make himself useful without any instruments at all. His announcement of nearly thirty stars, which, though visible to the naked eye, are not to be found in any catalogue, startling as it may seem at first, will not, perhaps, surprise those who remember how various the objects of different catalogues have been. It is most desirable that these stars should be observed, with a view to the verification of the fact. If only half the number of new visible stars should be substantiated, it will be a useful lesson in any point of view. It is to be remembered that we are not positively to assume that these stars have been neglected by preceding observers: there is much reason to suppose that such bodies have before now disappeared. There may be *reappearances*, and there may be *new appearances*. Variable stars, with periods of several hundred years, or even less, might easily give rise to what Mr. Argelander has observed.

While commemorating the results of the past year, we ought not to forget those who are working in distant lands, under circumstances which prevent our receiving immediate information of their proceedings. M: Maclear's verification of Lacaille's arc at the

Cape of Good Hope is an operation in which English and French astronomers have each their peculiar interest; and some little account of this arduous and toilsome undertaking will doubtless be interesting to the Society.

With the aid of his assistants and a military detachment placed under his command, Mr. Maclear began, on the 1st of September, 1840, the measurement of a base line, eight miles in length, on the plains of Zwartland. Its direction is about west by south to east by north. The measurement of the first 1600 feet was repeated, and the difference of the results was found to be inappreciable, though the operation was rendered most harassing by the excessive heat. The measurement of the whole line occupied six months, not more than from 500 to 750 feet being completed in one day. Mr. Maclear thinks that there is just ground for believing that the entire base, 42,818 feet in length, is scarcely erroneous to the amount of half an inch, and that the error is probably much less than this.

Thenceforward the trigonometrical survey continued to occupy a large share of Mr. Maclear's attention. In December 1842 he proceeded in H.M.S. Arrow to take the zenith sector up to the Oliphant's River, which was then intended to be the northern limit of the arc, though it was afterwards deemed advisable to extend the triangulation both north and south of Lacaille's original arc; northward to Lily Fontein on the Kamies Berg, which has been accomplished; and southward to Cape l'Agulhas, which is approaching to completion.

This labour has subjected the observers to the greatest privations, to intense heat on the arid Karroos in summer, and to extreme cold in the winter, when they had to ascend such commanding heights as the Cedar Berg, the Snew Cop, the Winter Hock, and the Worcester Range, leaving their wagon or horses far below them, and being consequently reduced to sleep under the open sky, amidst snow and sleet, waiting from day to day, till the weather would admit of flashing their heliotropes towards the concerted points. Mr. Maclear speaks in the highest terms of the zeal of his assistants during the whole of these laborious and trying operations.

The East India Company has lately presented the Society with two valuable volumes, printed by the order of the Madras government. The first, containing the meteorological observations made at the Madras Observatory for twenty years (1822-1843), will find those who can more appropriately discuss its merits than your Council. The second touches us more nearly. It is a Catalogue of 11,015 stars, made from the five volumes of *Madras Observations*, and includes all of the Astronomical Society's Catalogue and of Piazzi's, which are visible at Madras, together with 3445 southern stars, selected with reference to the Paramatta Catalogue, all reduced to January 1, 1835, about the middle period of the observations. A systematic error of considerable magnitude in the divisions of the mural circle was discovered in 1840, and its amount ascertained

for each single division; and every place has been corrected for the error incident to the division in which it was observed. The proper motions of stars which appear to exceed a quarter of a second of space have been tabulated. Of the merits of this Catalogue it would be impossible to give any opinion at present; but your Council need not say that, from what is known of the zeal and industry of Mr. Taylor, they are well prepared to believe that it will soon be characterised as a valuable addition to sidereal astronomy, and an indispensable aid to the southern astronomer.

The progress of Sir John Herschel's reductions of his southern observations must interest the Fellows of this Society. From the state in which they now are, it is not very improbable that the Catalogue may appear in a year from this time. The constant attention which the completion of this great undertaking requires, has prevented Sir John Herschel from taking that share in the business of the Society, from which, previously to his departure for the Cape of Good Hope, its affairs derived so much benefit. The Council hope that the impediment will speedily cease to exist, not more for the sake of the Society than of its distinguished ex-president; for it must be admitted that, in travelling through the numerical reduction of observations, the astronomer finds very few of the flowers with which the love of science may strew the more intellectual part of the road.

By the death of Mr. Baily, the superintendence of the standard measure of length, and of the astronomical catalogues in progress at the expense of the government and of the British Association, pass into other hands. The standard scale has been undertaken by Mr. Sheepshanks, who has obtained from the Council the use of a vaulted room in the basement story in which to make the comparisons.

The *Nautical Almanac* for 1848, the fifteenth of the new series, appeared in the month of December last. For several years past, the advance which it was judged necessary to provide, namely, four complete years, has been gained; and it must not be forgotten that, when Lieut. Stratford was placed in charge of this national work, there was hardly a year in advance. On looking at the steady and effective manner in which this arduous undertaking has been executed, your Council feel that their praise is needless. With regard, however, to the obligations under which Mr. Stratford has laid astronomers, in matters unconnected with the immediate routine of the ephemeris, they can hardly be fully appreciated, and certainly not duly acknowledged, till after the next appearance of Halley's comet. There is also a stability given to the ordinary mode of conducting the *Nautical Almanac*, by the formation of a regular board of computers; a thing which did not exist when the present Superintendant came into office, and without which it would have been difficult to have fully carried out the recommendations of the *Nautical Almanac* Committee.

Since the last anniversary of the Society three comets have been discovered. The first was found by M. Mauvais at the Royal

Observatory of Paris on July 7; and, two nights afterwards, it was detected at Berlin. The elements differ materially from those of any comet whose orbit has been previously calculated.

The second comet was discovered at Rome at the observatory of the Collegio Romano, by our Associate, Signor Francesco de Vico, on the night of August 22. Considerable interest is attached to this comet, its period of revolution appearing to be only five and a half years. At the latter end of September it was easily seen with a small telescope; but, after that time, it became rapidly fainter, and, during the first week in December, was visible only with instruments of very great power. Another revolution of the comet will be completed in the early part of the year 1850.

A third comet was discovered on the evening of December 28, by M. D'arrest, of the University of Berlin. It is still visible. The elements somewhat resemble those of the second comet of 1793, for which Burckhardt computed an elliptical orbit of about twelve years' period; but the perihelion distance of the comet in 1793 was 1·4, while that of the present comet is only 0·9, a difference far too large to admit of the identity of the two bodies, except in the case of enormous perturbations.

Your Council will conclude this Report with one remark. It is obvious, from what has been stated, that this is a period of great activity, and that all parts of practical astronomy are in full cultivation. It may, perhaps, be rather a proof of this than the contrary, that the number of communications forwarded to the Society to be read at the ordinary meetings has materially decreased within the last few years. We may easily suppose that, as the work of the Observatory becomes more arduous, its superintendant has less time for the consideration of points which are not immediately connected with annual duties; and it is certain that nothing diminishes the occasional power of contributing to *Transactions* in the private astronomer, so much as self-devotion to some great branch of the subject, pointed out by personal taste or strong inducing circumstances. But, nevertheless, it ought to be remembered that the *Memoirs* of the Society are not merely the depository of facts, processes, or reasonings, which it is right to preserve. Though they may finally become nothing more, if it be right to apply such words to so important a function, yet it must be remembered that, on its first appearance, each volume is the impulse and stimulus of its day, the suggester as well as the receptacle of thought, the cause of future as well as the proof of past progress. The ordinary meetings of the Society must dwindle, to the serious diminution of its utility, unless they be nourished by communications of interest. And even granting that the magnitude of the objects on which astronomers are engaged at any one time, renders it almost impossible for them to supply the *Memoirs* at the equable rate hitherto maintained, it must not be forgotten that there are many points connected with the theory and practice, and, above all, with the history, of astronomy, which are constantly arising out of every research, directly or collaterally. Those to whom such things present themselves, are

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especially requested by the Council to bear in mind that any interesting information, however inadequate to be the basis of a formal paper, finds its immediate use at our meetings, and its record in our *Monthly Notices*. There is much reason for regret in the circumstance that the scientific body of our day has almost entirely lost the character of the collector of suggestions worthy of consideration, hints of useful tendency, and valuable, but isolated, information. If the minutes of the Royal Society in the time which preceded the publication of the *Principia*, should now and then excite a smile, it will be checked by the obvious consideration that we owe much of what has been since done to the currency which the disposition to communicate, and the facilities for doing it, afforded to mere suggestions. Much might, perhaps, be done by encouraging the slighter and less elaborate form of communication, which it is one object of the *Monthly Notices* to perpetuate; and your Council hope that those Fellows who have shewn themselves capable of greater things, will not forget that the small matter which, thinking only of themselves, they might be inclined to throw on one side, may possibly meet the difficulty, or advance the object, of some other member of the Society. At the least, it will add attraction to the proceedings of the ordinary meeting, which it is so important to the welfare of the Society, and, through it, of astronomy, to render both instructive and interesting.

Titles of Papers read before the Society between February 1844 *and February* 1845.

1844.

March 8. Observations of the Comet of Faye. By W. Lassell, Esq.

Right Ascensions and North Polar Distances of the Comet of Faye, from Observations at the Royal Observatory, Greenwich. Communicated by G. B. Airy, Esq. Astronomer Royal.

Right Ascensions and Declinations of the Comet of Faye, observed with the Equatoreal at the Observatory of Trinity College, Dublin, by Mr. C. Thompson. Communicated by Sir W. Hamilton.

Observations of the Comet of Faye, by C. Rumker, Esq.

Observations of the Comet of Faye made at the Observatory, Durham. Communicated by the Rev. Temple Chevallier.

Observations made at Hartwell, by the Rev. J. B. Reade. Communicated by Dr. Lee.

Elements of the Comet of Faye. By Professor Henderson.

Elements of the Comet of Faye. By J. R. Hind, Esq.

Letter from Professor Encke, on the Comet of Pons.

Occultations of Fixed Stars by the Moon, observed at Hamburg. By C. Rumker, Esq.

Further Remarks on the Revision of the Southern Constellations. By Sir J. F. W. Herschel, Bart.

Extract from a Letter of Professor Bessel to Sir J. F. W. Herschel, Bart.

A Letter from J. R. Crowe, Esq. concerning a Society established at Alten. Communicated by Dr. Lee.

April 12. Additional Observations of Faye's Comet. Communicated by Sir. W. Hamilton.

Meridian Observations of the Moon and Moon-culminating Stars, made at Hamburg during the years 1838 and 1839. By C. Rumker, Esq.

Elements of the Comet of Mauvais. By M. Götze. Communicated by Dr. Lee.

Observations of the Comet of Encke made at the Cape of Good Hope. By T. Maclear, Esq.

An Account of the Erection of the Herschel Obelisk at the Cape of Good Hope, accompanied by Colonel Lewis's Report, and a Plan of the Obelisk. By T. Maclear, Esq.

On Loud Beats of Clocks used in Observatories. By J. S. Eiffe, Esq.

May 10. The Right Ascensions of the principal Fixed Stars, deduced from Observations made at the Observatory, Cape of Good Hope, in the years 1832 and 1833. By Professor Henderson.

Observations of the Great Comet of 1843, made at Cape Coast Castle by G. Maclean, Esq. Communicated by Capt. Beaufort.

Extract of a Letter from J. R. Crowe, Esq. to Dr. Lee. Communicated by Dr. Lee.

Elliptic Elements of Bremicker's Comet, by M. Götze. Communicated by Professor Schumacher.

Elements of the Comet of Faye. By J. R. Hind, Esq.

June 14. Some Remarks on the Telescopic Appearance of the Moon, accompanying a Model and a Drawing of a portion of her surface. By James Nasmyth, Esq.

Observations of the Solar Eclipse of 1843, made near Mahe. By John Caldecott, Esq.

Sextant Measures of the Sun at the Eclipse of the Sun, December 21, 1843. By Capt. Sir E. Belcher, R.N.

On a Graphical Method of Predicting Occultations. By J. I. Waterston, Esq. Communicated by Captain Beaufort.

Some Remarks on the Great Comet of 1843, as seen in the Neighbourhood of Paramatta. By the Rev. W. B. Clarke. Communicated by Sir John Herschel.

Observations made at the Hamburg Observatory. By C. Rumker, Esq.

Scheme of Planetary Elements. By S. M. Drach, Esq.

Nov. 8. Memoir of Francis Baily, Esq. By Sir J. F. W. Herschel, Bart.

Dec. 13. Observations of the Moon and Moon-culminating Stars,

made at Port Essington. By Owen Stanley, Esq. Commander, R.N.

Letter from M. Mauvais to Mr. Baily, announcing the discovery of a Comet.

Circular Letter from Professor Encke, announcing the independent discovery of the second Comet of Mauvais by M. D'arrest.

Astronomical Observations made at Hudson Observatory, United States, by E. Loomis, Esq. Communicated by Lieut.-Col. Sabine.

Elements and an Ephemeris of the second Comet of Mauvais. By Professor Henderson.

Observations of the second Comet of Mauvais. By W. Lassell, Esq.

Observations of the second Comet of Mauvais and of De Vico's Comet. By C. Rumker, Esq.

Observations of De Vico's Comet, made at Aylesbury, by T. Dell, Esq. Communicated by Dr. Lee.

Elliptical Elements of De Vico's Comet, with an Ephemeris. By J. R. Hind, Esq.

Observations of De Vico's Comet, made at Ashurst, by R. Snow, Esq.

Observations of the Great Comet of 1843, made at St. Helena, by G. Brand, Esq.

Extract from the Translation of a Letter from Professor Bessel on the Variability of the Proper Motions of *Sirius* and *Procyon*. Communicated by Sir John Herschel.

1845.

Jan. 10. On the Flexure of a uniform Bar supported by a number of equal Pressures applied at equi-distant points, and on the Positions proper for the Application of these Pressures, in order to prevent any sensible Alteration of the Length of the Bar by small Flexure. By G. B. Airy, Esq. Astronomer Royal.

An Account of some Ancient Astronomical Tables in the Library of the Rev. C. Turnor, A.M. F.R.S. &c. By Mr. R. Harris, Assistant Secretary to the Society.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Hon. East India Company.
Royal Society of London.
L'Académie Royale des Sciences de l'Institut de France.
Depôt Général de la Marine de France.
British Association.
Royal Irish Academy.
Imperial Academy of St. Petersburg.

American Philosophical Society.
Royal Academy of Brussels.
Royal Academy of Berlin.
L'Administration Imperial des Mines de Russie.
La Société de Physique, &c. de Genève.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Bombay Branch Royal Asiatic Society.
Geological Society of London.
Zoological Society of London.
Linnean Society of London.
Society of Arts.
The Observatory at Milan.
The Observatory at Bologna.
The Radcliffe Trustees.
The Master-General of the Ordnance.
The Registrar-General.
The Editor of the Athenæum Journal.

M. Aimé.
F. S. Archer, Esq.
Professor Argelander.
M. Biot.
Dr. P. A. Browne.
R. Bryson, Esq.
Professor Challis.
Professor Christie.
E. J. Dent, Esq.
M. C. Dien.
Professor Encke.
Dr. Flügel.
T. Forster, Esq.
Major J. D. Graham.
Count Graberg da Hemsö.
J. Herapath, Esq.
E. Hopkins, Esq.
T. Jones, Esq.
H. Lawson, Esq.
Professor Littrow.
Professor Mädler.
Professor Narrien.
J. Nasymth, Esq.
Professor Plantamour.
Rev. J. Pullen.
Professor Quetelet.
Rev. S. J. Rigaud.
C. Rumker, Esq.
W. Rutherford, Esq.
Lieut.-Col. Sabine.
Professor Schumacher.
Rev. R. Sheepshanks.
Capt. R. Shortrede.
F. W. Simms, Esq.
Mrs. Somerville.
C. Stokes, Esq.
Lieut. W. S. Stratford.
Professor Struve.
M. O. Struve.
R. Taylor, Esq.
M. Van Hulst.
J. Waterhouse, Esq.
Miss Wollaston.

The President (G. B. Airy, Esq. Astronomer Royal) then addressed the Meeting on the subject of the award of the Medal as follows:—

Before I proceed to my immediate object, you will allow me, gentlemen, to express my regret that the duty of addressing you should have fallen upon me. The place which I reluctantly oc-

cupy ought, in the ordinary course, to have been filled this day by one to whom this Society mainly owes its existence and prosperity, and with whose scientific life every portion of our history is indissolubly interwoven. I will not, however, enlarge further on this irretrievable loss, but will briefly remind you that every member of the Society, who has been proud of his connexion with it while its interests were successfully supported by Mr. Baily, is bound in honour and conscience to give his best efforts for its continued prosperity, by the means which have hitherto proved so successful: individual and independent efforts tempered by a love of unanimity, and zealous industry guided by prudent forethought.

It is the duty of your president, gentlemen, to announce the decision of the Council as to the award of the Medal which, by the laws of the Society, is at their disposal on the present occasion: and I have to inform you that the Council have awarded the Medal to Captain W. H. Smyth, R.N. Foreign Secretary of this Society, for his *Bedford Catalogue*, forming the second part of the work entitled *Celestial Cycle*, which has been published by that gentleman within a few months.

The *Bedford Catalogue* contains the most interesting double and multiple stars of which the primaries are in Piazzi's Catalogue, and a selection of clusters and nebulæ from Messier's Catalogue (*Conn. des Temps*, 1784) and from the papers of the two Herschels; comprising 170 nebulæ and clusters, 580 double stars, 20 binary systems, and 80 triple and multiple stars. The magnitude, colours, &c. of the stars are carefully noted; there are numerous comparisons with the results (published and unpublished) of other observers; and the proper motion of the primary of each system is given with care. This scientific information is accompanied with much antiquarian research and literary history, and the work is likely to prove extremely attractive to the general reader. But the value of the work, in reference to the Medal of this Society, is derived almost entirely from its measures of double and multiple stars, and to these I shall confine my further remarks.

The subject of the labours for which this Medal is awarded is interesting; and the circumstances under which the Medal is awarded are peculiar. It is incumbent on me to make a few remarks upon the subject, and to explain the circumstances to which I allude; but, after the Report which has so long engaged your attention, it is necessary for me to be brief.

The astronomy of double stars may be stated to have commenced with Mr. W. Herschel's paper in the *Philosophical Transactions*, 1782. It is, therefore, essentially a modern science. But it is worth while to remark that it was not then begun with the views which have since become the principal motives for pursuing it. It was begun with the intention of discovering whether the observations of stars presumed to be at very different distances, but nearly in the same position as seen from the earth, would exhibit any indication of the earth's annual parallax. No such indication was discovered; but an unexpected and perhaps a more important

result was in no long time deduced from the observations. The relative places of the components of a double star were found to change, and the change had no respect to the position of the earth in its orbit, but went on from year to year. In several instances the change altered its character so completely that it could be represented in no way but by supposing that one star revolves round the other. And from that time the observations have been pursued almost exclusively with the view of tracing the orbits of binary stars.

One step of great importance has been made. Methods have been successfully introduced for the investigation of the elements of the orbits of double stars, on the supposition that the law of gravitation applies to them. And these methods have been applied to many stars, and from the elements so computed ephemerides have been prepared, by means of which the theoretical position of the double stars, computed on the same law of gravitation, may be compared with the position which shall be observed in the heavens.

To complete this outline of the progress of double-star astronomy, it appears only necessary to add, that it is believed that not more than one or two stars have completed their revolutions since they were first observed, and that there is no sufficient evidence that the same orbit has been retraced in successive revolutions.

Had I attempted, gentlemen, to enter more in detail into the history of this science, I should have done little more than weary you with the repetition of the same name. A Herschel was the projector of the science; the same Herschel established it to the utmost extent to which a most powerful intellect using the most powerful instruments in the world could carry it; another Herschel improved the accuracy of the observations, increased their number, and fixed the positions of many stars for an epoch sufficiently distant from the former to give accurate measures of their motions; and, finally, introduced that method of determining the elements of their orbits which is yet probably the best that exists. A Struve has filled volumes with the records of the observations made with the magnificent instruments at his command. Compared with these, the additions made by others to the theory or to the observations appear small. Yet it would be unjust to omit mention of the labours of Savary on the theory, and of those of South, Dawes, Bessel, and Mädler on the observations. To these names we can now add that of one whose labours place him in a higher position, the name of Smyth.

I may perhaps, for a moment quit the scientific part of this notice to remark that this science is in its origin and principal advances essentially English, and that by far the greater part of the work done upon it has been done by private and not by official observers. The former class is one of whom our country has good reason to be proud. I say advisedly that, since the time of Tycho, no country has witnessed efforts, directed with such force, such judgment, and such perseverance, as those of Herschel and Groombridge in sidereal observation, and those of Baily in

astronomical literature and in observations of a different class. It has been the pride of our men of business to shew that in them at least the effect of the cares incidental to their position has been not to degrade but to sharpen the intellect; not to render it insensible to every thing but gain, but to shew that honourable gain is only a means to an end, and that that end is the very highest cultivation of the mind.

Although the instance before us is in some degree different, its general character is the same. An officer, whose rank has been derived, in the first instance, from the honourable profession of arms; whose European reputation has been founded upon his services, first as a volunteer and afterwards in official employment, in the scientific and useful task of maritime survey; employs the leisure hours of his riper years upon the furtherance of the astronomy of double stars, devotes to that object his fortune and his energies with a perseverance scarcely inferior to those of the persons to whom I have already alluded, and finally produces an extensive catalogue of double stars possessing, as we believe, the highest claims to the favourable reception of the scientific world. I cannot forbear to add that the results of this labour have been published in a form which cannot fail to fix the attention and to direct the studies of many other able men of the same class: but I add also that this circumstance ought to have no influence, and has had no influence, in deciding your Council on the award of this Medal.

I might offer you my reasons, gentlemen, for believing that observations, like those of double stars, requiring little calculation, but demanding peremptorily the most complete devotion of time and energies when favourable states of the air occur, are likely always to be better carried on by private observers than by official astronomers. I might state, that the regular observer, when wearied with five hours' calculation in the forenoon, is not likely to seize upon that precious sky which sometimes suddenly presents itself in the evening, and to continue his observations till dawning day terminates them. I might, on the other hand, explain that private observers can seldom undertake the masses of calculation which are incidental to meridional observations. But I shall remark no further on this than to observe that, in the instance of the double-star observations before us, as well as in many of the others, discretion has been shewn in the selection of the subject as well as perseverance and skill in the pursuit of it.

Gentlemen, the position of the person by whom this work has been executed is thus far important, that it is necessary for us to distinguish between the work executed in the discharge of official duty, and that which is presented by the gratuitous labour and expense of a private individual upon whom we have no claim of any kind. The former circumstance may frequently prevent us from even taking into consideration the merits of some important works: the latter will render similar works admissible for our judgment as to the propriety of awarding to them our medal. But

when I have said *admissible* for our judgment, I have said all. No claim whatever is established by this gratuitous character of the work. The claim must be founded only upon the value of the work with reference to the present wants of science: and to this point I shall now advert.

It has happened, gentlemen, that I have twice had the honour to deliver addresses from this chair, in which allusion has necessarily been made to the nature of the nebulæ, and of those apparently nebuloid bodies, the comets. I have endeavoured to explain my idea, that it is in the examination of these that the cosmogonic mysteries of the world are to be read on the large scale, as in geology on the small scale. The investigation of the motion of double stars appears to me likely to give us an insight into laws equally grand, but of very different character. It is here alone that we can see the mechanics of the universe on a grand scale. The radius of the orbit of *Uranus* is small in comparison with the distance of the two stars of 61 *Cygni*, and probably *very* small in comparison with the distance between the components of some binary stars whose parallaxes have not yet been ascertained. The law of gravitation seems to be failing even at the distance of *Uranus*. May it not, perhaps, fail more completely, or may not a different law almost completely prevail over it, at distances like those of the double stars? Whether this question is to be answered affirmatively or negatively, and whatever may be the modification which the law may require, this appears certain, that it is only in the observation of double stars that we can find an answer to the question.

If now we inquire what is the quality of our observations upon which the correctness of our answer will mainly depend, we find that it is *accuracy;* comprising under this word the two different steps, of exactness in making observations, and certainty in transmitting them to the reader. And upon the importance of this accuracy, as contrasted with number or variety, I cannot express my opinion with sufficient strength. It is matter of history that the establishment of the laws of Kepler, of the system of Copernicus, and finally, of the theory of gravitation, depended upon a discordance between the theory of that period and the observations amounting to eight minutes of arc. This was then a small quantity but certainly appreciable, if the best instruments and best methods of the time were used. Now, gentlemen, am I wrong in saying that the establishment of a cosmical theory, infinitely more comprehensive and more important than that of gravitation, may depend upon the certainty of a measure to the tenth part of a second? I say, that it is more than possible; that it is highly probable; and that there is fair probability of its occurring within our time. Kepler on a similar occasion put to himself the question, "Is it likely that an observer so accurate as Tycho can have been in error by eight minutes?" and boldly answered to himself, "It is impossible, and, therefore, a new theory must be formed." When the question shall be put regarding the measures of the Bedford

Catalogue, made at a critical time, and on which a future theory may hinge, Can these numbers be trusted with certainty to one or two-tenths of a second? shall we be able to answer, Without doubt they can? This leads me to a very important part of my present remarks.

The Catalogue of the Celestial Cycle, as exhibited to the public, contains simply the result for each star of all the observations made on that star. In some cases results are given for more than one epoch: but, in all cases, every result is given without exhibition of the individual observations from which it was deduced. This form of publication is open to a very grave objection, and one which was seriously discussed by your Council. In a remark above I spoke of the accuracy of published observations as depending on two circumstances: exactness in making the observations, and certainty in transmitting them to the reader. With regard to the exactness of the observations, we had the evidence of a member of the Council who had compared unpublished observations made under the most unquestionably favourable circumstances with individual observations made by Captain Smyth: and we had also the indirect evidence derived from the progressive changes in the relative positions of some of the stars. These kinds of evidence probably had their weight with members of the Council; but to me, I must aver, they were entirely unnecessary. My confidence in the exactness of the observations is purely personal. Knowing the attention which has been given to the adjustments, the intentness of the observer upon his work, the nerve which is made steady rather than disturbed by the anxiety to procure a good observation, and the general skill in the management of the instruments, I can truly say, that if an accurate observation were required, I would desire that it should be made by Captain Smyth. Yet I could wish that I had the means of exhibiting to the world the grounds of my general confidence in the skill of the observer. Still more, I wish that I could produce sufficient means for estimating numerically the probable error of the observations, as affected not only by personal sources of error, but also by the causes of error which no skill can overcome. These remarks apply to the probable exactness of the observation only. With regard to the certainty of transmission to the reader, there is no sufficient security. The fairness of apportionment of weights, the correctness of means of separate results, the correctness of the printing; for all these there is no security. Let it not be supposed that these remarks are answered by referring me to the circumstances, that the computation is easy, that it has been performed by the astronomer himself or immediately under his eye, and that he has himself superintended the printing. I know by experience that errors are more likely to occur in easy than in difficult computation: that the principal person usually performs calculations and reads proof sheets with less accuracy than comparatively illiterate assistants; and that, after all his care in passing the sheets through the press, errors will creep in over which he has no control whatever. Had the manuscripts of the

observations and of the calculations in this instance been placed at our command, my remarks would have been completely answered. In the case of Lord Wrottesley's Catalogue, to which the Medal of this Society was awarded, and in that of Groombridge's Catalogue, the printing of which was placed by the Admiralty under my superintendance, the original observations and the intermediate calculations were placed at the command of this Society; and the repeated references which already it has been found necessary to make to the latter manuscripts prove the propriety of this caution. In the instance of the Bedford Catalogue we have no such power of referring to the originals. Feeling these things, gentlemen, and impressed with a sense of the responsibility to you and to the world of science which is implied by my position in this chair, I deem it my duty to state to you, that I for one have hesitated in assenting to this award except in the hope that the manuscripts relating to these observations would at some time be placed in our hands. And I am fully persuaded that it is the general feeling of the Council that the reasons upon which this Medal is now presented are such as have never before been used to justify our awards, and are not likely to be used again. I trust that the value of the Medal will be greatly enhanced to Captain Smyth by this consideration. I trust that he will perceive that, where direct evidence was wanting, this Council have not refused to give to the world their most solemn assurance of the value of the Bedford Catalogue, relying only, until further security shall be given to them, upon their personal appreciation of the instrumental skill, the editorial care, and the general exactness and fidelity of the observer.

(*The President then, delivering the Medal to Captain Smyth, addressed him in the following terms*):—

Captain Smyth,—In the name of the Council of the Royal Astronomical Society, I present to you this Medal. And I beg leave to convey with it the expression of my own opinion that never was a Medal more worthily earned. Permit me, sir, at the same time to remark, that the character of the Council is most deeply pledged in this award, and that I trust that, at no distant period, it will be redeemed by such communication of the details of the observations as will enable the Council to refer other inquirers to publications that are within the reach of all for a sufficient justification of this judgment.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: Captain W. H. Smyth, R.N. K.S.F. D.C.L. F.R.S. —*Vice-Presidents:* George Biddell Airy, Esq. M.A. F.R.S. Astronomer Royal; Samuel H. Christie, Esq. M.A. F.R.S.; Bryan Donkin, Esq. F.R.S.; Thomas Galloway, Esq. M.A. F.R.S.—

Treasurer: George Bishop, Esq.:—*Secretaries:* Rev. Robert Main, M.A.; William Rutherford, Esq.—*Foreign Secretary:* Rev. Richard Sheepshanks, M.A. F.R.S.—*Council:* George Dollond, Esq. F.R.S.; Solomon M. Drach, Esq.; Lieut.-Col. George Everest, F.R.S.; Rev. George Fisher, M.A. F.R.S.; Manuel J. Johnson, Esq. M.A.; John Lee, Esq. LL.D. F.R.S.; Edward Riddle, Esq.; Richard W. Rothman, Esq.; Lieut. William S. Stratford, R.N. F.R.S.; The Right Hon. Lord Wrottesley, M.A. F.R.S.

James Joseph Sylvester, Esq. F.R.S.; John Glaisher, Esq. of the Observatory at Stone, near Aylesbury; William Dashwood Fane, Esq. of Old Square, Lincoln's Inn; John Hartnup, Esq. Director of the Observatory at Liverpool; and the Rev. William Reade, B.A. of Welby, near Grantham, were balloted for, and duly elected, Fellows of the Society.

The following communication, which contains a continuation of the observations of Mauvais' second Comet made at the Cape of Good Hope, has been received since the Meeting:—

Observations of Mauvais' second Comet made at the Royal Observatory, Cape of Good Hope, under the direction of T. Maclear, Esq. Communicated by the Astronomer Royal.

Day of Observation.	Star of Comparison.	Cape Mean Time for R.A.	Difference of Right Ascension.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination.	No. of Obs.
1844.		h m s	m s		h m s	′ ″	
Nov. 10	(12)	14 59 4·5	−0 56·22	10	14 59 4·5	+17 52·2	10
	(12)	15 45 49·6	−0 59·90	12	15 45 49·6	+19 22·6	12
11	(13)	14 49 40·6	+1 12·60	8	14 49 40·6	− 5 24·1	8
15	(14)	14 58 57·4	+1 44·22	10	14 58 57·4	+29 20·3	10
	(14)	15 48 30·3	+1 39·44	8	15 48 30·3	+31 9·3	8
16	(15)	14 45 39·1	+2 59·71	6	14 45 39·1	+12 47·4	6
	(15)	15 32 45·7	+2 54·98	9	15 32 45·7	+14 32·1	9
19	(16)	14 11 56·1	+0 13·53	10	14 11 56·1	− 1 6·7	10
	(17)	15 9 33·7	−3 2·30	12	15 10 55·0	−23 51·3	11
21	(18)	14 33 32·6	−3 31·58	10	14 33 32·6	+18 21·6	10
	(18)	15 38 36·8	−3 40·87	10	15 38 36·8	+21 14·3	10
24	(19)	14 52 58·3	+0 53·79	12	14 52 58·3	+12 38·0	12
25	(20)	14 33 58·9	−1 16·61	5	14 33 58·9	−20 7·9	5
	(20)	15 5 12·6	−1 21·31	6	15 5 12·6	−18 41·8	6
	(20)	15 36 19·5	−1 26·64	6	15 36 19·5	−17 17·2	6
26	(21)	15 15 56·3	−1 3·29	9	15 15 56·3	−13 38·8	9
	(22)	15 52 53·3	+2 17·43	5	15 52 53·3	+ 7 21·9	5

The sign + denotes the comet's right ascension or declination to be greater than the star's; the sign — the contrary.

The observations were made with Dollond's 46-in. achromatic telescope and position-wire micrometer having two moveable wires (spider lines), and one fixed at right angles to them.

The preceding observations are not corrected for the effects of refraction and parallax.

Approximate Places of the Stars of Comparison.

Star.	Mag.	Approximate Right Ascension.	Approximate Declination.	Remarks.
		h m s	° ′	
(13)	8·9	12 14 35	−33 8	
(14)	7·8	12 5 47	35 52	
(15)	7	12 2 37	37 0	
(16)	9	11 57 43	39 57	
(17)	6	12 0 54	40 22	No. 6471 of the Madras General Catalogue.
(18)	6	11 55 39	41 34	No. 6430 of the Madras General Catalogue.
(19)	8	11 40 56	44 43·5	
(20)	8·9	11 39 19	46 18	
(21)	7·8	11 34 44	47 18·8	
(22)	6·7	11 31 17	−46 59·6	

Note.—In the former list of observations the differences of declination made on November 10 were not corrected for the index error of the micrometer. The observations are repeated above, with the proper correction.

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ROYAL ASTRONOMICAL SOCIETY.

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Capt. W. H. Smyth, R.N., President, in the Chair.

John Washington, Esq. Capt. R.N. F.R.S., and Edwin Dunkin, Esq., of the Royal Observatory, Greenwich, were balloted for, and duly elected Fellows of the Society.

The Secretary read the following extract of a letter from the Rev. C. Turnor to the President, accompanying the gift of a valuable astronomical manuscript, concerning which a paper was recently presented to the Society by Mr. Harris:—

"The favourable opinion you have expressed to me of the merits of the two volumes of Astronomical MSS., written on vellum, in, I think, the fourteenth century, which I purchased of Mr. Thorpe, the London bookseller, induces me to think they might be acceptable to the Royal Astronomical Society; may I, then, request the favour of you to present them, in my name, to that Society? I beg to assure you I shall feel great satisfaction in their being preserved in the library of that Society, which has been instituted for the purpose of advancing the science of Astronomy."

The following letter from the President to the Secretary was read:—

"*No. 3 Cheyne Walk, Chelsea, March 13, 1845.*

"Dear Sir,—As the cabinet-box of which I make mention on page 423, Vol. I., of my *Cycle of Celestial Objects*, contains the original slips from which my work was written, it may, possibly, be a desirable reference at some future day, and, in that view, I beg to present it to the Royal Astronomical Society. It contains the whole results of my measure-department in the working-list order of right ascension, the means having been reduced most carefully by myself, and repeatedly compared with such rigour, that none but the most insignificant error can possibly remain. In addition to what has been printed, these slips contain the diagrams and quadrants of each object, to the absence of which all the graver double-star mistakes which I have yet encountered are directly imputable. There are also a few unprinted remarks relative to the identity, &c., of the objects.

"It was intended that this cabinet should remain at Hartwell

House, together with the polar axis and the Bedford manuscripts, but Dr. Lee has kindly consented to this transfer. Previously, however, to its removal, a most careful scrutiny has been instituted of the slips and other papers with the printed *Cycle*, and various typographical errors have been detected in consequence. Some printed lists of these are herewith forwarded for any Fellows of the Society who may possess a copy of the work.—I have the honour to be, &c.

"W. H. Smyth.

"*Rev. R. Main.*"

The following communications were read:—

I. Copy of a Letter from Professor Fuss, Perpetual Secretary of the Imperial Academy of Sciences at St. Petersburg, to the Baron Berzelius, Perpetual Secretary of the Royal Academy of Sciences at Stockholm, on the contemplated extension of the Swedish Arc of Meridian, from its northern extremity at Pahtawara to the North Cape; with a Letter from Capt. Sir John Ross, R.N., to Dr. Lee. Communicated by Dr. Lee.

II. A Letter from the Rev. S. J. Rigaud to the Secretary, on the Character of Halley.

"*January* 9, 1845.

"Dear Sir,—In addition to the arguments which I have employed in defence of Halley, in the tract which I did myself the honour to present to the Astronomical Society, I wish to mention the following facts:—

"1st. I have evidence to shew that Halley was supposed at one time to be an aspirant to the office of precentor in a cathedral, I think that of Chichester, but I have not the memorandum by me in London. He was not in orders, so that this is clearly a mistake; but it is inconceivable that a man of known infidel views could have been seriously mentioned as a candidate for ecclesiastical duties.

"2d. The following passage appears in the correspondence of Sir Thomas Browne, the learned and excellent author of the *Religio Medici*, vol. I. p. 224:—

"'Heé (*i. e.* Halley) came to church constantly, the parish church, which was St. Aldate's, commonly called St. Fowl's, and whither the scollars of Pembroke college also went, and had one isle for themselves. Hee was a very good and plaine-dealing man, and had read Euclide and Ptolemie very accurately, and also Aristotle, whereof we should often discourse, and I cannot but remember him with some content.'

"The name of the Saint *Fowl's* is probably a mistake in transcribing or printing from the original MS. for *Towl's*, the ordinary pronunciation of the name being *St. Olls*.

"I consider this extract of great value; not only does it give the direct testimony of Sir T. Browne to regular habits of religious observance in Halley, but it bears remarkably on another point. It has been usual to trace Halley's supposed scepticism to an exclusive

devotion of his mental powers to demonstrative reasoning, or to physical phenomena, and the inductions derived from these. It is not now necessary to argue how utterly false is the supposed connexion as cause and effect between such speculations and infidelity; for the passage which has been cited shews that in the present case even the presumed cause was not present; it was not only inactive, but actually non-existent. Halley's mind had been directed, not only to physical, but to metaphysical and moral science. His reasoning powers were practised, not only in demonstrative, but in moral and contingent evidence.

" Nor is this passage from Browne the only evidence to this fact. An analysis of the Aristotelian *Ethics* carefully drawn out in Halley's own handwriting, is in existence, preserved in Lord Macclesfield's library, at Shirburn castle; bound up with it, is also a treatise on logic. I have not yet had an opportunity of examining this, but, knowing how logic was taught at that period, I think it very likely that the tract may shew him to have been well acquainted with dialectical science, or the logic of contingent, no less than that of necessary truth. Much, however, may be inferred from what I am able to state positively; and, trusting that it may be of interest to yourself and some members of the Society,—I remain, dear sir, &c.

" Stephen Jordan Rigaud."

III. A Description of the Observatory of the College of Georgetown, near Washington, in the United States of America, by James Curley, Esq. Communicated by W. Simms, Esq.

" The Observatory of Georgetown College is situated a short distance from the banks of the Potomac River, 160 feet above high-water mark; it commands an extensive view of Washington and the neighbouring country, and is in sight of, and one and a half miles distant N.W. of, the Naval Observatory recently erected by our government. The house fronts the south, is 60 feet long, and about 30 feet wide, has 3 rooms on the ground-floor, of which the east and west, designed for meridian instruments, are 15 feet in height; the central part of the building is 30 feet square, running up two stories, each 15 feet in height, and appropriately surmounted with a moveable hemispherical dome 20 feet in diameter.

" In the western room we have lately mounted a transit telescope, having a $4\frac{1}{2}$ inch object-glass of 76 inch focal length, and made in the most approved manner by Ertel and Son, of Munich. A sidereal clock by R. Molyneux is in the same room.

" The eastern room is ready for a meridian circle, which Mr. Simms, of London, has in hand; the circles will be 46 inches in diameter, and the telescope 5 feet in length.

" The room formed by the dome will be occupied by an equatoreal refractor, now being made by Gambey, of Paris; it has an object-glass of 7 French inches in diameter, and of about 10 feet focal length; the hour and declination circles are 16 and 20 inches in diameter, the latter reading to 10 seconds of space, and the former to 4 seconds of time. This instrument will rest on a solid

pier of masonry 40 feet high, 11 feet square at the base, and 6 at the top, and capped with heavy slabs of freestone.

" Besides these, there are some smaller instruments, and a good mean-time chronometer by R. Molyneux : another sidereal clock is daily expected from London. We desire and hope, in time, to obtain what is necessary to make the establishment complete."

IV. Communications respecting the Great Comet of 1845.

1. Estimated Positions of the Comet made at Nevis in the West Indies. By George Webbe, Esq. Communicated in a letter to the Secretaries.

" *Nevis, West Indies, Jan.* 12, 1845.

" On the evening of Sunday, the 5th instant, a large and very brilliant comet became visible here at sunset, having then first emerged from the solar rays.

" On the evening of Monday, the 6th, I first observed it; but being unprovided with any instrument mounted equatoreally, and, indeed, having no instruments at all prepared for observation, in consequence of a change of residence, I was constrained to be satisfied with such estimated values of its position as I could best obtain. On the evening of the 6th, its near approach to λ^1 and λ^2 *Gruis* enabled me to lay down a telescopic triangle by estimation, by which I succeeded in obtaining an approximation to its place, which probably was not very erroneous. On the 7th, 8th, and 10th, I was not so fortunate, and was obliged to be satisfied with estimating its position by eye, in reference to β, θ, and ι *Gruis*. On the 9th, from its proximity to θ, I had an opportunity of making a better estimate. On the 11th, the clouds prevented any observation.

" Although this comet will bear no comparison with the magnificent one of 1843, it is yet a very splendid object. The nucleus is large, and rather suddenly condensed, but indicates nothing like a defined termination. The tail appears homogeneous, undivided, and straight, and at present seems to be about 10° in length. From recollection, I should say, that the general appearance of this comet is fully as imposing as that of Halley's in its most brilliant condition, if not more so.

Deduced Positions of the Comet.

Day.	Right Ascension.	South Declination.
d h m	h m	° ′
Jan. 6 7	22 21·5	44 44
7 7 15	22 35·5	44 32
8 7	22 47·5	44 22
9 7 30	22 58	44 10
10 8	23 7	43 52

2. Results of Sextant Observations of Distances of the Comet from known Stars. By W. H. Simms, Esq. Extracted from a letter to W. Simms, Esq., and communicated by him.

"*Colombo, Ceylon, January* 15, 1845.

"A large comet has been visible here lately. I saw it on the 31st of December last, when its nucleus was about as bright as a star of the third magnitude, and the tail about 15° long, its edges being sharp and clear, and the light very equally diffused between them. I subjoin the results of a few observations with the sextant,* which I believe may be depended upon as *tolerably good*. The mode of taking the observations was to measure the comet's distances from two known and properly chosen stars, and then to calculate its place.† By observing it with several pairs of stars in the course of the same evening, I found the results to agree very well, and am, therefore, inclined to place some confidence in them.

"The following are the places of the comet observed at a house in the neighbourhood of Colombo, whose latitude and longitude are respectively about 6° 54′ 59″ N.; and 80° 10′ E.

Mean Time at Greenwich, January 1845.	Comet's Right Ascension.	Comet's Declination.
d h m	h m s	° ′ ″
5 1 54	22 5 49	44 49 21 South
6 1 49	22 18 17	44 46 28
7 1 52	22 30 50	44 40 20
8 1 40	22 42 51	44 27 44
11 1 54	23 18 10	43 23 1

"I suppose that you will have seen the comet in Europe before this reaches you, as the declination is diminishing very fast. It is, however, rapidly decreasing in brilliancy, and just now the moon is too bright to allow of the comet being observed. I saw it, however, last night, but it was too faint to be observed with a sextant."

3. Remarks on the Comet, by J. Robinson, Esq. Communicated by the Astronomer Royal.

The comet was seen on leaving Ichaboe on the 23d of December, 1844, and its nucleus appeared like a star of the second magnitude, with an immense tail, bearing W.S.W. by compass. It continued to be seen on every fine evening. On the 6th of January, 1845, in longitude 6° 20′ W., and latitude 8° 30′ S., it was seen in great brilliancy, the tail being vertical, and the head downwards. The length of the tail was 8° 20′. It was seen for the last time on February 2.

4. Remarks on the Comet. By J. J. Waterston, Esq. Accompanied by a Chart, marking its course among the Stars. Including also, Observations of the Comet, made at the Madras Observatory, by T. G. Taylor, Esq. Communicated by Captain Beaufort, R.N.

"*Bombay, January* 31, 1845.

"The new year has brought us a celestial visitor which, by its splendid appearance in our southern sky, reminds us of the great

* An instrument of 10 inches radius, reading to 10″ by Troughton.

† It is desirable that the measures of distance should be communicated to the Society.—*Sec.*

comet of March 1843. There is the same disproportion between the tail and nucleus, but it is altogether on a reduced scale, the tail being only 10° long when first seen, and the nucleus shining with the brightness of a star of the fifth magnitude. The great comet of 1843, when first seen here, was visible in the midst of bright twilight, half an hour after sunset. It shone with the dull, red light of an ignited coal, and the immense tail seemed like a dense, white cloud, springing up from the horizon, sharply defined for about 20°, and in a few days it extended to 43°. After a few days, the head of the present comet could not be recognised by the naked eye, but the large tail continued to be a conspicuous object until the increasing moonlight overpowered it. After full-moon, it could still be recognised as a faint streak of light, until this evening, when it was only visible through a telescope.

" In the accompanying chart, I have traced its motion with the assistance of the *Madras Catalogue of Southern Stars*, and made two attempts to compute the parabolic elements, but the positions seem too loosely fixed to obtain them with any accuracy. The second trial ought, nevertheless, to be an approximation, as it includes a sweep of twenty-one days, and the utmost error of the observations cannot exceed 5′.

" I have since been favoured by Mr. Taylor with a copy of his observations up to the 17th January, which I annex, but have not had time to compute an orbit from them.

*Observations of the Comet made at the Madras Observatory.**

Day.	Madras Mean Time.	Right Ascension.	North Polar Distance.
1845.	h m s	h m s	° ′ ″
Jan. 5	6 47 31	22 5 7	134 49 10
6	6 48 33	22 18 6	134 49 20
7	6 53 13	22 30 35	134 42 45
8	6 52 15	22 42 38	134 27 39
9	6 46 23	22 54 31	134 12 16
10	6 46 4	23 6 25	133 50 29
11	6 51 23	23 17 45	133 24 15
12	6 42 27	23 28 46	132 53 54
13	7 0 17	23 39 38	132 21 49
14	6 50 23	23 49 52	131 43 40
15	6 47 12	23 59 42	131 6 39
16	6 35 14	0 9 4	130 24 30
17	6 50 54	0 18 29	129 40 57

(Signed) T. G. Taylor.

* The irregularity of the differences of the North Polar Distance seems to denote that some of the results are erroneous, probably through errors of transcribing.—*Sec.*

"These supersede the chart; but it may still serve as a picture to shew a singular luminous appendage, which, on the evening of the 16th, I observed for the first time to proceed from the head of the comet *towards* the sun almost diametrically opposite to the proper tail. It consisted of a narrow band of faint light of about the same breadth as the head. The edges were well defined and parallel. It could be traced for 3°, and probably extended much farther, as the increasing moonlight was very unfavourable to so faint an object. Its direction and appearance, when first seen on the 16th, is shewn on the chart. Another representation of it is given, on a large scale, as it appeared on the evening of the 25th. The two tails now made an evident angle, and the space was filled with a diffused, irregular light, giving a triangular shape to the comet when seen by the naked eye. The direction of the tails, in reference to the adjacent stars, has been carefully set down on the chart. This evening the same appearance continues, but very faint, the angle at the head of the comet being about 140°. Mr. Milne, in his essay on comets, states, that in the comet of 1824, the same kind of double tail was observed. It would be interesting to know if there is any chance of their being the same. A newspaper correspondent here remarks, that there is a considerable resemblance in the elements (except the perihelion distance) to the comet of 1737 (Delambre's *Astronomy*). The comet of 1557, which is expected to return in 1848, is also not very widely different in its elements.

"J. J. WATERSTON."

5. Observations made at the Observatory of Georgetown, Demerara. Communicated by Lord Stanley through the Astronomer Royal.

"*Demerara Observatory, 18th January, 1845.*

"*Observations of the Comet.*

"On the 26th December, 1844, about seven o'clock P.M., discovered a comet about 5° above the south-west horizon. Not having any instruments at hand except a compass, took its bearings, and found it to be about 40° south declination.

"From this date to the 8th January, 1845, could not obtain an observation, from the density of the atmosphere.

"On the 8th January, found, by observation, its position to be in the east wing of *Grus* or *Crane*. About 12° in an easterly direction from δ *Gruis*, and 4° distance from γ *Gruis*.

"On the 12th January, the tail of the comet extended a little to the west of the star γ *Phenicis*, and was about 7° in length; the nucleus was nearly in a line with the stars ι and γ in the same constellation, and, although not well defined, appeared equal in size to a star of the fourth magnitude.

"On the 15th January, found, by observation, that it was going in nearly a direct line to γ *Phenicis*, and was distant about 5° from ι in the same constellation, bearing north.

"On the 16th and 17th instant, its appearance was so very indistinct, that no accurate observations could be obtained; and as

it has been apparently fast receding from the sun since the 12th instant, no doubt it will soon disappear altogether.

"From observing the distances of the four fixed stars, viz. *Sirius*, *Aldebaran*, *Achernar*, and *Fomalhaut*, from the nucleus, found its right ascension and declination to be as follows, at about $7^h\ 30^m$ mean-time, each evening :—

Day.	Right Ascension.	South Declination.
1845.	h m	° ′
Jan. 8	22 10	44 00
10	22 32	44 05
12	23 05	44 15
15	23 30	44 30

"JAMES DONALD,
"W. WILSON."

V. Observations of Distances of the Great Comet of 1843, from known Stars, made at Port Essington, by Sir Everard Home, and Mr. Brown, Master of her Majesty's Ship Alligator. Communicated by Captain Beaufort, R.N.

VI. Description of a Method of using Scales constructed for the Prediction of Occultations. By J. J. Waterston, Esq. Communicated by Captain Beaufort, R.N.

VII. Observations of the Second Comet of Mauvais, accompanied by a Chart of its Progress among the Stars. By J. J. Waterston, Esq. Communicated by Captain Beaufort, R.N.

VIII. Observations and Elements of D'arrest's Comet. By C. Rumker, Esq. Communicated by Dr. Lee.

The following table contains the right ascensions and declinations of the comet resulting from the observations :—

Day.	Mean Time at Hamburg.	Apparent Right Ascension of Comet.	Apparent North Declination of Comet.	No. of Obser.
1845.	h m s	° ′ ″	° ′ ″	
Jan. 3	7 45 3	292 34 1·5	38 35 17·2	15
10	7 45 54	290 5 18·5	41 30 37·7	14
11	8 16 5	289 38 38·3	41 57 51·1	2
,,	16 11 23	289 30 31·5	42 6 45·5	12
12	7 32 5	289 12 53·8	42 24 14·9	10

From an observation made at Berlin, on December 28, and observations at Hamburg, on January 3 and 10, Mr. Rumker has computed the following elements :—

Perihelion Passage, Jan. 8, 2388752, Greenwich Mean Time.

Longitude of perihelion	91° 21′ 29″
Longitude of ascending node	337 7 37
Inclination	47 4 21
Logarithm of perihelion distance	9·95756

Motion direct.

IX. Correction of the Longitude of the Observatory of Hamburg, by Observations of Moon-Culminating Stars. By C. Rumker, Esq. Communicated by Dr. Lee.

The author compares the observations of the moon and moon-culminating stars, made at Hamburg, with those made at Greenwich and Edinburgh in 1839, and at Berlin in 1840. He assumes the longitude of Edinburgh to be $+12^{m}\ 43^{s}\cdot6$; that of Berlin to be $-53^{m}\ 35^{s}\cdot5$; and that of Hamburg to be $-39^{m}\ 54^{s}\cdot0$ west of Greenwich; and he obtains for the correction of this assumed longitude of Hamburg, $-0^{s}\cdot054$, whence the corrected longitude of Hamburg results $-39^{m}\ 54^{s}\cdot054$.

X. Observations of D'arrest's Comet. By R. Snow, Esq.

The equatoreal observations made by Mr. Snow are approximate.

On February 5, the right ascension of the comet was observed with the transit instrument, by marking the time of its entrance into and departure from the field, and of its estimated passage across the centre. The corrected sidereal time of its passage across the meridian, below the pole, was $5^{h}\ 6^{m}\ 40^{s}$.

On February 6, it was again observed with the transit instrument, and the resulting right ascension was $16^{h}\ 50^{m}\ 12^{s}$.

Before the conclusion of the meeting, it was proposed by Mr. Sheepshanks, seconded by Professor Powell, and unanimously resolved,

"That the Society gives its hearty thanks to Captain Smyth for his valuable present of the original entries of the observations on which the *Bedford Catalogue* is founded."

It was also proposed by Mr. Sheepshanks, seconded by Mr. Galloway, and resolved unanimously,

"That the thanks of the Society be given to Mr. Turnor for his valuable present of two volumes of Ancient MS. Astronomical Tables."

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ROYAL ASTRONOMICAL SOCIETY.

Vol. VI. *April* 11, 1845. No. 15.

Captain W. H. Smyth, R.N., President, in the Chair.

William Wakeling Boreham, Esq. of Haverhill, Suffolk, and William Peters, Esq. of Beckenham Place, Kent, were balloted for, and duly elected Fellows of the Society.

The following communications were read:—

I. On the Longitude of Paramatta. By C. Rumker, Esq. Communicated by Dr. Lee.

The object of this communication is to shew that the method of determining the longitude by means of moon-culminating stars can be extended to large meridional differences. The observations of the moon and moon-culminating stars made at Paramatta are compared with those made at Greenwich, Paris, Abo, and Konigsberg, and the results have proved satisfactory. The correction to the assumed east longitude, $10^h\ 4^m\ 6^s{\cdot}25$, from the mean of all the observations, is $+\ 0^s{\cdot}967$; whence the resulting longitude is $10^h\ 4^m\ 7^s{\cdot}217$.

II. Observations of the Great Comet of 1844–5, made at the Cape of Good Hope, by T. Maclear, Esq., extracted from a letter to the Astronomer Royal. Communicated by the Astronomer Royal.

"*Cape Point Station, January* 3, 1845.

"Another splendid comet ornaments our western horizon, first seen by Captain Wilmot from Sea Point, west of the ***Lion's Rump***, on the 19th of December: it set at the same time by estimation with *Mercury*.

"It was seen on the same evening by myself from the station, the tail shooting above the edge of a nimbus or cloud-bank, but I doubted whether it might not be one of those beams that occasionally appear from behind a cloud with the setting sun. Foul weather prevailed until the 24th, when we had a short glimpse of it from the observatory over a south-eastern cloud upon the Devil's Berg. Christmas evening cloudy—rain on the 26th. I have since observed it here when practicable with the Beaufort theodolite, the only

instrument I can command. But I have received the following observations from Mr. Mann, made at the observatory.

Cape Mean Time.	Approximate Places. Right Ascension.	Declination.
d h m	h m s	° ′
1844, Dec. 24 8 20	19 43 30	38 18 South
27 8 20	20 16 44	41 3
28 8 30	20 28 37	41 51
29 9 23	20 40 42	42 32

"My observations here are differences in altitude and azimuth from α *Gruis* and Fomalhaut.

"On the 24th the tail was about 7° long, and seemed to be parallel to the equator. On the 28th I measured the length, and found it to be 8°—brushy and slightly curved (sword shape) towards the north. Difference in azimuth of head and tail 1° 45′, the head being most southerly, at 4^h Cape sidereal time.

"January 1, the tail is parallel with the arc of a great circle joining α and β *Gruis*, and extends to midway between these stars, being seemingly about 9° in length. The head is bright, and the general outline more sharp and clear than the great comet of March, 1843."

III. Results of Observations of De Vico's Second Comet, and of the Great Comet of 1844–5, extracted from a letter to the Astronomer Royal, from Professor Schumacher.

1. Observations at Rome.

	Mean Time.	Right Ascension.	Declination.
	d h m s	h m s	° ′ ″
Feb.	25 11 15 39·2	11 44 2·20	+55 5 8·5
	26 7 2 57·6	11 38 8·4	54 46 43·5

2. Observations at Paris.

	d h m s	° ′ ″	° ′ ″
March	6 12 36 33	158 28 18·6	+49 6 41.3
	7 8 28 41	156 51 17·2	48 15 10·4
	7 8 54 13	156 49 8·5	48 14 2·7
	8 8 0 55	154 57 31·4	47 9 57·7

Mr. Faye has calculated from these observations the following elements.

Perihelion Passage, 1845, April 12·85112, Paris Mean Time.

	° ′ ″	
Perihelion ...	191 53 27	Mean Equinox, Jan. 1. Parallax and
☊	346 59 41	aberration *not* taken in account.
Inclination......	56 45 44	
Log. q............	0·1015077.	Motion direct.

The middle observation is represented to — 2″·7 in long. } Calcul.—observ.
——— + 1·5 latit. }

3. Observations made at Hamburg.

d	h m s	° ′ ″	° ′ ″
March 10	10 13 43·5	151 2 20·7	+44 36 25
12	8 34 8·4	147 33 20·2	41 54 23·6
14	8 43 19·6	144 8 25·9	38 48 54·5

4. Observations made at Altona.

These observations are made with the meridian circle. I have put a small micrometer in the field of the telescope, which requires no illumination.

	d h m s	° ′ ″	° ′ ″
March	12* 10 27 10	147 24 51	+41 47 33
	13 10 16 25	145 42 0	40 17 37
	14† 10 5 54	144 2 33	38 43 37
	15‡ 9 55 36	142 26 34	37 5 44

March 16. The sky was covered.

March 18. I have to-day received a letter from Mr. Encke, which gives some more observations of the comet, recently observed at Naples.

	Berlin Mean Time.	Right Ascension.	Declination.
	d h m s	° ′ ″	° ′ ″
March	5 7 55 28·1	53 57 7·4	−9 48 36·6
	8 7 30 39·6	55 29 47·4	−8 43 45·9
	9 7 52 29·4	56 0 38·8	−8 22 11·4

From the observations, Feb. 7, Feb. 25, and March 5, he has calculated the following elements,—

Perihelion Passage, 1844, December 13·84846, Berlin Mean Time.

	° ′ ″	
Perihelion	295 45 41·7	Mean equinox, Jan. 0.
☊	118 21 6·6	
Inclination	45 37 50·4	
Log. q	9·395670.	Motion direct.

The middle observation is represented to $-$ 26″·6 in R.A. } Calcul.—observ.
$+$ 14·3 in decl. }

———

Since March 9, Mr. Encke has not seen this comet, even with his powerful telescope.

III. Elements of De Vico's Second Comet by the pupils of the Observatory at Naples, extracted from a letter to the Astronomer Royal, from E. Cooper, Esq. Communicated by the Astronomer Royal.

Perihelion Passage, 1845, April 21·398, Mean Time, Naples.

	° ′ ″
Longitude of perihelion	194 22 20
Longitude of ascending node............	347 31 30
Inclination	55 10 34
Perihelion distance	1·23018

Motion direct.

IV. Observations of the great Comet of 1844-5, made at the Observatory of Trevandrum, by J. Caldecott, Esq., Director of the Observatory. Communicated in a letter to the Secretary.

* The great cold ($-$ 14° Réaum.) and the east wind made the comet and stars tremulous.
† Good observations: the nucleus visible.
‡ Still better observations. The nucleus very distinct, of about 15″ diameter.

"*Trevandrum Observatory, 6th February*, 1845.

"Dear Sir,—I have the pleasure to send you, for the information of the Royal Astronomical Society, a list of observations which I have taken of a comet which was first seen at this observatory on the 30th of December, at 7 P.M. On that evening it was very bright for the short time it was seen before being covered by clouds, and must have evidently been conspicuous for several evenings earlier at places having a clear sky: here a cloudy state of the atmosphere prevailed for some days before and after that date; and it was not until the 8th of January that I was able to obtain observations of it. Since then I have observed it (in sets of 4 or 6 observations) nearly every evening with the 7-feet equatoreal of this observatory, fitted with a reticulated micrometer of gold wire. The instrumental corrections have been obtained principally by observations of α *Gruis* at about the same hour-angle; and as the instrument (mounted on pillars of granite) is very permanent in its adjustments, I consider all the observations (except those for the last three or four days) may be depended on to within 1^s of R.A. and $10''$ of declination. They are corrected for refraction.

"From the observations of the 9th, 14th, and 19th of January, I have calculated roughly (*i. e.* without correcting the observations for parallax and aberration) the approximate parabolic elements as follows:—

	Trevandrum Mean Time.
Time of perihelion passage, December	13·5606
Logarithm of perihelion distance	9·4325438 ($q = 0·270735$)
Longitude of perihelion (on the orbit)	$298^\circ\ 51'\ 28''$
Longitude of the ascending node	118 31 35
Inclination	45 33 46

Motion direct.

"The comet, when first observed, had a very distinct nucleus, resembling a star of the fifth magnitude, seen through a thin haze. The tail on the 9th of January I ascertained, by measurement with a sextant, to be about 7° long and $1\frac{1}{4}^\circ$ broad, at its broadest part, which was at about $\frac{1}{3}$ of its length from the nucleus. After that date it gradually diminished in length and brightness, both by reason of its own diminishing light and of the increasing moonlight, and for the last week the whole has disappeared to the naked eye; the head is, however, still seen in the telescope as a blotch of light, which will scarcely bear the least illumination of the field (even through a red glass) for observation.—I am, &c.

"JOHN CALDECOTT."

Places of the Comet of 1844-5.

Date. 1845.	R.A.	N.P.D.	Remarks.
d	h m s	° ′ ″	
Jan. 8·29650	22 42 58·54	134 29 41	
9·29491	22 54 57·26	134 11 28	
10·29780	23 6 37·40	133 51 1	
11·28721	23 17 53·14	133 25 6	
12·28729	23 28 57·18	132 55 13	The instrumental corrections were obtained nearly every day by observations of α *Gruis* in the afternoon.
13. Observations prevented by clouds.			
14·28895	23 49 57·38	131 45 28	
15·29545	23 59 57·59	131 6 11	
16·30384	0 9 28·71	130 24 23	
17·29724	0 18 40·30	129 41 5	
18·29307	0 27 26·00	128 56 13	
19·29585	0 35 50·36	128 9 16	
20·29177	0 43 50·76	127 21 32	
21·31826	0 51 44·16	126 31 46	
22·29709	0 58 56·80	125 44 10	
23·29344	1 5 57·00	124 55 17	
24. Observations prevented by rain.			
25·30090	1 19 12·0	123 16 0	
26·29747	1 25 20·2	122 26 41	Comet very faint.
27·30638	1 31 21·8	121 37 6	One observation only.
28·30539	1 37 01·0	120 48 53	
29·31184	1 42 27·8	120 0 23	
30·29529	1 47 36·2	119 13 5	
31. Observations prevented by clouds and rain.			
Feb. 1·36330	1 57 49·4	117 36 50	
2. Observations prevented by clouds.			Comet very faint, and observations very difficult.
3·33561	2 6 36·2	116 8 44	
4·32288	2 11 3·6	115 24 57	
5·32020	2 15 13·0	114 41 30	

Notes.

Jan. 8. Nucleus distinct, and like a star of the 5th magnitude.

9. A star of the 7th magnitude in field with comet, $4\frac{1}{2}$ squares* to the apparent south (real north), and preceding comet by $18^{s}\cdot2$.

The Tail measures 7° long, $1\frac{1}{2}$° broad.

* One square of this micrometer is = 3′ 50″·8 of arc.

Jan. 12. A small star, 8th magnitude, precedes comet 18ˢ, and from 5″ to 10″ to apparent south of it.
14. A star, 7th magnitude, not quite 1 square to apparent south, preceding comet 6ᵐ 11ˢ.
18. A star, 7th magnitude, in field with comet, preceding it 21ˢ, and 1 square to apparent south.
25. A star, 6th magnitude and of red colour in field with comet, 1⅛ squares to apparent north, and following it 23ˢ·7.
28. A green field found favourable to vision of the comet and wires.
30. A red field, still better.
Feb. 4. Observations very difficult.
5. Ditto ditto.

V. Observations of Mauvais' second Comet (3d series), made at the Royal Observatory, Cape of Good Hope, under the direction of T. Maclear, Esq. Communicated by the Astronomer Royal.

Day.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1844.		h m s	m s		h m s	′ ″	
Dec. 2	23	14 38 29·0	−2 3·48	12	14 38 29·0	− 2 37·0	12
4	24	14 12 24·2	+0 19·38	6	14 23 35·2	−19 33·8	10
		14 37 23·3	+0 10·51	6			
	25	14 47 34·2	−0 35·21	10			
5	26	14 12 5·5	+2 20·86	14	14 12 5·5	+ 7 42·7	14
7	27	13 34 40·9	+1 5·35	10	13 34 40·9	+ 2 8·7	10
	28	13 53 9·7	−0 7·45	6	14 1 45·7	+ 5 23·9	8
8	29	13 11 55·3	−0 15·24	10	13 24 39·7	+ 0 44·4	10
		13 34 22·3	−0 4·33	7			
12	30	11 58 38·6	+1 1·27	3	11 58 38·6	−22 19·2	3
13	31	10 56 28·8	+1 38·82	5	11 8 46·8	+ 0 28·9	10
		11 29 3·9	+1 16·14	8			
15	32	9 38 10·3	−2 6·71	10	9 38 10·3	+ 1 9·8	10
		10 27 7·0	−2 45·04	10	10 27 7·0	+ 1 54·0	10
16	33	9 39 42·3	−0 26·44	10	9 56 49·3	+ 1 1·9	10
		10 12 52·2	+0 0·69	14			
18	34	9 31 8·2	−2 18·45	8	9 31 8·2	+ 3 47·4	8

The sign + denotes the comet's right ascension or declination to be greater than the star's; the sign — the contrary.

The above observations were made with a spider-line position micrometer applied to Dollond's 46-inch achromatic, and with a power of 43.

Approximate Places of the Stars observed with Mauvais' second Comet.

No. of Star.	Magnitude.	Right Ascension.	Declination.	Remarks.
		h m s	° '	
23	8	11 2 53	−53 52·6	No. (23) precedes π *Centauri* 11m 5s, and is 14' 3" south of it.
24	6·7	10 45 29	−56 27·1	No. (24) precedes No. (25) 41s·8, and is 2' 1"·5 south of it.
25	6	10 46 11	−56 25·1	No. (25) is 4921 of the Madras General Catalogue.
26	7	10 34 51	−57 7·6	No. (26) is the same as 4818 of the Madras General Catalogue.
27	9	10 16 45	−59 23	
28	9·10	10 17 49	−59 21	
29	8·9	10 6 33	−60 28·7	No. (29) precedes 4600 of the Madras Catalogue 5m 23s, and is 4' 52" north of it.
30	8	9 11 22	−64 23	
31	10	8 55 1	−64 40	
32	5	8 24 2	−65 37	No. (32) is β *Piscis Volantis*.
33	9	8 2 38	−65 52	

VI. On the Use of a new Micrometer, and its Application to the Determination of the Parallax of *Mars* at his ensuing Opposition. By M. Boguslawski. Communicated, with a Letter to the President, from Sir John Herschel.

This communication is introduced by the following letter from Sir John Herschel:—

"*Collingwood, 30th March, 1845.*

"My dear Sir,—I request permission to lay before the Astronomical Society the accompanying communication from M. Boguslawski, just received, in which he describes a new micrometer, and urges its application to the determination of the parallax of *Mars* at his ensuing opposition.

"M. Boguslawski, as will be observed, requests me to take on myself the sole charge of directing and arranging the observations he recommends. My engagements utterly preclude the possibility of my undertaking this duty, or any part of it. Nevertheless, being willing and desirous, so far as I am able, to forward this or any other well-recommended scientific object, I have considered it, on the whole, as my duty to forward his letter where it is sure to receive every due consideration; leaving it to the Council, if it should seem of sufficient importance, to make any arrangements they may see fit for meeting the end in view.

"I ought to add that I have myself had in use, for some time during my observations at the Cape, a process of micrometrical measurement by means of the transit of stars over the moveable wire of a position micrometer, set at a given angle of inclination to the parallel, which I have designated as *the method of oblique transits* for the purpose of determining the distances of double stars,

which appears to me nearly or quite identical in principle with this of M. Boguslawski.

"An account of this method, and the results obtained by its use, have been for some time written for the press, and will form part of my forthcoming observations in the southern hemisphere.—I remain, my dear sir, &c.

"J. F. W. Herschel."

The author states that he intends shortly to publish a detailed account of the construction and use of this micrometer, for observing differences of right ascensions and declinations of stars. Its principal peculiarity consists in this, that its scale is not dependent upon the individual telescope employed, and is therefore independent of the optical power of it, except only as far as the distinctness and precision of the images are concerned. An account of some observations made with it will be found in some of the forthcoming numbers of the *Astronomische Nachrichten*. It consists simply of one *wire*, *thread*, or *lamina*, which is placed in the common focus of the object-glass and eye-piece of a telescope, as a diameter across the field of view, in such a manner as to turn round the centre in every direction, and to make with the declination circle any given angle. The author then proceeds to shew that if τ^0, τ', and T, be the times of passage across the wire of three stars (the first two being known, and the third being unknown or its position required), whose right ascensions are α^0, α', and A, and declinations δ^0, δ', and D, for any given position of the wire; and if θ^0, θ', and Θ, be the corresponding times of transit of the same three objects for any other position transverse to the former, then

$$A-\alpha^0=T-\tau^0+\left[(\alpha'-\alpha^0)-(\tau'-\tau^0).\right]\frac{(\Theta-\theta^0)-(T-\tau^0)}{(\theta'-\theta^0)-(\tau'-\tau^0)}$$

$$\text{or}=\Theta-\theta^0+\left[(\alpha'-\alpha^0)-(\theta'-\theta^0).\right]\frac{(T-\tau^0)-(\Theta-\theta^0)}{(\tau'-\tau^0)-(\theta'-\theta)}$$

$$\text{and } D-\delta^0=\frac{\delta'-\delta^0}{e}.\ \frac{(\Theta-\theta^0)-(T-\tau^0)}{(\theta'-\theta^0)-(\tau'-\tau^0)}$$

$$\text{where } e=\frac{\cos.\frac{1}{2}(\delta'+\delta^0)}{\cos.\frac{1}{2}(D+\delta^0)},\ \text{and is very nearly equal to 1.}$$

When the unknown object is a planet, since its position is determined by observations made at two different times, T and Θ, a correction will be necessary for its motion during the interval of time Θ—T; and the author determines the corrections to be applied to the times Θ and T, to reduce them to the same epoch.

The author considers this micrometer, both from its simplicity and from the precision of its results, to be exceedingly well adapted for observations of *Mars* and neighbouring stars, for the purpose of determining his parallax; and he gives the following formulæ for the value of the parallax :—

Let ψ_W and ψ_E be the latitudes of two observatories favourably situated for determining the parallax by observations of right ascen-

sions, or differing considerably in longitude, ψ_w being the latitude of the western and ψ_E of the eastern station. Let also h_w and h_E be the corresponding hour-angles of *Mars*; A_w and A_E the observed right ascensions, reduced to the same instant; and D the approximate declination; then,

The horizontal parallax of *Mars*

$$=\frac{(A_E - A_W)\cos D}{\cos\psi_W \sin h_W - \cos\psi_E \sin h_E}$$

Again, if D_N and D_S denote the declinations observed at two stations differing very considerably in latitude (N denoting the northerly and S the southerly station, and the rest of the notation being suitably changed),

The horizontal parallax

$$=\frac{(D_S - D_N)\,\text{Sec.}\,D}{\sin\psi_N - \sin\psi_S + \tan D\cos\psi_S\cos h_S - \tan D\cos\psi_N\cos h_N}$$

This communication is accompanied by a list of stars selected by the author as favourably situated for observation at the ensuing opposition of *Mars*; the list includes many of those set down for a similar purpose in the *Nautical Almanac* for 1845.

VII. On the almost total disappearance of the earliest Trigonometrical Canon. By A. De Morgan, Esq.

I lately found in a second-hand book-shop a trigonometrical canon, by the celebrated Rheticus, which was totally new to me, and, as I afterwards found, would have been just as new to any of the historians of astronomy and mathematics. I am therefore induced to make a communication on the subject to the Astronomical Society, which, more than any other, has a right to take interest in the history of tables for the facilitation of the application of arithmetic to geometry.

The paper which I now read is a remote consequence of the proscription of Copernican opinions by the Inquisition. No follower of Copernicus was more zealous or more plain-spoken than Rheticus or George Joachim of Rhætia. The master, whether from conviction or policy, called his theory no more than an hypothesis for the explanation of the planetary motions: the pupil strongly insisted on the absolute physical truth of the motion of the earth. Both were gathered to their fathers long before the storm arose: but long before that time arrived, there was a distinction drawn between the treatment of the two. In the *Index Expurgatorius*, it is not Copernicus who is forbidden to be read generally; the prohibition only extends to the work *de Revolutionibus*, and is accompanied with a *nisi corrigatur*. But Rheticus is wholly forbidden to be read in any of his works: nay, in Sotomaier's folio edition of the *Index* (Madrid, 1667) even the *Opus Palatinum*, though all but the very table is the work of Valentine Otho, is an unlawful book, unless on the condition that the praises of Rheticus in the preface be erased or passed over. And Riccioli tells us that

the condemnation of the books of Rheticus took place in 1550, which is the year before the canon I intend to describe was published. The extreme severity with which his writings were treated will appear less remarkable if we remember that he held the chair of mathematics at Wittemberg, in which Luther had very lately taught theology, and burned the pope's bull, and in which Melancthon actually taught Greek. Riccioli very gravely informs us that Rheticus received a supernatural punishment for his presumption. While he was puzzling himself about the motion of Mars, he invoked his genius or guardian angel to help him out of the difficulty: the angel accordingly lifted him up by the hair of his head to the roof, and threw him down upon the pavement, saying with a bitter laugh, "That's the way Mars moves." Kepler, it seems, had heard this story with such evidence as induced him to suppose that Rheticus must have knocked his head against the wall in the agitation of his spirits. Riccioli does not feel himself at liberty to make any other mention of him, and adds *damnatus auctor* to his name in a list in which Copernicus himself figures without remark: and Clavius, in the diffuse trigonometry attached to the edition of Theodosius, does not even introduce his name. The Jesuit Blancanus also, in his list of mathematicians, excludes Rheticus altogether, though he was a contemporary of the *Opus Palatinum*, and though he admits Copernicus.

The works of Rheticus are accordingly very scarce; and it can be no matter of surprise that one of them, and perhaps more, should have entirely slipped out of notice. Even the second edition of Copernicus, Basle, 1566, which was edited by Rheticus, is much more scarce than the first.

The early history of trigonometrical tables, as given, may be thus summed up. Tables of sines, which were substituted for Ptolemy's chords by Albategnius, were published, to every minute, from Regiomontanus* (who died in 1476), and also by Apian, both in 1533. Regiomontanus used tangents, as the Arabs had done before him, and there is a table, to degrees only, in a work of his published by Reinhold in 1561. This same Erasmus Reinhold had printed tangents of every minute, in his *Liber Tabularum Directionum*, published in 1554, three years after the Prutenic tables, on which his fame principally rests. Regiomontanus had called

* Such is the statement: but on examining the work on triangles by Regiomontanus, in which these sines are said to occur, and which was published in 1533, I cannot find any table at all. But Apian published *two* tables of sines: one in the *Introductio Geographica*, 1533, and another in the *Instrumentum primi mobilis*, 1534. There is a table of sines in the *Tabulæ Directionum*, 1552, and another in the *second* edition of the work *De Triangulis*, Basle, 1561, folio. And in the catalogue of De Thou's library, and in Niceron's list, Murhard's catalogue, and Kästner's history, is mentioned the following work, "Joh. Regiomontani compositio tabularum Sinuum, cum tabulis duplicibus Sinuum ejusdem," folio, Nuremberg, 1541. All the writers who assert that sines of Regiomontanus were published before those of Apian, go on the assumption that what is in the second edition of the work on triangles is also in the first, which assumption is not true. Lalande gives a wrong date, place, and title to this first edition.

the table of tangents *canon fœcundus*, which name Reinhold adopted. As to secants, the table of which was afterwards called *canon fœcundissimus*, a name which I think first occurs in Vieta, the first table mentioned is that of Maurolycus in the spherical treatise which accompanies his edition of Theodosius and Menelaus, published in 1558; this table goes only to degrees, and is called *tabula benefica*. The *Canon Mathematicus* of Vieta, published in 1579, is said to have been the first work in which sines, tangents, and secants, were joined together; that is, the first complete canon printed. But all admit that Rheticus, who died in 1576, had very nearly completed the enormous table which Valentine Otho published in 1596, under the name of *Opus Palatinum*. Still, the rigid rule is, that first publication gives a right which nothing but unquestionable proof of fraud can impugn; and accordingly Vieta has been justly considered, up to this time, as the first author of a complete canon. I intend to shew, however, that under the same rule, Rheticus is not only the first who published either tangents or secants,* but the first who joined the three into a complete canon, and also the first who adopted the now universal semi-quadrantal form. Before, however, I come to the description of the table which establishes these things, I shall shew that it once had a recognised existence. It is not enough, if better may be, to produce a printed book as the sole evidence of the fact of publication. There may have been a suppressed edition, or one accidentally destroyed by fire, and of which only a few copies escaped: the forgery of a work is neither impossible nor unexampled, and more than one big catalogue consists entirely of pseudonymous works.

Lansberg attributes the first publication of tangents, and Bossut the first publication of secants, to Rheticus: on what authorities I do not know. Moestlinus, in a letter written to Kepler in 1594, and published in the folio correspondence of the latter, expresses a great desire to see a certain little book of Rheticus, the title of which he does not mention. In a subsequent letter, of 1595, he reports that he has seen this book, that it treats only of plane triangles, and that it strives at (*nititur*) the canon of triangles which Rheticus afterwards (*olim*) published. What can this *canon of triangles* be? At the date of this last letter, the *Opus Palatinum* was not published. It can refer to nothing but the work which I now bring forward, and which, it thus appears, was known. The next witness

* Montucla gives the secants to Rheticus, and Delambre (*Astron. Mod.* ii. 34) seems to assent, because Maurolycus only published them to every degree. This is hardly fair: a person who points out the uses of a given function, and tabulates it to a certain extent, is the inventor, and must not lose his right because another gives more and better tabulation. But we now see that Rheticus has a claim absolutely prior to that of Maurolycus. Again, Delambre says that a certain Finckius gave secants to minutes, in 1583, referring to Rheticus: he seems to imply that Finckius had access to the materials of the *Opus Palatinum*, not then published. Perhaps it may now be held that the ten-minute canon described in the present article was the original, the intermediate minutes being supplied by interpolation.

I shall cite is Thomas Digges, who is likely to have taken a particular interest in the writings of his fellow Copernican. In the *Alæ seu Scalæ Mathematicæ*, published in 1573, during the life of Rheticus, Digges says that those who dislike labour should consult the tables of the proportions of right-angled triangles by Rheticus, of which tables he further states that they go to every ten minutes.

Valentine Otho, in the preface to the *Opus Palatinum*, says that "when Rheticus gave to the public a specimen of his method of enriching the canon and doctrine of triangles, he excited a wonderful degree of hope and expectation about it in the minds of the learned, especially when in the dialogue which he prefixed to his ten-minute canon, he brought forward extraordinary and almost incredible things concerning its use." I produce to the Society the ten-minute canon with the dialogue, which however is not prefixed to the table, but follows it. Otho afterwards mentions this dialogue again, and says that on reading it he was so struck with the pretensions of its author that he sought out Rheticus in Hungary, and commenced the acquaintance which led to his becoming the editor of his friend's posthumous work. He further adds, that at their first meeting, when he had just stated that he was come to acquire knowledge on the properties of triangles, Rheticus interrupted him with, "You are just as old as I was when I went on the same errand to Copernicus." Thus it appears that this canon was the indirect cause of the publication of the *Opus Palatinum*.

But nevertheless, this work is not mentioned in the catalogues of Lipenius, Dechâles, or Murhard; it is not alluded to either by Riccioli, Clavius, Gassendi, Weidler, Heilbronner, Delambre, Montucla, Hutton, or Kästner. Delambre distinctly says he never heard of any canon containing sines, tangents, and secants, previous to that of Vieta: though elsewhere he describes the preceding passage from Otho by saying that Rheticus had published a programme of the *Opus Palatinum*, and even an extract for every ten minutes. But he then quotes the account of Lalande, to which I shall immediately come, and drops the matter, as if declining to decide the point. Kästner quotes the passage from Otho which I have translated above, and makes a separate head of it, in his abstract of the preface of the *Opus Palatinum*; he does not give his usual short comment, and evidently leaves it to the reader without knowing what to make of it.

That Weidler should have been utterly ignorant of this work is rather a striking proof of the complete oblivion into which it had fallen. For Rheticus (with Reinhold) professed mathematics in the University of Wittemberg, after having taken degrees there; and Weidler was not only of this university, but wrote his history of astronomy, and printed it, at Wittemberg. Consequently he had access to the *matricula*, or register-book, and to all other records; so that he is able to give several minute particulars of the literary life of Rheticus, which another writer could hardly have obtained. But not even at Wittemberg did any tradition exist of the work on which this paper is written. It was printed, certainly, at Leipsic,

and on the residence of Rheticus in this latter university, Weidler can say nothing more than that Rheticus is reported to have taught there. It is worth noting that, next to Wittemberg, Leipsic was the university most obnoxious to the adherents of the old church.

Lalande actually possessed a copy of a reprint of this same canon, published (he says) in 1580, and has given the title-page with perfect correctness, in his short description of the *Opus Palatinum*, contained in the *Bibliographie Astronomique*. But, with a negligence which is unusual in his bibliographical accounts, he represents it as a canon for the first forty-five minutes only, to every ten seconds; a kind of extract from the forthcoming *Opus Palatinum*. Had he looked more closely, he would have seen that his *minutes are degrees, and his seconds minutes*; and he would have seen the remaining 45 degrees rising in the reverse direction on the opposite side of the page. Perhaps Lalande could not imagine the possibility of the calculator of the immense *Opus Palatinum* publishing a table to every ten minutes only.

Murhard has this reprint, Basle, 1580, in his list, but not the original work, and it is also in the catalogue of De Thou's library. The title is to be found in Teissier's *Eloges des Hommes Savans*, from whence it is copied into a work in which we should hardly have looked for mathematical treatises which are unknown to mathematicians, Gorton's *Biographical Dictionary*. The library of the British Museum, which is unusually rich in the mathematical works of the nineteenth century, has both the original work and the reprint. But I do not find any date to the reprint, nor do I know from whence Murhard and Lalande got theirs. The copy of the original edition now in my possession probably escaped the Inquisition from the accident of its being bound up with the *editio princeps* of the Greek text of the optical writings of Euclid, published six years after it.

We thus see that the existence of a work may be forgotten, and the fair claims of its author reduced in amount, by the neglect of a biographer in stating precisely its title, date, place, and form; and also that it may be possible, partially at least, to repair the neglect, by collection of scattered notices. We also see that the publication of proper catalogues of our libraries would tend to promote historical knowledge. And while on this subject, I trust it is not out of place to make the following remark. I very much fear that the publication of a good alphabetical catalogue of the splendid library in the British Museum is retarded by the demand which has often been made for a *classed catalogue*, or one arranged in order of subjects. From much, almost daily, use of catalogues for many years, I am perfectly satisfied that a classed catalogue is more difficult to use than to make. It is one man's theory of the subdivision of knowledge, and the chances are against its suiting any other man. Even if all doubtful works were entered under several different heads, the frontier of the dubious region would itself be a mere matter of doubt. I never turn from a classed catalogue to an alphabetical one without a feeling of relief and security. With the

latter I can always, by taking proper pains, make a library yield its utmost: with the former, I can never be satisfied that I have taken proper pains, until I have made it, in fact, as many different catalogues as there are different headings, with separate trouble for each. Those to whom bibliographical research is familiar know that they have much more frequently to hunt an author than a subject: they know also, that in searching for a subject, it is never safe to take another person's view, however good, of the limits of that subject, with reference to their own particular purposes.

I now proceed to the description of the canon.

The title is, "Canon doctrinæ triangulorum. Nunc primum a Georgio Joachimo Rhetico, in lucem editus, cum privilegio imperiali, Ne quis hæc intra decenniom, quacunq: forma ac compositione, edere, neue sibi uendicare aut operibus suis inserere ausit. Lipsiæ ex officina Wolphgangi Gunteri. Anno M.D.LI." quarto. The title is followed by one page of verses, fourteen of tables, and six of dialogue. In the title-page is an obelisk, with a man drawing a diagram on the base. The degrees and the sines, &c., are in black ink, the minutes and the differences are in red ink. The tables extend across the open book, the $\frac{\text{sine}}{\text{cosine}}$, $\frac{\text{cosine}}{\text{sine}}$, and $\frac{\text{secant}}{\text{cosecant}}$, being on the left; the $\frac{\text{tangent}}{\text{cotangent}}$, $\frac{\text{cosecant}}{\text{secant}}$, and $\frac{\text{cotangent}}{\text{tangent}}$, on the right. Thus at 30° 0′, we read along the double page, as follows, all in one line, differences coming before the semicolon which is here substituted for the double line in the work.

30 0 5000000 25171; 8660254 14581;
11547005 19474; 5773503 38851;
20000000 100180; 17320508 115774; 0 60.

The words sine, tangent, &c., are not used. The running title is, "Canon doctrinæ triangulorum in quo triquetri cum angulo recto in planitie partium 10000000 ponitur." This running title is not a complete sentence, the nominative of the last verb being wanted: it is completed in three different ways in the three great compartments of the table; the three nominatives are "Subtendens angulum rectum,"—"Majus latus includentium angulum rectum,"—"Minus latus includentium angulum rectum." Under the first heading, come four columns, the *perpendiculum* and its *differentia*, and the *basis* and its *differentia*. Under the second, come the *hypotenusa* and *perpendiculum*; under the third the *hypotenusa* and *basis*: all with differences. The words *perpendiculum* and *basis* are of course interchanged in the running titles at the bottom of the page, where also no other part of the running title is repeated. This system of headings is faithfully preserved in the *Opus Palatinum*.

Modern teachers of trigonometry have pretty generally abandoned the system of independent lines, which used to be called sines, tangents, &c.; and have substituted, for the meaning of these words, the ratios of the sides of right-angled triangles. It appears that they have antiquity in their favour; indeed so completely has the idea of representing the ratios of the sides of triangles taken possession of the

mind of Rheticus, that he abandons the use of the word *sine*. He dwells on the importance of the right-angled triangle, without any reference to the circle: his maxim, expressed in the dialogue, is *Triquetrum in planicie cum angulo recto, est magister Matheseos.* It would also seem as if his choice of the semiquadrantal arrangement with double descriptions was dictated merely by the convenience of heading one division with *majus latus*, and the other with *minus latus*. This is worth noting: most persons suppose that this disposition must have arisen from the circumstance of the sines and cosines of the latter half of the quadrant being only repetitions of the cosines and sines of the first half, and so on. But the very reverse is the fact; the names cosine, cotangent, and cosecant, are the consequence, not the cause, of this duplicate system of arguments. Rheticus made his arrangement with a view to separate from each other all the cases in which the greater side and the less side were data. This involved the bringing into the same line the tangent of each angle with the tangent of its complement, and the same for the secant. Completeness then required that the same should be done with the sine. The introduction of the terms, sine of the complement, complemental sine, and cosine, &c., followed after an interval of more than half a century, and was a consequence of the semiquadrantal arrangement.

The dialogue at the end is between Philomathes, a supposed friend of Rheticus, and Hospes, his pupil. The pupil asks what the intention of the book is, and is answered at length. He suggests that, perhaps, the intention may be to complete the system of Copernicus, by publishing tables from it resembling those then in use. But he is answered that Rheticus would rather that Copernicus himself had not done so much in this line, as he thereby diminished the geometrical practice of the learner. The modern astronomer, if such a one there be, whose luxurious means render him discontented whenever he has to go to the common trigonometrical tables, even of logarithms, should think of Rheticus, so well content with his intervals of ten minutes, and their differences, that he asked for nothing more, and regretted that any further help should be interposed between the observer and his wholesome exercise. I think I might have hoped for this sanction to an opinion which has been for a long time my own, namely, that the only way of learning to use a table thoroughly well, is to learn to do without it.

There is much examination yet wanted into the history of the sixteenth century. The era of logarithms, of literal algebra, and of sound mechanics, has naturally diverted attention from the day of smaller things. One question has never been properly considered: what were the immediate producing causes of that burst of successful energy which marks the first half of the seventeenth century?

The biography of Rheticus may be collected from the preface to the *Opus Palatinum*, Teissier's *Eloges des Savans*, with the references therein given, &c. He led a wandering life for a calculator of such a mass of tables, being successively with Copernicus, at Wittemberg, at Leipsic, and in Hungary. And his friend and

correspondent, Peter Ramus, informs us (*Schol. Mathem.*) that he taught at Cracow, and would have taught at Paris, but was prevented by being obliged to learn and practise medicine in the stead of a certain patron, *Mæcenatis cujusdam loco.* What this means I do not know: perhaps *loco* is a misprint for *domo*. Though Otho does not mention that Rheticus practised medicine, he to a certain extent confirms Ramus, by stating that his friend died at Cassau, in Hungary, on his way home, after being called out by a certain baron. I mention these things, because it is never stated that Rheticus was a physician. He died in 1576, in the sixty-first year of his age.

With regard to the choice of intervals of ten minutes, it may have been dictated by the existing state of astronomy; but it is more than likely that Copernicus was the suggester of the arrangement. Rheticus has preserved it as a saying of Copernicus, that if he could only succeed in giving planetary tables which should be true within ten minutes, he should feel as much gratified as Pythagoras, when he discovered the great property of the hypothenuse of a right-angled triangle.

In this communication I have confined myself to points which are either new, or very little known. I will add one more circumstance of the latter kind.

I have noted that Rheticus stands in the *Index* as *damnatus auctor*, while Copernicus is *damnati libri auctor*: a material difference. But perhaps it may suggest itself to some that Copernicus is only the writer of one work, which, being condemned, makes him a condemned author: and that it would not be thought necessary to condemn, in general terms, a writer all whose works can be prohibited under one title. But, not to dwell here upon such a supposition really implying an ignorance of the usage, it is not true that Copernicus wrote and published only one work. Though it be but little known, and not mentioned by any of the French school of historians, it is certain that Rheticus himself published (Wittemberg, 1542) in 4to. the "De lateribus et angulis triangulorum tum planorum rectilineorum, tum sphæricorum libellus," containing a table of sines to every minute, and to a radius of ten millions, or, as we should now say, to seven decimals.

This work is mentioned by Weidler, who, though of Wittemberg, had no knowledge of it till he published the supplements of his history: it is catalogued by Murhard, and described by Kästner. The great work *de Revolutionibus*, published the next year, contains a probably abridged treatise on triangles, and a certainly abridged table of sines.

In speaking of the great work of Copernicus, it should be remembered that Rheticus procured its publication, or extracted the author's consent to its appearance, as much as Halley did that of Newton in the case of the Principia; and nothing but circumstances which made it more convenient to print it at Nuremberg than at Wittemberg, prevented the name of Rheticus from appearing in the title-page as editor.

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ROYAL ASTRONOMICAL SOCIETY.

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Captain W. H. Smyth, R.N., President, in the Chair.

The following communications were read:—

I. Extract of a Letter from the President to the Secretary.

"I forward you a new epoch of γ Virginis for this apparition, thinking it may be welcome to such of our Fellows as are following up the orbit of that remarkable system, which promises, comparatively, to be to double stars what Halley's Comet is among that class of bodies. I made the observations in Dr. Lee's Observatory, at Hartwell House, with my former instruments, which are always obligingly kept in readiness for my immediate use. The mean micrometrical readings gave 174° 36′·6 for the position, which, corrected to the meridian, and brought to proportional parts, afford

Position 185° 23′·3 (w. 6); Distance 2″·10 (w. 4); Epoch 1845·34.

"The measures made on the 2d and 3d inst. merit considerable confidence, for, previously to my placing the micrometer on the objects, it was steadily viewed in an unilluminated field with a negative eye-piece magnifying 240 times, under which the minute companion following nearly on the parallel was distinctly seen. On this occasion, the wire-micrometer was under a positive eye-piece of 340 times during three sets of the measures, and once under one of 600; and the rock-crystal was applied with its power of 145, doubled by the interposition of the achromatic lens described in my 'Cycle.'"

II. On a new construction of the Divided Eye-glass Double-Image Micrometer. By G. B. Airy, Esq., Astronomer Royal.

The author remarks, that the convenience and accuracy of the double-image micrometer are so great that it is highly desirable to remove, if possible, any of the inconveniences to which it is subject. The form in which he has employed the instrument is that of a four-glass eye-piece, the second lens (as measured from the object-glass) being divided, and one or both segments being

moveable. The second lens must, therefore, be placed in such a position that every pencil of rays coming from the object-glass is equally divided upon the two segments,—a condition which is satisfied when the distance of the second lens from the first is (sensibly) the same as the focal length of the first lens. When this adjustment to focal length is perfectly made, and when also the observer has the power of securing the equal division of light upon the two segments for one pencil (and therefore for all), the use of this eye-piece is extremely convenient. Without any effort in placing the eye in a definite position, a single image, in whatever part of the field, is broken up into two of equal intensity.

The most important defect of the micrometer is this, that, unless certain conditions are respected, the images will be coloured in all parts of the field of view except the centre. This colour is not produced, like the colour in an achromatic object-glass, from the want of convergence of all the rays in one pencil to the same focal distance. It arises from a lateral chromatic disturbance of the focus of each pencil; and it may be represented most conveniently by tracing the course of the axis of each pencil, considered as a single ray subject to chromatic separation, through all the lenses to the eye, when, if they enter the pupil in any relative direction, except that of parallelism, they will produce the sensation of chromatic separation in the direction of the radius of the field of view. In this micrometer eye-piece such a defect is wholly inadmissible, and therefore the first condition is, that the eye-piece shall be (without reference to the fault described, and without regard to the separation of images,) achromatic.

The theory of achromatic eye-pieces was given by the author in the *Cambridge Transactions*, Vol. II., and the first divided eye-glass micrometers which he constructed were made of four-glass eye-pieces in which the equation of achromatism was satisfied, subject to the condition already mentioned, that the distance between the first and second lens should be equal to the focal length of the first lens. But in the use of these a new inconvenience soon manifested itself. The separation of the images is produced by so moving a segment of the divided lens that the pencils of light are received upon a part of the segment where the surfaces are inclined, so that there is introduced in this part of the eye-piece a prismatic refraction and consequently a prismatic dispersion. In general, the dispersion thus produced will not be so modified by the passage of the rays through the two remaining lenses that the rays of all colours (as separated by the motion of the segment of the second lens) will enter the eye in a state of parallelism; and, therefore, the separated images will appear coloured from this cause without respect to the part of the field of view in which they are seen.

This failing is, perhaps, more important than the other, for it affects the estimation of the scale of the micrometer as well as the ordinary use of the micrometer. The only practicable method of ascertaining the scale of the divisions is to place a wire in the meridional direction across the centre of the field; to make two

images of a single star by separation through a known number of divisions in an equatorial direction, and to observe the transit of these two images across the wire. If one or both images are coloured, so as to present the appearance of a spectrum in the direction of separation these transits are uncertain, and the estimation of the scale-divisions derived from them will be uncertain.

It was not till long after this inconvenience had been perceived that it occurred to the author that it was possible to ascertain, in an algebraic form, the theoretical equation upon which the removal of this chromatic dispersion would depend, and thereby to discover whether the two equations (of achromatism in the ordinary case of no separation of images, and achromatism as regards only the dispersion produced by the separation of images) could be satisfied simultaneously, and without disturbing the assumption that the distance between the first and second lenses should be equal to the focal length of the third lens. On forming the equations, and substituting trial numbers, he had the gratification of discovering that numbers could be found for the distances and focal lengths which are very favourable as regards the breadth of the pencil of rays where it meets each lens, and which leave the focal length of the first lens perfectly arbitrary, so that the power of the eye-piece may be changed, preserving all its properties undisturbed, by merely changing the first lens. This is the "new construction" proposed as the object of the paper.

The algebraic investigation is then given in detail; and the author, after the reading of the paper was concluded, gave an oral explanation, illustrated by diagrams, of the construction and the theory upon which it depends.

III. Communications respecting Comets.

1. Mauvais' Second Comet.

1. Observations made at the Royal Observatory, Cape of Good Hope, under the direction of T. Maclear, Esq. Communicated by the Astronomer Royal.

Date.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1844.		h m s	m s		h m s	′ ″	
Dec. 19	35	....	....	...	9 13 40·5	+ 6 31·0	11
24	36	15 22 16·9	+ 0 56·89	12	15 22 16·9	− 1 31·8	12
25	37	15 42 36·1	− 1 12·41	8	15 42 36·1	+ 7 56·9	8
27	38	15 17 55·9	+ 1 55·55	6	15 46 5·7	− 12 2·2	6
		15 34 44·7	+ 1 47·33	6			
30	39	14 59 38·6	− 1 21·34	6	15 19 42·2	+ 11 16·6	6
		15 53 47·9	− 1 42·02	6			
	40	15 9 45·2	− 1 45·45	5	15 32 39·7	− 8 11·5	6
		15 48 32·9	− 1 59·02	6			

Date.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Star and Comet.	No. of Obs.
		h m s	m s		h m s	′ ″	
1844. Dec. 31	41	14 24 46·5	− 3 22·48	12	14 47 5·2	− 2 55·1	11
		15 5 37·0	− 3 35·97	12	15 27 10·8	− 5 1·9	8
		15 42 21·7	− 3 48·50	12			
1845. Jan. 5	42	14 15 17·7	− 0 12·23	20	14 59 22·7	+ 5 18·5	6
		14 35 0·3	− 0 15·70	10	15 23 57·9	+ 4 5·2	6
	43	13 59 26·6	− 0 39·98	18	14 45 26·4	− 9 24·2	8
		14 26 30·6	− 0 45·76	10	15 13 3·2	− 10 52·2	6
9	44	13 24 12·1	+ 0 27·12	20	13 48 58·2	+ 5 39·0	20
		14 21 24·6	+ 0 18·31	20			
11	45	12 56 7·0	− 1 43·47	16	13 21 26·4	+ 0 46·7	14
		13 48 50·3	− 1 36·85	16			
12	46	12 35 26·5	− 1 25·37	16	12 59 35·4	+ 4 0·0	12
		13 25 19·1	− 1 31·23	16			
15	47	12 24 53·9	+ 1 21·53	16	12 53 17·6	− 7 55·0	16
		13 22 53·9	+ 1 16·46	16			
18	48	11 58 45·3	− 2 9·91	18	12 33 18·3	+ 1 40·5	14
		13 9 23·1	− 2 14·87	18			
24	50	10 58 23·8	− 1 31·24	18	11 20 59·4	− 10 34·4	14
		11 46 36·7	− 1 32·94	18			
25	51	10 59 51·2	+ 2 39·65	18	11 31 19·5	− 0 36·1	10
		12 2 48·6	+ 2 37·30	18			
28	52	11 7 40·1	+ 0 51·29	12	11 22 54·1	+ 6 53·2	10
		11 28 10·8	+ 0 50·29	12			
29	53	10 52 59·1	− 1 15·15	16	11 10 54·8	+ 6 24·6	12
		11 29 3·7	− 1 16·36	16			
30	54	10 53 35·0	+ 1 19·54	16	11 13 54·1	+ 7 20·5	12
		11 36 10·6	+ 1 18·25	16			
31	55	11 3 22·3	+ 4 6·18	12	11 28 23·4	+ 1 25·8	10
		11 54 19·7	+ 4 5·18	12			
Feb. 4	56	10 34 27·9	+ 0 24·85	12	10 52 21·8	− 2 23·6	6
		11 12 57·5	+ 0 24·71	12			
	57	10 34 27·9	− 0 26·53	12	10 55 56·5	+ 10 7·5	6
		11 12 57·5	− 0 26·81	12			
8	58	10 33 58·3	− 1 0·65	12	10 48 11·7	+ 2 37·4	10
		11 7 9·7	− 1 0·93	8			
9	59	10 10 51·4	+ 1 0·29	12	10 25 39·0	+ 6 36·4	10
		10 39 53·5	+ 1 0·00	12			

The sign + denotes the comet's right ascension or declination to be greater than the star's; the sign — the contrary.

The observations on December 19th, 24th, and 25th, were made with the spider-line position micrometer applied to Dollond's 46-inch achromatic, and all those on the following days, with the bar micrometer; the comet having become too faint to bear the slightest illumination.

The differences of right ascension and declination are not corrected for the effects of parallax and refraction.

Approximate Places of the Stars of Comparison.

No. of Star.	Magnitude.	Right Ascension.	Declination.	Remarks.
		h m s	° ′	
35	8	7 0 2	−65 41	* (36) follows β *Doradus* 2^{m} 44^{s}, and is 11′ 11″ north of it.
36	10	5 35 1	62 24	
37	8	5 23 23	61 13	
38	7	4 56 18	59 22	
39	3	4 30 40	55 22	* (39) is α *Doradus.*
40	8	4 31 0	55 42	
41	8	4 24 54	54 22	
42	9	3 50 58	47 58	
43	7	3 51 30	48 14	
44	8	3 33 59	43 18	
45	8·9	3 26 24	41 8	
46	10·11	3 26 46	40 0	
47	8·9	3 16 49	37 8·2	
48	8·9	3 14 44	34 6	
50	7·8	3 7 21	29 21·5	* (50) follows 12 *Eridani* (A.S.C. 353) 1^{m} 51^{s}·8, and is 14′ 35″·2 north of it.
51	6	3 1 13	28 26	
52	9·10	3 0 32	26 9	* (51) is A. S. C. (347)
53	7·8	3 2 1	25 29	
54	8·9	2 58 57	24 48	
55	4	2 55 34	24 19	* (55) is A. S. C. (336)
56	9	2 57 33	22 1	
57	9	2 58 24	21 43	
58	7·8	2 57 51	19 41	
59	8·9	2 55 48	−19 7	

2. Observations made at Starfield, by Mr. Lassel.

1845.	Sidereal Time.	Apparent R. A.	Sidereal Time.	Apparent Dec.
	h m s	h m s	h m s	° ′ ″
Jan. 31	4 8 54·9	2 59 41·7	4 8 28·0	−24 1 8·1
Feb. 1	4 57 28·6	2 59 12·8	4 57 28·6	23 42 20·9
3	4 49 32·8	2 58 20·5	4 19 1·6	22 30 48·2
6	4 55 54·9	2 57 26·4	4 51 53·3	20 48 6·7

3. Observation made at Mr. Bishop's Observatory, Regent's Park, by Mr. Hind.

1845. Feb. 2, at $7^h\ 7^m\ 7^s$ Greenwich Mean Time.
Right Ascension, 44° 42′ 32″ Declination, −23° 7′ 44″

Corrected for refraction and index-error.

4. Observation made at Dr. Lee's Observatory, Hartwell, by Mr. John Glaisher.

1845. March 8, at $7^h\ 31^m\ 40^s$ Greenwich Mean Time.
Apparent Right Ascension, $3^h\ 2^m\ 2^s{\cdot}2$ Apparent N. P. D. 99° 9′ 32″·1.

II. Great Comet of 1844–5.

1. Observations made at the Royal Observatory, Cape of Good Hope. Communicated by the Astronomer Royal.

Day.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1844.		h m s	′ ″		h m s	′ ″	
Dec. 24	1	8 17 29·4	+0 45·27	2	8 17 29·4	+ 9 5·7	2
27	2	8 14 40·3	+0 24·90	10	8 14 40·3	+ 8 54·5	10
	3	8 37 35·2	−0 14·38	10	8 37 35·2	− 3 52·0	10
28	4	8 9 13·8	+1 7·68	10	8 9 13·8	+ 5 17·8	10
		9 7 33·0	+1 36·14	8	9 7 33·0	+ 7 4·5	8
30	5	8 38 56·8	+0 54·74	7	8 38 33·8	−25 29·7	6
		9 31 50·4	+1 22·63	6	9 31 50·4	−24 15·8	6
	6	9 6 2·2	+1 25·21	9	9 6 29·7	+30 8·9	8
31	7	8 31 23·7	−0 40·94	10	8 31 23·7	+ 5 26·6	10
		9 8 33·4	−0 21·89	10	9 8 33·4	+ 6 9·5	10
1845. Jan. 1	8	8 42 56·6	−1 57·08	9	8 42 56·6	− 4 0·6	9
3	9	9 20 27·9	−0 54·37	5	9 20 27·9	+ 8 48·6	5
5	10	9 58 45·3	+2 8·31	7	9 58 45·3	+20 21·6	7
6	11	8 14 9·3	−0 32·87	10	8 14 9·33	− 5 28·3	10
		9 34 11·7	+0 8·46	12	9 45 58·7	− 5 51·9	12
		10 3 5·9	+0 23·64	12			
	12	9 9 6·2	+0 52·26	10	9 9 6·2	+15 11·2	10

Day.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R.A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1845.		h m s	′ ″		h m s	′ ″	
Jan. 9	13	9 1 58·5	− 0 21·10	12	9 1 58·5	− 14 21·7	12
		9 25 20·6	− 0 9·50	10	9 40 13·1	− 14 55·9	14
		9 58 19·8	+ 0 6·55	8			
11	14	9 24 12·3	− 1 18·59	10	9 24 12·3	+ 26 20·0	10
12	15	9 14 45·7	− 1 55·55	10	9 14 45·7	− 21 37·7	10
	16	9 54 17·5	− 1 8·97	10	9 54 17·5	+ 18 49·3	10
15	17	9 58 3·8	+ 1 34·78	10	9 58 3·8	+ 17 43·6	10
	18	9 26 42·8	+ 0 58·12	10	9 26 42·8	− 19 38·2	10
16	19	10 34 4·0	− 0 50·50	14	10 34 4·0	+ 4 27·6	14
17	20	10 13 24·1	− 4 47·72	6	10 13 24·1	− 5 23·8	6
18	21	9 41 8·8	+ 2 38·02	15	9 41 8·8	− 8 35·5	15
19	22	9 49 12·3	+ 2 2·87	1	9 49 12·3	+ 8 55·8	1
		10 19 42·4	+ 1 51·78	2	10 19 42·4	+ 7 56·2	2
22	23	9 4 24·4	+ 1 38·42	10	9 4 24·4	− 6 16·9	10
		9 55 28·2	+ 1 53·69	10	9 55 28·2	− 7 59·7	10
24	24	9 3 12·9	− 0 11·31	12	9 21 25·8	− 10 47·2	16
		10 12 54·2	+ 0 7·72	12			
25	25	8 54 28·4	+ 0 48·03	10	8 54 28·4	− 17 24·2	10
	26	9 30 22·6	− 0 58·12	10	9 30 22·6	+ 11 41·4	10
27	27	9 49 1·8	− 4 4·61	8	9 49 1·8	− 7 3·9	8
28	28	9 41 9·0	+ 1 40·10	12	9 41 9·0	− 0 22·7	12

The + sign denotes the comet's right ascension or declination to be greater than the star's ; the − sign the contrary.

The above observations were made with the spider-line position micrometer attached to Dollond's 46-inch achromatic, and with a power of 43. They are not corrected for the effects of refraction and parallax.

Approximate Places of the Stars of Comparison.

No. of Star.	Magnitude.	Right Ascension.	Declination.	Remarks.
		h m s	° ′	
1	7	19 42 45	−38 10	
2	8	20 16 26	40 55	* (2) follows Madras Cat. 9387 1m 0s, and is 22′ 39″ north of it.
3	9	20 17 17	41 8	* (3) follows Madras Cat. 9387 1m 51s, and is 9′ 3″ north of it.
4	7	20 27 1	41 44	
5	8	20 51 57	43 35	
6	...	20 51 37	42 40	
7	9	21 5 57	43 34	
8	7·8	21 19 48	44 12	
9	9	21 44 18	44 32	
10	8	22 6 54	44 26	
11	7	22 20 29	44 32	
12	5	22 21 26	44 53	* (12) is γ *Gruis*.
13	5	22 58 8	44 21	* (13) is θ *Gruis*.
14	8	23 22 11	42 50	* (14) is Madras 10·784.
15	7·8	23 33 42	43 8	* (15) is Madras 10·851.
16	8	23 33 14	42 26	
17	8	0 0 56	40 37	
18	8	0 3 17	41 15	
19	7·8	0 13 11	40 6	* (19) is No. 64 of the Madras General Catalogue.
20	8·9	0 25 46	39 32	
21	8	0 27 6	38 51	* (21) is Madras (149).
22	8	0 40 1	37 48	
23	7·8	0 59 1	35 38	
24	8	1 14 34	34 4	
25	7	1 19 55	33 22	
26	8·9	1 21 50	32 52	
27	9	1 36 51	31 30	
28	9	1 36 44	−30 36	* (28) follows (527) 7m 45s·4, and is 6′ 34″ north of it.

Mr. Maclear remarks that this comparatively splendid comet was reported to have been seen by a person at Green Point on the 18th of December. Captain Eardly Wilmot, R.A., saw it the next evening from the same neighbourhood. The tail, then pointing upwards, was 3° or 4° in length, and the head seemed to descend below the horizon simultaneously with the planet *Mercury*. On the 30th December, the tail was 10° 30′ in length, and with the power 43 of the nucleus pungently bright. The angle at the nucleus between the meridian circle passing through the nucleus and the axis

of the tail was 22° 30′ towards the north or right hand. On the 31st this angle was 21° 15′, and on January 6, 11° 5′, and on the latter evening the length of the tail was about 8°. On January 11, a faint ray of luminous matter, about 1¼ degree in length, was seen to extend to from the anterior portion of the comet's head in a direction opposite to that of the tail. The breadth of this ray near the head was about 2′, increasing slightly towards the extremity. Its borders were comparatively well defined, and the light gradually diminished in intensity from that portion nearest the comet's head, until it became insensible. A dark space seemed to separate the head of the comet and ray. From the 18th to the 27th, the tail and the anterior ray were both rendered invisible by the moonlight. Viewed with the comet-sweeper on the latter evening, the northern borders of the tail and anterior ray appeared distinct and sharply defined, but the light became fainter towards the southern border of tail, and the corresponding border of the ray could no longer be traced. The whole presented a fan-like appearance. Viewed with the 46-inch achromatic, power 43, the anterior extension of the light could be traced as a distinct ray for about 5′ from the comet's head, the luminous matter on the southern border becoming then diffused and scattered, while the northern border continued well defined. The actual connexion of the rays and head of the comet still could not be traced. On the 9th of February both comets (the great comet and Mauvais' comet) were within the field of view of the comet-sweeper.

2. Extract of a Letter from W. Pole, Esq. dated Elphinstone College, Bombay, Jan. 30, 1845, to Professor Christie. Communicated by Professor Christie.

Mr. Pole states that the comet was seen in the southern part of India towards the end of December, and at Bombay in the beginning of January. The nucleus resembled a star of the fifth or sixth magnitude, and the tail (which was straight and pointed directly from the sun) could be traced distinctly from 7 to 10 degrees. Its light was but faint, but the whole was visible to the naked eye, and formed a beautiful object in the south-west part of the heavens. From two positions obtained roughly by means of a sextant and theodolite, combined with a third observation made at sea, Mr. Pole deduced approximate elements, from which he inferred that the orbit has some resemblance to that of a comet which appeared in 1737; and he remarks that, if we go back by periods of 107 years, we arrive at A.D. 238, and that a comet resembling the present one appeared in 240. We have also $240 + (107 \times 7) =$ A.D. 989; and again, $989 + 107 =$ A.D. 1096; and it happens that comets are mentioned in both these years.

3. Observation made at Mr. Bishop's Observatory, Regent's Park, by Mr. Hind.

The comet was observed on the evening of March 3, 1845: it was extremely faint, appearing as an oval mass of light, with a

slight condensation of the cometic matter towards the centre. No micrometrical measures could be made owing to the faintness of the comet: it was, therefore, compared instrumentally with ϑ *Eridani* and its position connected for refraction and index error, was found to be as follows:—

March 3, at 7h 19m 56s Greenwich Mean Time Æ 52° 56′ 9″ .... 2 obs.
7 21 53 — — Dec. −10° 34′ 25″ 3 obs.

III. De Vico's First, or Periodical Comet.

1. Observations made at Starfield, with the 9-foot equatoreal, corrected for refraction, but not for parallax. By Mr. Lassel.

1844.	Sidereal Time. h m s	Apparent R. A. h m s	Sidereal Time. h m s	Apparent Dec. ° ′ ″
Sept. 19	23 56 48·9	0 52 47·6	23 57 52·4	−10 57 55
20	23 34 10·8	0 54 38·4	23 34 10·8	10 29 46·3
30	23 50 26·0	1 9 29·1	23 50 26·0	5 58 55
Oct. 7	1 9 48·7	1 16 16·1	1 9 48·7	3 9 38
11	23 34 43·7	1 19 5·7	23 34 43·7	1 43 48·7
15	2 36 4·0	1 21 48·6	2 33 31·3	0 21 27·6
17	1 45 27·2	1 22 54·3	1 44 8·8	+0 15 49·4
22	1 11 22·0	1 25 32·1	1 8 24·5	1 44 34·5

2. Observations made with the large refractor at Mr. Bishop's Observatory, Regent's Park. By Mr. Hind. Corrected for refraction, but not for parallax.

1844.	Greenwich Mean Time. h m s	Comet's App. R. A. h m s	Comet's App. Dec. ° ′ ″	
Oct. 19	9 55 29	1 23 55·9	+0 51 8	Instrumental place.
20	9 36 14	1 24 26·7	1 8 19	
22	10 27 12	1 25 27·6	1 43 28	
	11 7 12	1 25 29·50	1 44 15·4	Microm. Measures
Nov. 4	10 2 21	1 32 5·67	5 0 55·5	
6	9 27 53	1 33 10·20	5 27 44·9	
9	8 58 10	1 34 54·15	6 5 45·7	
30	7 22 51	1 50 40·19	10 0 7·9	R. A. uncertain.
Dec. 6	7 36 10	1 56 27·86	10 59 30·3	Microm. Measures.

IV. De Vico's Second Comet.

1. Observations made at Starfield. By Mr. Lassel.

1845.	Sidereal Time. h m s	Apparent R.A. h m s	Sidereal Time. h m s	Apparent Dec. ° ′ ″
March 29	9 25 41·0	8 24 29·0	9 23 7·0	+11 27 13·2
31	9 56 9·1	8 16 48·4	9 53 44·1	7 59 20·3
April 2	9 54 43·5	8 13 10·5	9 52 4·4	4 43 50·9
4	9 42 31·0	8 8 30·6	9 40 29·0	1 39 36·7
7	9 59 22·8	8 2 25·7	9 58 57·9	−2 38 0·7

The apparent places of the stars of comparison are deduced from observations with the equatoreal of the nearest known stars; the correction for differences of refraction being duly applied.

2. Observations made at Hamburg. By C. Rumker, Esq. Communicated by Dr. Lee.

1845.	Mean Time at Hamburg. h m s	Apparent R.A. of Comet. h m s	App. Declination of Comet. ° ′ ″
March 10	10 13 43·5	151 2 11·9	44 36 25.0 N.
12	8 34 8·4	147 33 20·2	41 54 23·6
13	12 23 19·6	145 32 56·8	40 9 7·5
14	8 43 19·6	144 8 24·9	38 48 53·9
15	8 11 26·0	142 33 34·2	37 12 48·4
17	8 11 19·7	139 30 4·0	33 45 19·4
24	8 3 40·3	130 45 24·0	20 45 6·2
25	9 49 20·2	129 40 4·3	18 44 16·4
27	9 2 59·5	127 49 19·2	15 5 30·8
29	11 34 11·9	126 3 40·6	11 19 21·4
April 1	8 54 17.5	123 58 8·5	6 24 50·4
2	8 25 54·7	123 20 8·3	4 49 55·6
3	8 44 20·2	122 43 15·9	3 14 35·5
4	9 28 17·1	122 7 54·1	1 40 15·9
5	9 8 41·7	121 35 54·3	0 12 53·4 N.
6	9 36 25·6	121 5 14·7	1 14 31·3 S.
7	8 51 57·4	120 37 41·2	2 35 2·0
10	9 22 14·3	119 21 57·1	6 29 26·0

3. Observations made at Dr. Lee's Observatory, Hartwell. By Mr. John Glaisher and the Rev. J. B. Reade.

1845.	Greenwich Mean Time. h m s	Apparent R. A. ° ′ ″	Greenwich Mean Time. h m s	Apparent N.P.D. ° ′ ″	Star compared.	
March 28	8 52 29	8 27 44·98	8 52 29	76 45 2·7	A.S.C.	1062
31	9 30 51	8 18 28·64	9 37 53	82 1 9·7	Piazzi VIII.	67
April 3	9 11 26	8 10 45·61	9 25 32	86 51 14·7	—	107
7	9 21 45	8 2 23·54	9 26 50	92 39 20·1	A.S.C.	994
8	—	—	9 30 44	93 59 39·6	—	994

4. Elements and ephemeris. By Mr. Hind.
The elements are as follows :—

Passage through Perihelion, 1845, April 20·98295, Mean Time, Greenwich.
Log. least distance .... 0·0987530
Inclination to Ecliptic .... 56° 26′ 35″
Longitude of Perihelion...... 192° 29′ 14″ } from apparent
Longitude of ascending node 347° 6′ 7″ } Equinox, March 0.
Motion direct.

5. Elements computed by Mr. Gotze, of Berlin; and also by Mr. Funk, of Hamburg. Communicated by Dr. Lee.
Those by Mr. Gotze agree very closely with the above.

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v. D'Arrest's Comet.

1. Observations by Mr. Lassel.

1845.	Sidereal Time. h m s	Apparent R.A. h m s	Sidereal Time. h m s	Apparent Dec. h m s
Jan. 19	14 50 25·5	18 59 25·9	14 50 25·5	+46 13 31
20	3 50 40·1	18 57 50·7	3 46 15·3	46 32 0·7
24	3 10 20·1	18 43 59·0	3 6 54·2	48 58 44·2
Feb. 8	8 25 19·8	16 6 35·5	8 20 15·6	61 36 11
14	9 3 25·4	13 10 3·6	9 4 3·5	58 37 30
March 11	7 53 3·1	9 12 54·1	7 49 44·5	− 2 16 11·3

2. Observations by Mr. Rumker, at Hamburg. Communicated by Dr. Lee.

1845.	Mean Time at Hamburg. h m s	Apparent R.A. of Comet. ° ′ ″	Apparent Declination. ° ′ ″
Jan. 3	7 45 3·4	292 34 1·5	38 35 17·2
10	7 45 54·4	290 5 18·5	41 30 37·7
11	16 11 22·6	289 30 35·3	42 6 47·5
12	7 32 11·4	289 12 55·8	42 24 17·2
13	6 44 29·0	288 45 24·8	42 51 19·3
30	12 0 29·8	272 32 52·5	53 39 43·8
Feb. 3	15 55 38·8	262 36 38·1	57 28 39·3
5	10 37 14·8	256 25 51·0	59 9 31·7
6	9 45 53·6	252 24 11·8	60 1 44·2
7	8 50 45·2	247 50 19·6	60 49 42·1
8	9 2 8·6	242 22 25·6	61 31 44·4
9	13 54 27·9	234 54 57·6	62 7 37·6
10	10 24 48·5	230 43 51·0	62 18 44·6
18	17 28 43·5	171 30 21·9	46 21 8·8
19	17 22 1·1	167 8 56·6	42 43 59·7
24	7 59 25·5	153 18 52·7	26 17 12·9
26	11 20 39·5	149 16 53·6	19 49 53·6
27	9 56 5·5	147 49 30·4	17 18 16·4
Mar. 1	10 7 41·7	145 13 0·3	12 31 37·2
5	11 14 21·3	141 27 44·2	5 6 18·8
8	9 1 47·0	139 37 3·0	1 9 13·2

3. Observations at Dr. Lee's Observatory, Hartwell. By Mr. Glaisher and Rev. Mr. Reade.

1845.	Greenwich M. Solar Time. h m s	Apparent R.A. h m s	Greenwich M. Solar Time. h m s	Apparent N.P.D. ° ′ ″	Star Compared.
Mar. 5	10 58 41	9 25 46·65	11 2 40	84 51 10·4	A.S.C. 1168
6	9 57 49	9 23 7·28	10 50 45	86 22 33·6	Hydræ 131 (Bode)
7	10 4 1	9 20 35·85	——	——	Piazzi IX. 114
8	11 0 21	9 18 13·70	10 31 15	88 55 33·5	Piazzi IX. 54
11	10 14 9	9 12 50·28	10 20 12	92 4 51·1	A.S.C. 1151
12	8 50 29	9 11 21·38	8 50 29	92 55 1·1	——

IV. Observation of the commencement of the Solar Eclipse of May 6, 1845, at the Observatory of the Royal Military College, Sandhurst. By Professor Narrien.

		h m s
Mean Time,	May 6, 1845	8 27 19·38 A.M.
or	May 5, 1845	20 27 19·38

The sun shone brightly between the clouds, but waves of vapour rolled continually along the edge of his disk, causing the instant of commencement to be uncertain within four or five seconds.

By a geodetical operation the observatory is connected with one of the stations (Norris' obelisk on Bagshot Heath) in the Ordnance Survey; from which have been found,—

	° ′ ″
For the latitude of the observatory..................	51 20 32·99 N.
For the longitude (in time) West of Greenwich	3 3·78

Clouds prevented the end of the eclipse from being observed.

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ROYAL ASTRONOMICAL SOCIETY.

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Captain W. H. Smyth, R.N., President, in the Chair.

James Prior, Esq. Deputy Inspector of Naval Hospitals, and John Dickenson, Esq. of Abbott's Hill, were balloted for, and duly elected Fellows of the Society.

The following communications were read :—

I. On the Ephemerides of Jarchi, the Chaldean Months, and the derivation of *Orion* and his Dogs. By S. M. Drach, Esq.

The author suggests, on the authority of Weidler's *Historia Astronomiæ*, that the first astronomical tables and ephemerides published in Europe are those of Rabbi Solomon Jarchi, although still inedited. Jarchi is supposed by Dr. Jost to have lived from the years 1040 to 1105 (*Gesch. der Israel*, Vol. VI. p. 243).

The author then proceeds to the consideration of the passage of the book of Job, contained in the thirty-first and thirty-second verses of the thirty-eighth chapter: "Canst thou bind the sweet influences of *Pleiades*, or loose the bands of *Orion?* Canst thou bring forth *Mazzaroth* in his season? or canst thou guide *Arcturus* with his sons?" And he cites the opinion of the Rabbinical commentators for the meaning of each of the astronomical words contained in it.

He then gives from Buxtorf's *Lexicon* the meanings of the names of the Jewish months, and concludes that their calendar is founded on the agricultural occupations of the Chaldean shepherds, who were employed during winter in preparing the fleeces of their flocks for clothing, &c.

He thinks it not impossible that the numerical values of the names of the months indicate the number of days wherein celestial phenomena recur, mystically decomposed for priestly purposes; and he gives instances in which certain combinations of their values represent pretty nearly the synodical periods of some of the planets.

There is some reason for supposing that *Orion*, *Sirius*, and

that some of the very best definition have been witnessed on these mountains in the middle of summer with a light northerly air; while at the same time, in the level country below, a strong south-east wind has been blowing with its usual accompaniment of large, faint, woolly blotches in the place of what ought to have appeared, viz. stellar points."

Magnitudes of Stars.—" Bessel's recommendation of steel specula reminds me of a series of astronomical phenomena that are passing unrecorded, viz. the changes in the magnitude of some stars.

" In March 1843, Mr. Mackay wrote from Calcutta to Sir John Herschel that η *Argus* had become as bright as *Canopus*, from which Sir John concluded that η was making another step forward, and would in time rival *Sirius* and the planets. But, after having for a few days in the beginning of the above-mentioned month approximated to *Canopus*, η dwindled down again very rapidly. But Sir John was not far out, for, although the star may not be advancing by successive steps, it seems to be doing the same by undulations; for, in March 1843, it did not go down to the point whence it started, and it is now again on the increase. It is, and has been for a month, brighter than *Canopus*, half way indeed between him and *Sirius*, and is very red.

" Not having been able yet to get my tourmalines applied to a telescope, I cannot report on their actual sufficiency for measuring the magnitudes of stars; the principle, however, seems to answer, inasmuch as a mode of observation is produced, in which the only variable element is angular motion.

" Numerical measurement of the periodical and secular changes in the *colour* of stars would seem to be as important as that of their *lustre*. My plan for doing it cannot be put into action without a clock-moved telescope. The principle is merely this:—a prism is to be introduced into the tube of a telescope so as to make the linear spectrum of a star, instead of the round white image to be viewed by the eye-piece. The mean yellow ray is to be brought by mechanical adjustments to a fixed point in the field, and, the spectrum being divided into a number of spaces of certain angular extent, the brightness of each is to be measured separately, as in the case of the *magnitudes* of the stars.

" White stars of all sizes will, of course, shew the same relative intensities in the different divisions of the spectrum; while coloured stars will exhibit a preponderance in the part which answers to their colour. To eliminate the differences of the effects of the atmosphere, and to determine what is white light, a method must be pursued similar to that in the discussion of the measurement of *magnitudes*; and the *colour* of the star will ultimately be expressed by numbers, without attempting to determine by what particular designation such a tint would be conceived by a person of ordinary eyesight.

"Let this be a spectrum divided into equal angular spaces of 2″ each, the one marked 0 being the yellow ray; by means of a perforated plate in the focus of the telescope, each of these portions is to be brought singly and successively into view, and have its brightness separately measured as a single star; the several portions of colour will then be represented thus:

	−2	−1	0	+1	+2	+3
Sirius............	20	40	70	50	30	10
η Argus............	30	80	40	20	10	0
Canopus.........	10	20	35	25	15	5

"The method is undoubtedly rough, and inapplicable to the smaller magnitudes of stars visible in any telescope, but if a measurement is obtained in numbers (free from theoretical objection), and capable of having the probable error computed, it should be adopted until something better can be advanced.

"It will not do for any one to tell the public, even in the case of the magnitudes of stars, that 'he has tried all instrumental methods, and found all to be inferior to estimation by the naked eye;' because, so long as such a method of observation is the only one followed, so long will the assertion remain mere rhetoric, — a consumption of time without producing its equivalent of useful effect. Let us rather remember the primary and aphorismal foundations of practical science, such as:—

"'Science begins with the observation of common facts, but, even at this stage, requires that the observations be precise.'

"'Facts are the materials of science, but all facts involve ideas, and since in observing facts we cannot exclude ideas, we must take care that for the purposes of science, the ideas be clear and vigorously applied. Therefore, facts for the purposes of material science should involve *conceptions* of the intellect only, and not *emotions*, and must be observed with reference to our most exact conceptions, number, place, figure, motion, force, &c.'"

III. On the Anomalous Phenomenon of the Apparent Projection of a Fixed Star on the Disk of the Moon after Apparent Immersion. By G. W. Hearn, Esq. Communicated by T. Galloway, Esq.

The author attempts to account for the anomalous phenomenon of the projection of a star on the disk of the moon before its immersion by the theory of the aberration of light. He supposes, in illustration of his theory, a screen to be placed across the *actual* direction of a ray of light proceeding from a star along the tube of a telescope; then, though this screen might not intercept the apparent path of the ray, yet the star would certainly disappear. If, on the contrary, the screen should intercept the apparent path of the ray, and not the real path, the star would not disappear, but would appear to be projected on the screen. He imagines that the moon in the heavens is such a screen, and that by the composition of the *secular* aberration of the light of a star (by which is meant the aberration arising from the star's proper motion), with the annual aberration computed according to the usual rules, most of the anomalous phenomena of occultation may be accounted for.

He takes as examples of the application of his theory the occultations of 119 *Tauri* and 120 *Tauri*, as observed by Sir James South and Mr. F. Simms, on December 31, 1831. By supposing in the case of the first star, its proper motion, resolved perpendicularly to the line joining the star and sun, and then parallel to the ecliptic, to be equal and in the same direction as the earth's motion in its orbit, or the absolute aberration to be nothing; and, in the case of the second star, this same resolved proper motion to be thrice the earth's motion in its orbit, and in the contrary direction, he thinks he can explain the fact of the instantaneous disappearance of the first at the moon's border, while the second was observed to disappear after being fairly within the illuminated part of the moon.

The author next discusses the case of the occultation of a planet by the moon, and takes, as an instance, the immersion of *Jupiter* and his satellites, behind the moon's dark limb, alleged to have been seen by Mr. Cornfield, at Northampton, on August 23, 1824. He then gives a general formula for finding the secular aberration deduced from the theory.

The author then proceeds to consider the indirect effect of the difference of atmospheric refraction of the light from the moon and stars in the production of the phenomenon in question, and states his views as follows:

"Admitting that light is propagated with the same velocity from each of the fixed stars, still their proper motions, combined with the probable motion of our own system and that of the earth in its orbit, must cause the relative velocities of light from the various fixed stars impinging on our atmosphere to be different, and this difference of relative velocity must produce a difference of refraction, however small that difference may be."

He lastly proceeds to shew how in certain cases this difference of refraction, though very small, may produce a very sensible effect

on the time between apparent immersion and disappearance, and investigates a mathematical formula, in which the supposed direct and indirect effects of aberration are combined, for determining the secular aberration of the star, and the amount of the difference of refraction for the moon and star.

IV. On the Longitude of the Honourable East India Company's Observatory at Madras. By T. G. Taylor, Esq.

The want of an accurate determination of the Longitude of the Madras Observatory has been greatly felt, both in an astronomical and a geographical point of view. In the former case, any error in the value assumed in the comparison of the places of the moon and planets with the tables, necessarily leads to seriously mischievous results; and in the latter, the triangles in the great trigonometrical survey of India depend for their zero upon the meridian passing through the Observatory; — the inquiry must therefore be considered one of singular importance. Mr. Goldingham, the predecessor of Mr. Taylor, had determined the longitude from no less than 230 observations of the eclipses of *Jupiter's* first and second satellite, and, before a result derived from so large a number of observations could be safely disputed, it seemed desirable to meet the inquiry with at least something like a corresponding number of observations.

The observations of moon-culminating stars were begun in 1831, on the erection of the present five-feet transit instrument, and the longitude, resulting from the observations of the first year as determined from the corresponding observations at Greenwich and Cambridge, was $5^h\ 21^m\ 3^s{\cdot}7$ or about 5^s less than the value assigned by Mr. Goldingham. At that time Mr. Taylor was not inclined to give to results obtained from moon-culminating stars that degree of credit which later experience has shewn them to deserve, from considerations connected with the probably varying error of the observations of the moon's limbs, but, as far as the present observations go, it appears that this error must be confined within narrow limits, since the semi-diameter of the moon, as measured at Madras, does not differ above half a second of space from that observed at Greenwich, Cambridge, Edinburgh, and Hamburg. Mr. Riddle, at the suggestion of Mr. Baily, undertook the reduction of the observations of the corresponding moon-culminating stars for the years 1834–1837, and the results are given in the twelfth volume of the Society's *Memoirs*.

The formula employed by him is precisely the same as that which has been used by Mr. Taylor, both in the previous as well as the present results; but there are one or two circumstances in which the treatment of the observations differs. In cases in which the full moon has happened in the interval between the moon's transiting the meridian of the Madras and the Western Observatory, it has sometimes happened that the first limb of the moon has been observed at Madras, and the second limb at the Western

Observatory; in these cases Mr. Riddle has computed the time of passage of the moon's semi-diameter from the semi-diameter given in the *Nautical Almanac*, while Mr. Taylor has rejected such cases: again, in taking the means, Mr. Riddle has given to each result a weight equal to the number of stars observed; whereas Mr. Taylor has made use of a table of weights depending upon the square root of the number, the rapidity of change of the moon's right ascension, and the relative accuracy of observations of the moon or stars.

Mr. Taylor also, having to discuss several observations of the second limb of the moon, has thought it necessary to separate these from observations of the first limb, and to treat them distinctly.

Mr. Taylor has, in the present paper, given the recomputed results for 1831–1833; so that Mr. Riddle's paper, with the present one, exhibits each single determination from corresponding observations at Greenwich and Cambridge, from 1831 to the end of 1844; and at Edinburgh, from the commencement to the end of 1839. The Hamburg observations have been taken from the *Astronomische Nachrichten*, Nos. 503, 504, and 508. To reduce each series to Greenwich, the longitudes of Cambridge and Edinburgh have been taken from the *Nautical Almanac* for 1845, and that of Hamburg from the *Connaissance des Temps* for 1840.

Finally, from 442 observations of the moon's first limb, the resulting longitude of Madras is $5^h\ 20^m\ 56^s{\cdot}38$ east, with a probable error of $\pm\ 0^s{\cdot}23$; and from 86 observations of the second limb, the result is $5^h\ 20^m\ 58^s{\cdot}19$, with a probable error of $\pm\ 0^s{\cdot}57$. Or, for the present, the most probable value is, $5^h\ 20^m\ 57^s{\cdot}28$.

V. On the Brahmin Zodiac. By W. W. Boreham, Esq. Communicated in a Letter to the Secretaries.

"I send you a rubbing from an engraved zodiac on a small silver bowl, which came into the possession of a relative of mine through the late Captain Taynton, who took it from the tent of a Burmese officer during the late war in that country.

"I cannot say that the bowl itself possesses any great antiquity; but the engraving may be regarded as traditionary evidence of the ancient Brahmin zodiac, and as such, possesses an interest.

"The Indian zodiac is well known, and engravings from it are not uncommon; but the Brahmin version of it is, I believe, not so generally given in works on the subject. Its peculiarity appears to consist principally in the retention of the fish and omission of the goat in *Capricornus:* it is alluded to by Dupuis in Lalande's *Astronomie*. The reverse of this is observed in the Greek zodiac.

"There are other minute peculiarities, but as they may be the result of the engraver's fancy I will not dwell upon them, merely observing that the fish in *Capricornus* (if it be one) does not appear

to be the peculiar sort of fish Dupuis asserts is found in that place, and which he calls a sword-fish.

"Pray excuse my trespassing upon your valuable time.

"Invented at a time when every figure was a symbol, these must still possess a meaning, which every version of them may help to explain."*

VI. Comet communications:—

1. A Communication from Professor Schumacher of Observations of Comets made at Berlin by Professor Encke.

Name of Comet.	Month and Day.	Berlin Mean Solar Time.	Right Ascension.	North Declination.	No. of Comp.	Remarks.
	1844.	h m s	° ′ ″	° ′ ″		
D'Arrest.	Dec. 28	8 8 27·0	294 9 13·5	+ 36 18 52·9	2	Piazzi, Circular Micrometer.
	1845. Jan. 10	6 33 16·8	290 6 55·3	41 28 47·1	15	B. Z. and H. C.
	11	6 31 19·8	289 41 25·3	41 55 24·0	13	B. Z.
	11	7 7 11·6	289 40 29·5	41 56 7·5		B. Z.
	13	6 25 27·0	288 45 47·7	42 50 38·0	12	B. Z.
	14	7 51 12·1	288 13 49·5	43 20 56·4	10	B. Z.
	27	8 36 29·1	277 28 24·5	51 7 15·6	12	Determined at the Merid. Circle.
	28	10 37 47·9	275 57 15·9	53 57 33·4	10	H. C.
	30	8 17 45·8	272 50 18·2	53 31 9·3	10	H. C.
	Feb. 4	8 42 59·6	260 23 14·6	58 7 53·2	10	H. C.
	25	12 23 32·8	150 57 42·6	22 37 28·4	5	B. Z.
	Mar. 3	11 5 43·4	143 7 12·6	8 28 17·6	10	B. Z.
	6	11 37 51·9	140 45 29·4	3 37 27·3	18	B. Z. and H. C.
	8	9 14 1·1	139 37 21·9	+ 1 10 10·1	11	B. Z. and H. C.
	12	10 52 34·5	137 50 46·0	− 2 57 27·3	10	B. Z.
	30	10 0 39·4	135 43 56·7	11 44 24·8	5	Observed in the dark field.
Mauvais (Second).	Feb. 4	7 19 26·0	44 31 16·6	− 20 58 21·2	19	H. C.
	8	7 16 33·6	44 17 40·4	19 48 7·2	2	H. C.
	9	7 56 32·4	44 15 24·4	19 16 40·0	5	H. C.
	Mar. 6	7 52 42·7	45 24 24·1	9 43 0·9	3	Own determination.
Colla.	Feb. 25	8 6 37·9	49 27 31·0	− 13 6 10·2	2	Piazzi.
	Mar. 5	7 55 28·1	53 57 7·4	9 48 36·6	15	B. Z.
	6	7 30 39·6	55 29 47·4	8 43 45·9	2	B. Z.
	9	7 52 29·4	56 0 36·8	8 22 11·4	5	Own determination.

* See Bailly, Hist. de l'Astronomie Ancienne, pp. 157–488; Gebelin, Hist. du Calendrier, pp. 60–88; Jamieson's Celestial Atlas; Maurice, Hist. Hindostan, vol. i. p. 298.

Name of Comet.	Month and Day.	Berlin Mean Solar Time.	Right Ascension.	North Declination.	No. of Comp.	Remarks.
	1845.	h m s	° ′ ″	° ′ ″		
De Vico.	Mar. 13	10 9 33·5	145 43 34·5	+ 40 18 55·8	8	B. Z.
	15	8 22 14·8	142 33 42·7	37 13 0·0	9	B. Z.
	22	8 54 44·4	132 55 41·2	24 29 12·0	11	Own determination.
	27	11 42 32·7	127 43 59·4	14 54 36·0	12	B. Z. and H. C.
	30	8 47 24·1	125 23 28·4	9 48 2·3	10	B. Z. and H. C.
	April 1	8 32 16·5	123 59 2·2	6 27 16·3	10	Own determination.
	2	8 51 36·0	123 19 34·0	4 49 6·6	11	B. Z.
	3	8 54 37·9	122 43 4·8	3 14 39·8	10	B. Z. and H. C.
	4	8 30 19·7	122 9 16·3	+ 1 44 43·7	10	B. Z.
	6	8 51 47·3	121 6 15·4	− 1 11 2·7	15	H. C.
	7	8 20 40·5	120 38 18·7	2 32 25·8	10	Piazzi.
	13	8 24 5·2	118 21 29·1	9 56 33·8	6	Own determination.
	14	8 40 46·2	118 3 40·5	11 2 46·9	15	B. Z. and H. C.

The observations were for the most part made at the great refractor, with Fraunhofer's wire-micrometer, excepting the few, for which another mode of observation is stated in the remarks. The remarks shew from whence the stars of comparison are taken.

Piazzi......Piazzi's Second Catalogue.
B. Z.......Bessel's Zones.
H. C.......Histoire Celeste.

2. Observations of Mauvais' Second Comet (6th series) made at the Royal Observatory, Cape of Good Hope. Communicated by G. B. Airy, Esq.

Date.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1845.		h m s	m s		h m s	′ ″	
Feb. 16	60	9 41 33·9	+ 1 40·59	8	9 50 25·6	+ 7 22·5	4
26	61	8 48 13·9	+ 1 6·90	12	8 59 48·1	+ 7 36·8	6
Mar. 2	62	8 40 27·0	+ 1 2·17	16	8 55 53·5*	+ 3 34·4	10
3	62	8 11 11·3	+ 1 21·73	18	8 27 6·6	− 14 33·7	8
4	63	8 18 50·1	− 1 22·22	18	8 34 24·9	+ 8 4·6	10
5	63	8 27 25·1	− 1 0·12	16	8 11 0·3	− 9 26·8	10
6	64	8 17 34·2	− 1 14·09	16	8 32 19·2	− 0 23·5	10
10	65	8 8 5·0	− 1 40·46	16	8 32 14·3	− 2 10·2	10

The sign + implies that the comet's right ascension or declination is greater than the star's; the sign — the contrary.

The above observations were made with the bar-micrometer attached to Dollond's 46-inch achromatic. They are not entitled to great confidence, on account of the extreme faintness of the comet.

Those made on March 2d, 3d, 4th, and 5th, are considered to be the best.

The above observations were made by Mr. Mann.

Approximate Places of the Stars of Comparison.

No. of Star.	Magnitude.	Right Ascension.	Declination.
		h m s	° ′
60	8	2 55 0	−15 52
61	9	2 57 46	12 8
62	7	2 59 9	10 54
63	9	3 2 11	10 11
64	9	3 2 49	9 43
65	9	3 4 51	− 8 39

(Signed) Thos. Maclear.

Royal Observatory, Cape of Good Hope,
24th March, 1845.

3. **Observations of the First Great Comet of 1844–5 (Wilmot's Comet) (3d series), made at the Royal Observatory, Cape of Good Hope. Communicated by G. B. Airy, Esq.**

Date.	Star of Comparison.	Cape Mean Time for Right Ascension.	Difference of R. A. of Comet and Star.	No. of Obs.	Cape Mean Time for Declination.	Difference of Declination of Comet and Star.	No. of Obs.
1845.		h m s	m s		h m s	′ ″	
Jan. 29	29	9 41 13·7	+ 0 53·22	12	9 41 13·7	− 1 31·3	12
30	30	9 51 32·7	+ 0 30·53	12			
31	31	9 37 59·7	+ 1 10·98	16	10 1 9·4	− 1 4·1	16
		10 25 23·5	+ 1 20·84	16			
Feb. 1	32	9 39 5·9	+ 3 10·27	12	10 0 54·1	− 3 2·4	10
		10 23 5·3	+ 3 19·21	12			
3	33	9 28 17·5	+ 1 12·22	16	9 49 55·4	− 6 41·1	14
		10 12 24·9	+ 1 20·42	16			
4	34	9 10 9·1	− 1 0·72	16	9 32 3·5	+ 2 8·2	16
		9 53 23·9	− 0 52·78	16			
8	35	9 34 33·3	+ 0 13·12	30	9 8 4·6	− 8 28·1	10
					9 58 14·7	− 9 55·7	10
9	36	9 12 27·7	− 0 11·05	16	9 30 19·4	+ 5 15·9	12
		9 51 44·5	− 0 5·10	16			
10	37	9 25 54·6	− 0 39·98	14	9 44 26·2	− 3 5·4	14
		10 6 28·9	− 0 34·10	8			
16	39	8 56 57·0	− 0 15·06	12	9 10 59·8	−15 30·4	10
		9 24 24·4	− 0 11·18	12			
18	40	8 54 10·9	− 2 26·72	12	9 17 12·7	− 6 15·7	10
		9 37 34·3	− 2 21·73	12			
27	41	8 19 15·8	− 0 19·00	20	8 40 56·0	+ 2 0·1	12
		9 2 56·2	− 0 14·29	20			
28	42	8 30 7·5	− 1 0·13	12	8 46 22·2	+ 4 56·8	12
		9 2 8·3	− 0 57·47	12			
Mar. 4	43	9 20 50·3	− 2 3·33	18	9 0 49·5	− 7 9·9	10
5	44	8 48 0·9	+ 1 58·02	18	9 8 43·4	+ 5 47·1	10
6	45	8 57 18·3	− 0 31·11	16	9 13 12·1	− 4 40·6	10
9	46	8 20 25·0	− 3 9·48	24	8 49 42·7	− 0 18·8	12
12	47	8 25 48·7	− 0 42·36	20	8 46 5·5	− 1 24·6	14

The observations on January 29 and 30 were made with the spider line position micrometer attached to Dollond's 46-in. achromatic, power 43; the rest with the bar-micrometer. They are not corrected for the effects of refraction and parallax.

The sign + attached to the differences of right ascension or

declination, indicates that the comet's right ascension or declination was greater than the star's; the sign — the contrary.

Approximate Places of the Stars of Comparison.

No. of Star.	Magnitude.	Right Ascension.	Declination.
		h m s	° ′
29	8	1 43 0	−29 49
30	9·10	1 48 32	28 30
31	9	1 52 41	28 14
32	9	1 55 34	27 30
33	10	2 6 35	26 4
34	9	2 12 55	25 11
35	7·8	2 27 32	22 37
36	8	2 31 35	21 43
37	8·9	2 35 32	21 13
39	8	2 54 0	17 51
40	9	3 2 10	16 35
41	8	3 22 55	12 11
42	8	3 25 52	11 42
43	3·4	3 35 50	10 18
44	7·8	3 33 53	9 42
45	7	3 38 30	9 30
46	7	3 47 8	8 21
47	7	3 50 42	− 7 24

REMARKS.

		° ′
Jan. 29.	The angle of position of axis of comet's tail	94 24
	The angle of position of north border of the anterior luminous matter	301 6
	These measures are liable to considerable doubt, the field of view being thickly studded with small stars.	
Jan. 30.	The angle of position of axis of comet's tail	92 48
	The angle of position of north border of the anterior luminous matter	298 30
	The comet, when viewed with the "Comet Sweeper," presents the same general appearance that it did on the 27th, but it is altogether considerably fainter; the angles of position are, in consequence, rather uncertain.	
Jan. 31.	The angle of position of the axis of comet's tail	90 53
	The angle of position of north border of anterior luminous matter	301 47
Feb. 1.	The angle of position of axis of comet's tail	94 48
	The angle of position of north border of anterior luminous matter	305 0

The comet is rapidly decreasing in brightness; it is now too faint to allow of the measures of position being made with any degree of accuracy.

Of the above measures, those made on Jan. 31 are the most trustworthy.

Feb. 9. The comet is now merely a bright nebulosity, nearly circular, and of about 3′ in diameter; no trace whatever of a tail or any other appendage.
Feb. 16 and 18. Bright moonlight; the comet barely visible.
Feb. 18 to 27. No observations could be obtained; the moonlight completely obliterating the comet.
Feb. 27. The comet seen again. Its appearance was a faint nebulosity, about 2′½ in diameter, and with no apparent condensation of light.
March 13. The presence of the moon again rendered the comet invisible.

The foregoing observations were made by Mr. Mann.

(Signed) THOS. MACLEAR.

Royal Observatory, Cape of Good Hope,
24th March, 1845.

4. Sextant Observations of Wilmot's Comet made at sea during a voyage from Van Dieman's Land to England. By Joseph Dayman, Esq. Lieut. R.N. Communicated by Captain Beaufort.

The comet was first seen on the evening of December 27, 1844, at sea, in latitude 42° south, and longitude 129° east: observations with a sextant were repeated whenever circumstances would permit, until its disappearance to the naked eye on the 6th of February, 1845. The distances of the nucleus from those stars which formed with it, as nearly as possible, a right-angled triangle, were measured, and the mean time was noted by a chronometer.

A sketch of its appearance on the 10th of January is subjoined; and near the end of the series a sketch of the relative positions of the comet and some of the neighbouring stars with which it was compared, but of whose names the author was ignorant.

5. Sextant Observations of Wilmot's Comet made on board the barque Ceylon. By E. W. Beazley, Esq. Commander, R.N.

Observations were made on every favourable night, from December 20, 1844, to February 6, 1845. The author does not rely greatly on their accuracy on account of the unfavourable state of the weather, but considers that they may be useful as giving approximate places of the comet.

6. An Observation of the Second Great Comet of 1845 (Colla's). By C. Rumker, Esq. Communicated by Dr. Lee.

On June 9, at $12^h\ 26^m\ 44^s{\cdot}7$, Hamburg mean time, the apparent right ascension of the comet was $84°\ 52'\ 57''{\cdot}0$, and its declination $45°\ 28'\ 10''{\cdot}1$ north.

7. Observations of the Second Great Comet of 1845 (Colla's), made at Ashurst. By R. Snow, Esq.

The comet was first observed on June 9, and its right ascension deduced from the observation made with the transit instrument at the lower culmination was $5^h\ 40^m\ 8^s{\cdot}27$. It was also observed on

June 10, 11, and 12, and comparisons with neighbouring stars were made, which will be rendered available when the places of the comparison stars have been accurately determined.

VII. Observations of the Solar Eclipse of 1845, May 5, and of the Transit of *Mercury* of 1845, May 8, in a Letter from W. Lassell, Esq.

"*Starfield, Liverpool, 10th June*, 1845.

"I send you such observations as I have been able to make of the late solar eclipse and transit of *Mercury*, for which the weather was, in some respects, but very unfavourable.

"May 5, 1845. With a very unpromising sky I prepared to observe the first contact of the solar eclipse by placing that part of the sun's limb (then very indistinct) which the moon would first touch, between the parallel threads of the micrometer, applied to the nine-feet equatoreal, with a power of ninety-six times.

"As the time of contact approached the sky somewhat cleared, and the moon's first impression took place at $23^h\ 14^m\ 37^s{\cdot}8$ sidereal time, or $20^h\ 18^m\ 24^s{\cdot}17$ mean time at the observatory. During the greatest part of the obscuration the sky was very cloudy, but towards the end it cleared, and the last contact was well observed at $1^h\ 33^m\ 48^s{\cdot}9$ sidereal, or $22^h\ 37^m\ 12^s{\cdot}45$ mean time. No phenomena beyond what is usual occurred; nor was there, to my senses, any perceptible diminution of light on the landscape.

"May 8. For the transit of *Mercury* the appearances a short time before it began were still more unpromising. During the forenoon, we had several showers, with a most gloomy sky; and even as late as half-past three P.M. we had a smart shower of close, small rain. A little change for the better occurred shortly before four, and I had just time to set the micrometer by the sun's limb after he became visible, and get settled at the telescope, when the first notch was cut out by the planet. The sun's limb was beautifully sharp, but occasionally obscured by passing clouds. From the time, however, of the first impression until the planet had advanced about two of its diameters upon the disc of the sun, it was generally unclouded, and the atmosphere remarkably tranquil. The first contact took place at $7^h\ 13^m\ 36^s{\cdot}3$ sidereal time, or $4^h\ 8^m\ 12^s{\cdot}36$ mean time. The internal contact, or complete immersion of the planet, took place at $7^h\ 16^m\ 48^s{\cdot}7$ sidereal, or $4^h\ 11^m\ 24^s{\cdot}24$ mean time. Both times were carefully and, I believe, accurately noted. Whilst the planet was traversing the *edge* of the sun, an apparent distortion took place, the parts of the sun's edge, or limb, in contact with the planet, appearing *rounded off;* and a moment or two before the complete immersion of the planet, an appearance analogous to *Mr. Baily's beads* took place,—the planet apparently breaking contact two or three times with the sun's limb before the final separation occurred. *Mercury* had also, to my eye, somewhat of a pear-like shape previously to his

entering quite within the sun's disc. When he had advanced two or three of his diameters, the clouds rapidly thickened, and I saw him no more.

"I take this opportunity of stating, that a late redetermination of the longitude of my Observatory depending upon the lately determined longitude of the Liverpool Observatory, inclines me to adopt finally $11^{m}\ 47^{s}{\cdot}34$ as my longitude west of Greenwich, which differs scarcely a quarter of a second from that given in my paper contained in the forthcoming volume of the Society's *Memoirs*.

"The latitude I have also redetermined lately by transits of seven stars over the prime vertical, giving $53^{\circ}\ 25'\ 3''{\cdot}5$ as the mean result."

VIII. Observations of the Transit of *Mercury* made at Aylesbury by Thomas Dell, Esq. Communicated by Dr. Lee.

"*Transit of Mercury.*—The first contact of the two limbs was invisible, the sun being obscured by heavy clouds; but almost immediately afterwards it broke through them, and the interior contact of the limbs was well observed at $4^{h}\ 18^{m}\ 33^{s}$ mean time. The sun was covered by light fleecy clouds, through which it was distinctly visible, and the discs of both the sun and *Mercury* were most beautifully and sharply defined until $5^{h}\ 12^{m}$, when the whole sky became densely overcast, and continued so until after sunset. The time of interior contact is, I believe, accurate to a second, as the error of the chronometer had been determined at the sun's transit at noon."

The President then announced that, in pursuance of a resolution of the Council, which had been duly intimated to the Fellows, as required by the Bye-laws, the business of the Ordinary Meeting was now concluded, and that a Special General Meeting would immediately be held to take into consideration a subject, of which due notice had been given to the Fellows by the following circular, which he requested the Secretary to read:—

"*Somerset House, June 5th*, 1845.

"SIR,—I have the honour of notifying to you that in pursuance of a Resolution of the Council, passed on Friday, the 23d of May last, a Special General Meeting of this Society will be held at the Society's apartments on Friday, the 13th day of June instant, immediately after the business of the Ordinary Meeting to be held on that day is concluded, for the purpose of taking into consideration and deciding upon a recommendation of the Council to suspend upon that occasion the Bye-laws relative to the Election of Fellows, and to elect as Fellows of this Society the remaining Members of the Mathematical Society (now reduced to nineteen in number, of whom three are already Fellows), without payment of the usual Admission Fees and Annual Contributions (or compositions in lieu thereof), the Mathematical Society having announced

its resolution to transfer its valuable Library, with its Records and Memorials, to the Royal Astronomical Society.—I have the honour to be, sir, your most obedient servant,

"Robert Main, Secretary."

It was then moved by Professor De Morgan, and seconded by Mr. Galloway, and resolved unanimously,—

"That the recommendation of the Council in the circular now read be approved and adopted by this Meeting; and that, on the Library, Records, and Memorials of the Mathematical Society being delivered over to this Society, the following sixteen gentlemen, Members of the Mathematical Society, be admitted Fellows of the Royal Astronomical Society without payment of the admission fees or annual contributions required by the Bye-laws, viz.:

William Wilson, Esq.	Thomas K. White, Esq.
Robert Graham, Esq.	Philip James Chabot, Esq.
Robert Porrett, Esq.	Charles O. Dayman, Esq.
James Scott Bowerbank, Esq.	Thomas Cooper, Esq.
Julius Page, Esq.	John Walton, Esq.
John Williams, Esq.	Jacob Hoyer, Esq.
Henry De Berckem, Esq.	Robert Arthur Graham, Esq.
Alfred White, Esq.	Thomas Taylor, Esq.

and that the remaining three Members of the Mathematical Society, viz.:

Benjamin Gompertz, Esq.	John Lee, Esq.
John James Downes, Esq.	

or such of them as are liable to the payment of Annual Contributions, be exempted in future from such liability."

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COMETS.

Since the June Meeting, the following communication has been received from Professor Schumacher, in a letter to the Astronomer Royal, dated June 27:—

Observations of Colla's Comet.

Königsberg. Dr. Busch (Merid. Circle, Inferior Culmin.)

1845.	Mean Time. h m s	Right Ascension. ° ′ ″	Declination. ° ′ ″
June 8	12 8 33·0	79 18 24·5	+45 20 1·9
9	26 14·7	84 43 41·9	45 28 14·3
10	43 5·5	89 56 13·1	45 15 19·1
11	58 38·0	94 49 7·4	44 44 13·2

Mr. Wichmann (Heliometer, Stars of Comp. *own determ.*, *Bessel's Zones*, *Histoire Cel.*)

	Mean Time. h m s	Right Ascension. ° ′ ″	Declination. ° ′ ″
June 7	13 2 42	74 5 27·6	+44 51 7·3
9	12 22 0	84 42 36·9	45 28 21·4
10	12 4 59	89 48 12·4	45 16 1·4
	13 9 37	90 1 39·1	45 14 58·9
11	11 48 35	94 35 23·6	44 45 54·5
12	12 46 5	99 13 26·0	43 59 30·3
14	11 54 18	106 46 8·5	42 0 47·8
15	12 45 20	110 7 40·3	40 50 1·6
16	12 51 31	113 0 1·8	39 37 57·3
19	11 19 6	119 39 41·6	36 4 29·7

Göttingen. Professor Gauss (Merid. Circle, Inferior Culmin.)

	Mean Time. h m s	Right Ascension. ° ′ ″	Declination. ° ′ ″
June 12	13 12 59·9	99 26 5·4	+43 56 53·6

Modena. Professor Bianchi (Micrometer).

	Mean Time. h m s	Right Ascension. ° ′ ″	Declination. ° ′ ″
June 6	14 24 31·5	69 5 10·8	+44 5 8·4
7	14 56 9·2	74 33 56·8	44 51 34·4
8	9 19 34·3	78 36 12·9	45 20 56·4

Hamburg. Mr. Funk (Transit, Inferior Culm.)

	Mean Time. h m s	Right Ascension. ° ′ ″
June 12	13 12 59·8	99 26 2·8
13	13 25 13·6	103 29 9·1
15	13 44 41·5	110 20 11·6

Mr. Rumker (Wire Micrometer).

	Mean Time. h m s	Right Ascension. ° ′ ″	Declination. ° ′ ″
June 20	10 46 7·3	121 28 0·4 : :	+34 53 1·1

The following elements and ephemeris of Colla's Comet have been recently received from Mr. Hind :—

"33 *High Street, Marylebone, June 16, 1845.*

"*Ephemeris of Colla's Comet.*

Noon, Mean Time at Greenwich.

1845.		R.A. h	m	s	δ °	′		Log. Δ
June 14	R.A.	7	1	3	+42	29·6	Log. Δ	9·9597
16		7	26	50	40	11·5		9·9846
18		7	47	7	37	45·4		0·0105
20		8	3	5	35	21·9		0·0364
22		8	15	46	33	6·1		0·0617
24		8	25	59	31	0·0		0·0859
26		8	34	23	29	4·0		0·1090
28		8	41	23	27	17·6		0·1308

"This ephemeris is deduced from the following corrected elements depending on the Greenwich Observation, June 8, Cambridge, June 10, and South Villa, June 12 :—

Per. Passage, 1845, June 5·69870, M. T. at Greenwich.

$$\left.\begin{matrix} \varpi & 261 & 56 & 52{\cdot}0 \\ \Omega & 337 & 50 & 5{\cdot}6 \end{matrix}\right\} \text{M. Eq. June 0.}$$
$$i \quad 48 \quad 47 \quad 58{\cdot}8$$

Log. q 9·6036440.

Retrograde.

"The comet will pass the descending node on the evening of July 27, distant only 0·0617 (earth's mean distance = 1) from our path. We shall, however, be far from the comet."

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